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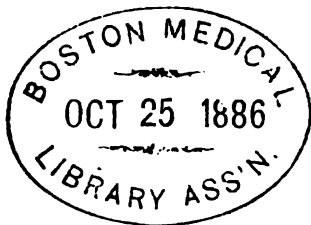


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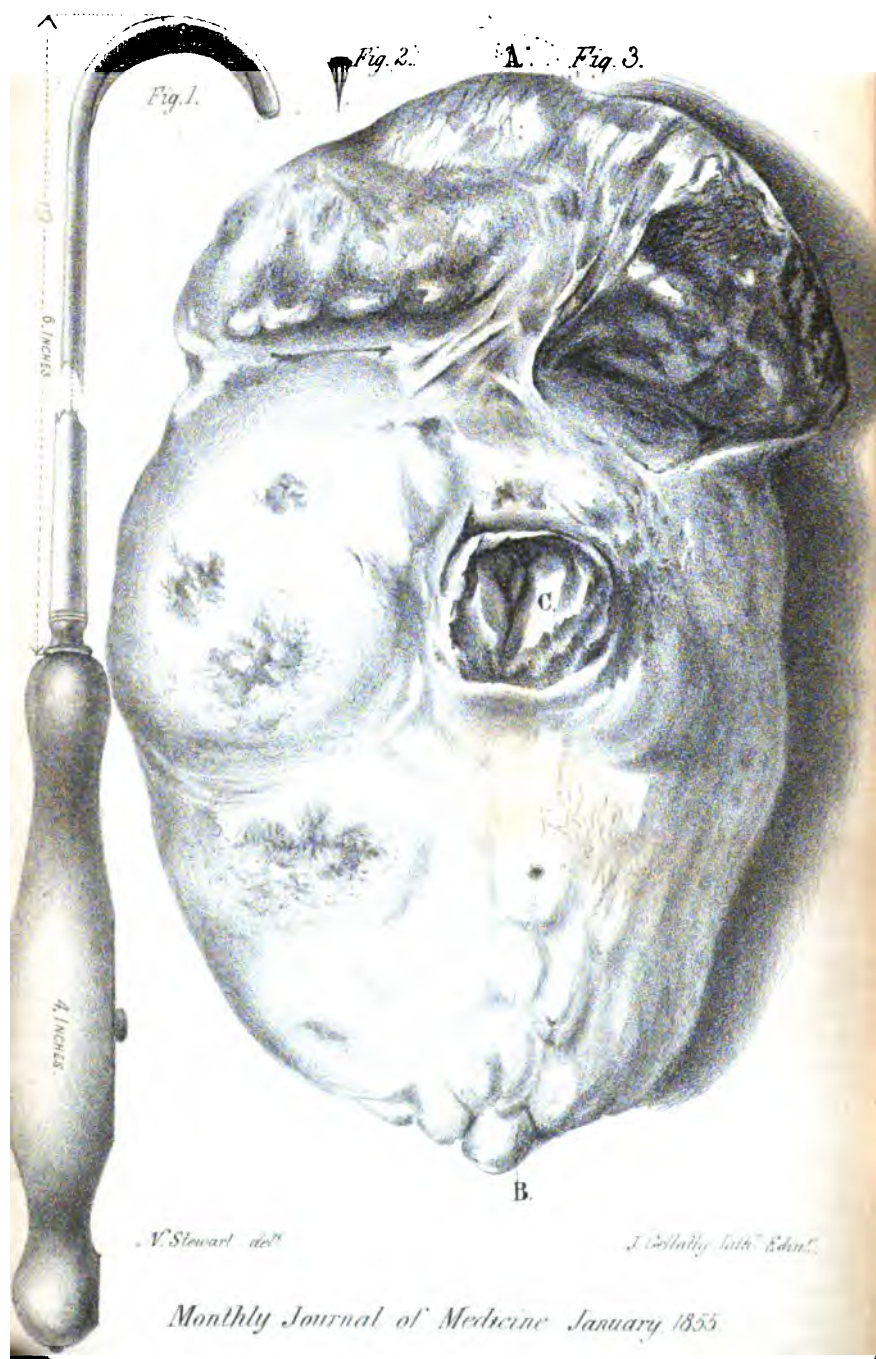
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ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Notes and Observations in Medical Practice.* By ROBERT CHRISTISON, M.D., V.P.R.S.E., Professor of Materia Medica in the University of Edinburgh. 1. *On Digitaline.* 2. *On Preserved Meat-Juice.*

1. *On Digitaline.*

So far as I am aware, no mention has yet been made in English works of any observations made in this country on the properties and uses of Digitaline, the active principle of foxglove. Nevertheless few substances would seem to deserve more attention. Digitalis is the best diuretic which medicine yet possesses; as every one will admit who studies its actions and the conditions for developing them. It is also thought by some to be one of the best sedatives of the circulation yet known; which, however, appears to be by no means so well ascertained. At the same time, all who use it have found reason to lament the uncertainty of its effects; a complaint, which, though sometimes unjust, because arising from insufficient acquaintance with the conditions for success, is clearly also often well founded, and dependent on the irregular strength of the galenical preparations of the plant, and the impossibility of trying their quality by any sensible properties or chemical test.

It is long since medical chemists were led by the great importance of foxglove to attempt to relieve medical practice from one source of uncertainty by endeavouring to discover its active proximate principle. At first these attempts were but partially successful. Its properties were concentrated at last in a pitchy extract of intense energy. But the very energy of this extract was only an additional objection to a drug whose form could not ensure uniformity of

strength, and left the really active principle in a condition liable to decay. The cause of difficulty in analysing foxglove was at length found to depend on the extreme instability of its active constituent under chemical operations. But after many years of fruitless search, MM. Homolle and Quevenne of Paris finally succeeded in detaching it in the form of a permanent, white, pulverulent Digitaline of neutral chemical relations.

Soon afterwards the same gentlemen proceeded to institute a searching inquiry into the actions of their newly discovered body. Their observations, as well as those of other French physicians, have been published from time to time in the useful little *Annuaire de Thérapeutique* of M. Bouchardat; and in January last their final results have been given in an excellent detailed treatise entitled, *Mémoire sur la Digitaline et la Digitale*. The researches of MM. Homolle and Quevenne clearly establish, what every scientific cultivator of therapeutics must long ago have confidently anticipated, from the analogy of other active plants which had been successfully analysed,—that the poisonous and medicinal properties of foxglove reside entirely in a peculiar proximate principle. Considering the apparent disconnection of the two leading and widely-different properties of the plant, its diuretic action on the kidneys, and its sedative influence on the heart, it might very possibly have been found to contain two active constituents, one for each property; and such sub-division would have proved a very convenient and desirable discovery for medical practice. Unhappily, as hitherto appears, both actions are concentrated in one principle, though the plant has been found to yield several others.

About two years ago, having obtained favourable results from a trial with Digitaline of French manufacture—for at that time I could not learn that any was prepared in Britain—I requested Mr Morton of London to prepare some for an extended trial of its effects. This gentleman, with his accustomed accuracy, supplied me with two forms, as suggested and employed by MM. Homolle and Quevenne,—a brown extractiform substance and a pale greyish-white powder. When the remedy comes into general use it is to be hoped that the latter form will alone be sanctioned. But, in the meantime, no harm can come from using the less pure form when supplied from a known and trustworthy source, and given strictly according to the injunctions of the discoverers. I therefore have hitherto used the extractiform digitaline alone, but the same sample of it.

My experience of its effects has been highly satisfactory. I have used it expressly as a diuretic only; but its sedative virtues have also come incidentally under observation; and it is quite evident that digitaline is a most energetic remedy in both ways.

In the first two trials made with it, which were both of them in cases of extensive, protracted, obstinate œdema in connection with disease of the kidneys, diuresis commenced, in one towards the close

of the second day, and in the other a day later; in both the flow of urine was profuse; and in both the œdema entirely disappeared, but with the slowness not uncommonly observed in this form of dropsy.

Here I cannot help observing in passing, that a striking illustration was presented, on both these occasions, of the unsoundness of the objection which has been brought by many practitioners, since it was first propounded by Dr Osborne, against the use of diuretics in the renal forms of dropsy. I have taken every opportunity, in my clinical lectures, and in occasional publications, to protest equally against the unsoundness of the theory and the looseness of the facts, which have led to the prohibition of diuretics in renal dropsy, and which have deprived many persons of the most immediate and most efficacious means of relief from the principal secondary affection occurring in Bright's disease of the kidneys. And I may here repeat, in the strongest terms, that I have never, except in a single instance, seen the albuminosity of the urine, or any other indications,—which either the other conditions of the urine or any other local symptoms can furnish,—increased under the employment of such diuretics as I have given in this disease. These are digitalis, squill, and bitartrate of potash, sometimes singly, more commonly combined. What may be the case with other diuretics, I shall not pretend to decide. I strongly suspect that they are all in the same category, and that they have been shunned on grounds purely theoretical and baseless. But at any rate I can answer for the theory not holding in the case of the three diuretics I have mentioned. So, too, in the case of digitaline. It is not a little remarkable that in the first two patients to whom I administered it, the albumen, instead of being increased, was quickly and greatly diminished. In one it disappeared entirely in a few days, and did not return so long as the patient remained under observation in convalescence. In the other it also disappeared; but after some days the albumen reappeared, though in a greatly diminished proportion. Digitaline, therefore, while it stimulates the kidneys to increased secretion, has not, in so doing, any effect, as some have thought of digitalis and diuretics at large, in increasing the peculiar renal irritation which constitutes or occasions Bright's disease.

Digitaline has proved equally, or even more serviceable in dropsy connected with disease of the heart. It has, in my hands, accomplished complete discharge of the dropsical effusions, and thus effected such relief as to enable the patient to return to his occupations, though given in circumstances apparently very desperate. In this respect digitaline has done no more than is often accomplished by foxglove itself. But it has appeared to me to act with more speed, and with greater force after the action did begin. In one instance great depression of the heart's action was brought on instead of a flow of urine.

I have also given it in some local dropsies, more especially ascites, either simple or combined with anasarca of the lower part of the

body only. It has not proved more useful, however, as a diuretic in these cases than digitalis and other diuretics; which in general fail to influence the urine or remove the dropsical effusion, when given internally. I have not yet tried it externally, according to the method recommended by me with infusion of foxglove.¹

There can be no doubt that digitaline possesses the action of foxglove itself upon the heart and circulation. I have not made express trial of this action. But while using it to excite diuresis in a case of dropsy with diseased heart, the pulse began to flag, soon fell to 44, and became very soft and somewhat irregular. At the same time languor, nausea in a slight degree, and faintness ensued. All these symptoms disappeared in a few days after the remedy was discontinued. On this occasion I observed that the two actions of digitaline, its diuretic and sedative actions, do not concur. It is very likely that they are even incompatible. This I have formerly stated to be a probable fact in regard to the actions of foxglove.² The disregard of it is, I apprehend, the reason why some underrate the efficacy of foxglove as a diuretic. Attention should therefore be paid to the fact in using digitaline.

The dose to be administered is stated by MM. Homolle and Quevenne at a seventy-fifth part of a grain of digitaline, three times a day. I have never given any other dose. It is a very small one certainly. But a tenth of a grain will kill a little dog; so that the dose is only in proportion to its exceeding energy. The form I have used is that of a pill about a grain in weight. It is to be given with the conditions and precautions observed in using foxglove.

Digitaline may be extracted from any part of the *Digitalis purpurea*; and it has also been obtained from the *D. lutea*. The leaves of the former yield as much as any other part of the plant, even as the seeds, and more conveniently. The dry powder is first saturated with cold water by the process of displacement. The di-acetate of lead is then added, to throw down much inert matter. The excess of lead is removed by a mixture of carbonate and phosphate of soda. Lime is next thrown down by oxalate of ammonia. Digitaline in an impure state is now precipitated by tannin, collected, and dried with a gentle heat, after being mixed with litharge, to detach the tannin. The dry powder is then made to yield a dry alcoholic extract by means of alcohol of the density of 836. From this extract, impurities are washed away with a little highly-concentrated sulphuric ether. Digitaline alone remains.

The process is troublesome, but will probably be simplified. The product is consequently expensive. But after all a cure with it is not costly, by reason of its extreme energy. It is of good quality, if a solution in 200 parts of alcohol do not lose its bitterness until

¹ Edin. Monthly Journal of Med. Science, 1850, xi. p. 310.

² Dispensatory, 1842, p. 401.

so diluted with water that the digitaline forms only a 200,000th of the solution.

Digitaline of this degree of purity constitutes pale yellowish-white scales, easily pulverizable, intensely bitter, intensely irritating to the nostrils, and permanent in the air. It fuses about 212° , and undergoes decomposition at a temperature somewhat higher. It is neutral, and destitute of azote; soluble in 2000 parts of water, and in 100 parts of pure sulphuric ether; very soluble in alcohol, and still more so in chloroform. It cannot be crystallised or combined with acids. Strong hydrochloric acid forms with it a beautiful grass-green solution. Potash added to its watery solution destroys its bitterness and substitutes astringency. The alkaline carbonates and caustic ammonia have the same effect, but act more feebly. Tannin throws it down from its solution in water.

Foxglove yields by analysis three other neutral proximate principles, which MM. Homolle and Quevenne have called digitalose, digitalin, and digitalide; but it may be surmised that these are the results of chemical reactions, and not true educts. There seem to be also three vegetable acids, called digitalic, antiirrhinic, and digitoëic acids.

Bouchardat and *Sandras* found that the seventh of a grain of digitaline, injected into the jugular vein of a dog, soon caused frequent vomiting, staggering, diminution of the pulse from 120 to 36, giddiness, and death in four hours and a half. A grain and a half caused death in 90 seconds. Three-fourths of a grain secured in the stomach caused violent efforts to vomit, extreme exhaustion, and death in three hours. In the dose of a twelfth of a grain they found that in man it occasions reduction of the pulse to one-half or two-thirds of its natural frequency, with disturbance of the senses, confusion, and fatiguing dreams, but only in one instance an increased flow of urine. *Stannius* ascertained that, in animals killed by digitaline, the heart, immediately after death, is motionless, and not excitable by galvanism, or any mechanical stimulus; while the voluntary muscles and intestinal muscles continue irritable as usual.

MM. Homolle and Quevenne have given in their treatise many cases of their own and of others, on the internal action and uses of digitaline. The general result is, that, in the dose of a 75th of a grain, given three times a day, it usually acts as a diuretic in general dropsies, and with great speed and efficacy in reducing the effusion; that this action on the kidneys is not so certain where there is no dropsical effusion, but nevertheless may be often brought on in other circumstances also; and that it is not rendered more certain by any material increase of the dose. The next result is, that in about double the dose, and sometimes in the same dose, it reduces greatly the frequency of the heart's action, arresting at the same time irregular action of functional origin, and even sometimes that connected with organic disease of the heart, and relieving palpitation. Lastly, the dose cannot reach the twelfth of a grain without

nausea and other incipient symptoms of poisoning showing themselves.

Many curious remarks of authors on foxglove, which will repay perusal, are brought to light in their valuable summary of prior observations on the employment of the galenical preparations of the plant. Among these, none is more remarkable than the following statement of *Professor Cruveilhier*:—"Alibert has formally denied the curative properties of foxglove in dropsies; and the judgment pronounced by this physician appears to have been generally adopted without appeal. This decision, however, is too absolute," etc. How are we to account for this marvellous error, discreditable to medicine—discreditable above all to an author on *materia medica*? The general sense of physicians in Britain has long since assigned to foxglove the first place among diuretics in the treatment of general dropsies; and for myself, I can confidently say that I have very seldom indeed obtained success in such dropsies from other diuretics, when foxglove failed. The probability is, that Alibert never found out how to use this singular remedy. It is plain that various authors and experimentalists quoted by MM. Homolle and Quevenne have also not known how to give it as a diuretic; for they have evidently aimed, in using both foxglove and digitaline, at exciting its action on the heart. But several others among them have gone very near to the enunciation of the proposition that diuresis is not produced in such circumstances. MM. *Andral* and *Lemaistre*, for example, have stated in their account of some trials with digitaline, that, "in every case, the diuretic action was invariably consecutive to the influence on the heart." By this I understand that it did not take place until the influence on the heart passed off. At all events the prior establishment of the action on the heart is most assuredly not a condition for inducing diuresis, but on the contrary, in my own observation at least, positively interferes with it, must pass off before diuresis occurs, and may prevent it altogether. In 1832 I taught, in my first lectures on *materia medica*, that the two actions are probably incompatible; and I have repeated this opinion annually since then. I have seen no reason at any time to doubt the fact; which first occurred to me when a pupil at St Bartholomew's Hospital in London in 1821, on observing some remarkable cases treated by the late Dr Powell. The question is evidently a very important one to settle, both on account of its pure practical bearings, and likewise because, if settled as I anticipate it will be, it will explain the strange contrariety of opinion among reputable authors as to the merits of foxglove, and thus relieve medicine in this point from what may well be urged against it at present as a reproach.

2. On Preserved Meat-Juice.

About eighteen months ago, when consulted in the case of a relative of Mr Gillon, the extensive and skilful manufacturer of pre-

served meats at Leith, I found that the patient was entirely supported, in a severe illness, by the Preserved Juice of Meat, which had been given at Mr Gillon's suggestion. Observing the readiness with which it was taken when other food of every kind was refused, I was induced to try it in other instances, and eventually to employ it in various states of disease. The results led me to suggest the use of it to many professional friends, and to advise the druggists of Edinburgh to keep it; so that it is now much in request, and may be easily obtained.

This substance is the pure juice of beef, preserved in the way in which meats and vegetables are now so extensively preserved in the fresh state, for store provisions. The mode of preparation is as follows: Cylindrical cases of tinned iron are filled each with six pounds and a half of beef; and the lid is soldered on, but with a hole about half an inch in diameter in the middle of it. Two trays of such cases are shoved into iron retorts, analogous in form to retorts for gas-making, but double-cased, so that steam may be introduced into the interstice around. They are thus subjected to a heat of 220° under steam pressure, for about three hours; by which the beef is partially cooked, and, being thus also made to contract strongly on itself, squeezes out a portion of its juice, amounting to a few ounces from each tin. The tins are then drawn, the juice is poured out, and the meat, with certain additions, is subjected to the preservative process. The juice, after being cooled and entirely freed of fat, is put into small four-ounce tin cases. Each of these has a small aperture at one end, which is secured with solder, after the juice is poured in. The tins are then subjected, on trays, to a temperature of 220° in a muriate of lime bath. On being removed, the solderer rapidly touches with his iron the solder on the top, which giving way allows steam to rush out forcibly and carry with it the air in the upper part of the interior. By the time he has thus swiftly passed over sixteen or twenty tins, the first is ready for being re-soldered by a similar dexterous application of his iron, which then in succession as quickly secures the whole open and steaming apertures. The process of heating in the bath, tapping, and re-soldering, is then repeated a second time, to make sure of the thorough expulsion of every particle of air. The tins finally are painted to preserve them against rust.

The process is most perfect. I have repeatedly opened tins eighteen months in my possession, and stated to have been many months in store when I got them, and in every instance the contents had the rich delicate aroma and taste of fresh beef-juice. Sometimes the taste is slightly resinous or soapy, in consequence of a little resin having obtained admission in the operation of soldering. But as this does not occur often, the impurity may be avoided with due care. The juice may be taken with relish in small quantity, either cold or warm, in its concentrated shape; but it is rather strong to be

used without dilution. When diluted with three times its volume of boiling water, and duly seasoned with salt and pepper, it makes a more palatable beef-tea than any which can be made in the usual way. Sometimes, indeed, a patient will be found to prefer the ordinary sort, either because the preserved juice has unluckily been resinous, or on the principle that leads some people from the plains of England to prefer hard water to the pure mountain springs of the primitive districts of Scotland, viz., because they are not accustomed to the finer sort. But this is not the general fact; and there can be no doubt that the preserved meat-juice makes a most palatable beef-tea, and an equally eligible basis for many soups.

Until about ten years ago, in concurrence with general opinion, I used to regard beef-tea as a highly nutritive article, not to be rashly or freely given during disease. My sentiments in this respect were shaken, when I ascertained, in the course of some experiments for adjusting the dietaries of the General Prison and the Royal Infirmary, that a pint of the very finest beef-tea contained scarcely a quarter of an ounce of any thing but water. Since that time I have much more readily listened to the cravings of patients for beef-tea in even many acute diseases, and above all in protracted subacute diseases, and in chronic diseases with fever; and I have thought I saw that it maintains the strength almost like wine, lessens emaciation and weakness in tedious diseases, and does not occasion any increase of reaction. There is no disease in which these properties are more remarkably shown than in protracted cases of gastric fever, of which, by the way, I have seen an unusual number both in town and country during the last three years. These cases have often lasted for six weeks, or,—with a relapse, from too early indulgence or exposure,—for the long term of three months nearly; during which little, or absolutely nothing else, was taken, except beef-tea or diluted meat-juice; and without the attenuation and debility which so protracted a fever and want of appetite ought to have induced. In some instances I could scarcely doubt that life was preserved by this nutriment. It is unnecessary to particularise the various states of disease in which the same practice has been followed. It is peculiarly applicable to all subacute protracted diseases, whether febrile or otherwise; and in all such there is even no great reason to hesitate in resorting to it when local inflammation is present. Every one, I think, will be struck with the readiness with which such patients will often take diluted meat-juice or beef-tea repeatedly when they refuse all other kinds of food. It should be given in the quantity of a teacupful at a time, every four or six hours; but it is well to alternate it with other simple nourishment, when the patient will consent to do so.

What is its mode of action? Not simply nutrient. A quarter of an ounce of the most nutritive material cannot nearly replace the daily wear and tear of the tissues in any circumstances. Possibly it belongs to a new denomination of remedies, whose action never

was even suspected to exist until recently—those which, by some peculiar influence, diminish the waste of the tissues under the exercise of their functions. *Professor Lehmann* has proved [*Annalen der Chemie*, 1853] that coffee possesses this singular property in so remarkable a degree, that in persons following an active occupation an infusion of an ounce of roasted coffee daily will reduce the daily waste by a fourth part; and the same property seems likewise to belong to tea, and other restorative beverages. It is not improbable that the sapid and saline principles of meat, united in what is called ozmazome, and constituting the ingredients of beef-tea and meat-juice, possess some such property. It is difficult otherwise to account for the interesting results obtained by the late *Dr Edwards* in 1833, who, in his researches on nutrition,—strangely overlooked by the celebrated Gelatin Commission of the French Institute, in their condemnatory report on gelatin in 1841,¹—found that dogs die slowly if fed on bread and gelatin alone, but, when thus greatly reduced, quickly regain flesh and strength by the addition of two ounces of meat-tea, which cannot appreciably increase their textures by its own insignificant amount of solids.² Either it acts as a digestive ferment, so to speak—promoting the assimilation of other nutriment—or, like coffee, it must lessen the waste of the tissues in the exercise of their functions.

Mr Gillon's meat-juice contains only $6\frac{1}{2}$ per cent. of solids. As a mere nutrient, therefore, it is much in the same category with beef-tea. Sixteen ounces of beef-tea, made with the contents of one tin, yield only 114 grains of solid extract. It contains no fibrin, no albumen, no gelatin. It does not even gelatinise, on exposure to the air for days: it is ozmazome, with the salts and sapid and odorous principles of meat, and materially different from all boiled extracts.

I should add, that no good beef-tea can be made so cheap as with this preserved meat-juice. A tin of four ounces makes sixteen of strong beef-tea. This much requires, in the ordinary way, a pound of the finest beef, which at present costs ninepence, and is scarcely ever so cheap as sixpence. The reason for the cheapness of Mr Gillon's meat-juice is, that the residual meat is economised, while that of the ordinary cooking process is good for nothing.

It is a much more convenient article for use than any of the extracts made from meat by extemporaneous processes in the kitchen, or by certain very dubious chemical methods lately come into vogue. It differs materially from all meat-extracts prepared by boiling.

¹ *Comptes Rendues*, etc. 1841. xiii. 243.

² *Archives Gén. de Méd.*, N. S. i. 313.

ARTICLE II.—*Contributions to Obstetric Pathology and Practice.*

By J. Y. SIMPSON, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh, etc., etc.

NO. XIV.—ON THE EXCISION OF LARGE PEDUNCULATED UTERINE POLYPI; AND ITS ADVANTAGES OVER DELIGATION.

A VARIETY of operative means have been proposed and practised for the removal of uterine polypi, after they have once passed the os uteri and come within the reach of surgical interference. The means usually followed differ according to the size and consistence of the polypus.

In the removal of small and soft uterine polypi of the mucous or cellular type, practitioners employ—1, simple avulsion with proper forceps, or with the fingers; or 2, avulsion by torsion; or 3, compression or crushing of the neck or body of the polypoid growth; or 4, scooping out its attachment with the nail, if the cervix is opened, or with the scraping or gouging instruments invented by Recamier and Dr Locock; and 5, the introduction and application of a stick of *potassa fusa* inside the os, followed by the free application of vinegar, is sometimes most advantageously had recourse to where, as is often seen in practice, several small, sessile or pediculated polypi—formed by dilatation of the Nabothian follicles—are attached to the lips of the os and along the interior of the cavity of the cervix uteri. In the present communication, however, I wish only to speak of the larger and firmer forms of pediculated uterine polypi. These are usually fibroid or cellulo-fibroid in their structure, and vary from the size of a small walnut to that of a newborn child's head. In the removal of them one or other of the following operations is generally had recourse to; viz., either,

- I. The slow and gradual division of the pedicle of the polypus by *Deligation*, or by the constriction of ligatures of silk, catgut, silver wire, etc., applied by means of various forms of canulæ; or
- II. The instantaneous division of the pedicle by *Excision* by the scissors, scalpel, or bistoury, and the consequent immediate removal of the amputated polypus.

Some practitioners combine together these two methods—applying deligation first, and then—either immediately or some days subsequently—using excision or resection in addition.

In this country the operation by deligation alone has been the plan usually adopted. "In England (as Dr Ashwell states), the ligature has *always* had a decided preference."¹ "British practi-

¹ Diseases Peculiar to Women, p. 485.

tioners have (says Dr Hamilton) *universally* agreed that the safe mode of operating is by ligature."¹ "I shall (observes Dr Rambotham) consider this as a point *settled*, at least in our Island."²

The removal, however, of large uterine polypi by excision, has long appeared to me to be in many respects a simpler and a safer operation than their removal by ligature. It is, on the whole, more easily accomplished; the cure of the disease by it is infinitely quicker; it is accomplished with far less restraint and annoyance to the patient; with less risk of local irritation; and, as I believe, with less ultimate chance of actual peril to health and life.

Two objections have usually been urged by accoucheurs against the removal of large uterine polypi by excision. The first of these objections, viz., the danger of hæmorrhage, has been particularly insisted on by those practitioners who have written in favour of deligation. But the fact is, that excess of hæmorrhage is not common after division of the peduncle of the polypus; and however great that or any other traumatic hæmorrhage from the impregnated uterus may be, it can always be arrested by properly filling the vagina for a few hours with pieces of sponge or other appropriate plug. Dupuytren, though using no plug or other means to prevent bleeding, only met with two cases of severe hæmorrhage out of some 200 instances in which he removed uterine polypi by excision; and Lisfranc only observed it twice out of 165 similar operations practised by himself.³ I believe, however, from what I have myself seen, that the practitioner, in employing excision, must expect considerable hæmorrhage in a proportion of cases much greater than this; but at the same time he can, I repeat, always readily *arrest* it, when it does occur, with proper plugging; and it is perhaps best—as a general rule—to *prevent* and forestall its occurrence by introducing, for ten or twelve hours after every case of resection, a proper tampon of fine sponge into the vagina. Let me add, that it is well to have each piece of sponge which is used previously transfixed by a strong thread, the end of which should be left out of the vagina, in order to permit of the more easy withdrawal of the plug.

The second objection usually urged against excision is of greater weight, viz., that more or less injury is always liable to occur to the pelvic attachments of the uterus, if it and the polypus are forcibly dragged down by Museux's forceps, or by hooked vulsella, so as to bring the peduncle of the tumour into view before dividing it,⁴ a plan followed by most operators;⁵ or, on the other hand, if the

¹ Practical Observations on Midwifery, p. 40.

² Medical Gazette for 1835, p. 433.

³ Lisfranc's Clinique Chirurgicale, vol. iii., p. 210.

⁴ Dr Heming's Translation of Mad. Boivin's Treatise on Diseases of the Uterus, pp. 210, 211.

⁵ When the texture of the polypus is too soft to allow, without tearing, of the mass being drawn downwards by vulsella, Lisfranc even advises the hooks of the forceps to be fixed in the cervix uteri itself, to get a sufficient

peduncle is divided while the polypus and uterus are *in situ*, the scissors or knife employed, are, it is averred, apt to injure and cut the vaginal walls and neighbouring tissues, while worked within the vaginal cavity.¹

Of late years, I have used a means of excising large uterine polypi, that seems to me to obviate entirely this last class of objections, and by which, as I have been led to believe, the whole operation is much simplified, and rendered both greatly more easy to the practitioner, and more safe to the patient. By the means or instrument in question, the peduncle of the polypus is divided *in situ*, and without any chance of its cutting portion injuring the structures of the vagina or vulva.

The instrument, or polypus-knife, to which I refer, is sketched in Plate I., fig. 1. It is of the form of the usual midwifery hook; with the concavity, however, of the hook not blunt, but turned into a cutting surface by the insertion of a small piece of well tempered steel blade into it. A transverse section of the curved or cutting portion of the instrument, and of its included knife-blade, is shown in the small figure, No. 2. The entire length of the instrument which I have hitherto employed in my own practice is ten inches—the length of the wooden handle being four inches, and that of the metallic shaft six inches. A shorter instrument might, perhaps, suffice equally well. The plate represents the curved portion, or hook at the extremity, as somewhat wider and larger than in the polyp-tome made for me in the first instance, and which I have generally employed in practice. Perhaps an increased or a diminished size and width, in the curved hook at the extremity of the instrument, would render the operation of division by it more easy, according as the stalk of the polypus was very thick, or comparatively slender. The extreme point of the instrument is blunt and rounded; and the cutting portion or blade is so protected and concealed by it, and by the back wall of the curve, that it can be introduced into and withdrawn from the vagina, without any chance of its edge injuring or dividing the vaginal structures themselves. To be always able to discover the direction to which its hooked extremity points after it is introduced into the vagina, the front aspect of the handle is distinguished by having a slight knob or other mark upon it.

In employing this polyp-tome, the stalk of the polypus is first to be reached by the apex of the first finger of the right hand, introduced along the short anterior or pubic surface of the vagina; the instrument is then pushed by the left hand along this finger as a guide, and passed over or above the peduncle of the tumour, in such a

hold for dragging down it, and the polypus with it, to the vulva.—*Gazette Medicale*, 1834, p. 149.

¹ *Quia secantia instrumenta aegre in vaginam immitti possunt, ne partes vicinas laedent, et facile lethales hæmorrhagias gignant.*—*Nissen De Polypis Uteri*, 1789, p. 34.

direction that the concavity of the hook will come down upon and embrace this peduncle, as the instrument is pulled again downwards. The next step is to make the blade of the polypus-knife cut through the stalk of the tumour. For this purpose, a little simple traction, with a slight rolling or sawing motion, is all that is generally required. If the tissue of the peduncle is dense and strong, the dividing force of the instrument may be increased by the forefinger of one hand being applied with a tractive power to the blunt extremity of the instrument, while the handle is dragged down and moved in a sawing direction, by the other hand of the operator. Sometimes when the polypus is round and loose, after the curve or hook is applied to its pedicle, the cutting portion of the polypome will divide this stalk most readily, by merely doubling backwards with the fingers the body of the polypus upon its own stalk, and pulling the knife against the bent peduncle. In such a case, the peduncle is divided as much by pressing it against the knife, as by pulling the knife through the peduncle.

During the last few years, I have removed a very considerable number of uterine polypi of different sizes, and some of them of large dimensions, with this curved polypome; and I can now speak from somewhat extensive experience of the perfect facility, and safety of its employment.

Sometimes soft and slender cellular and canaliculated polypi, usually of an elongated form, are met with in practice, which afford no sufficient resistance for a knife to divide them or their peduncles. In such cases, the peduncle of the polypus is, perhaps, most easily severed, by the careful clip of a pair of blunt-pointed, curved scissors. In all other forms of the disease, where the tumour was large and pedunculated, I have of late employed the polypome. In using it, the patient is placed in bed, in the common position on the left side; and generally the whole operation is accomplished so readily and easily, that she is often not aware that more than a common digital examination has been made. I have several times seen some difficulty attend the removal of the amputated polypus itself from the vagina, after its stalk was divided, in consequence of the great size of the tumour; and in order to effect extraction, I have occasionally been obliged to transfix it with the teeth of a large vulsellum. But hitherto I have met with no special difficulty, in at once and easily dividing the peduncles themselves of the polypi, with this polypus-knife.

The whole operation is simple and safe, expeditious and painless, and approaches, perhaps, more than any other in practice, to the Asclepadian character of the object of the physician: "*ut tuto, ut celeriter, ut jucunde curet.*"

In conclusion, let us compare and contrast, in a few points, the operation of the excision of a common or large sized uterine polypus, by the method I have described, and its removal by the ordinary operation of deligation.

I. *Relative difficulty of the Excision and Deligation of Uterine Polypi.*

No practitioner can ever be perfectly certain that any large growth detected in the vagina, is a uterine polypus, until his finger touches and traces the peduncle itself of the tumour. And wherever the finger can thus be made to pass and detect the stalk of the polypus, the polypotome may certainly be guided to, and applied so as to divide that stalk. I refer here to cases of considerable difficulty, from the unusual shape or size of the polypus. In such instances, one cannot but conceive it easier to pass upwards a solid curved instrument directly around the mere stalk of the tumour, than to pass a piece of whipcord or other ligature behind and over the whole body and mass of the polypus itself, till, in being retracted, it comes indirectly and ultimately to embrace the stalk. For example; in Plate I. fig. 3, there is a sketch of a large uterine polypus, which I sometime ago amputated with the polypotome. The plate represents the polypus of the natural size—A marking the upper, and B the inferior extremity of the polypus, while C denotes the site and thickness of the peduncle of the tumour, as divided by the polypotome. In this instance, the polypus is of an elongated form, its peduncle being attached to its middle, and not to its upper extremity. The tumour had evidently grown into this form after being expelled from the uterus into the vagina. It had developed upwards towards the roof of the vagina, as much, or more than downwards. In this case, the peduncle of the tumour was readily caught and divided by the polypotome; but it would evidently have been a matter of great difficulty to have passed a ligature over the back and top of such a polypus, so as to embrace with it the peduncle from above.

When, however, a polypus is smaller, round or oblong, and its peduncle is attached to its upper part, there is not more difficulty in applying the ligature, than in applying the knife to the stalk of it. Some, however, of the practitioners who have had most experience with the ligature, confess to the occasional difficulty of its application, with even the best kind of canula. "By practice and dexterity," says Dr Burns, "this instrument (the double fixed canula) may doubtless be adequate to the object in view, but without these requisites, the operator will be foiled—the ligature twisting, or going past the tumour; every attempt giving much uneasiness to the patient, and not unfrequently, after many trials and much irritation, the patient is left exhausted with fatigue, vexation, and loss of blood. This is very likely to happen if the polypus be so large as to fill the vagina. Dr Hunter," adds Dr Burns, "after repeated trials failed in a case, where the polypus filled the vagina; the pedicle in the preparation is long, and as thick as the finger." The application of a ligature

¹ Burns' Principles of Midwifery, p. 130.

to a large uterine polypus is, "in many cases (Dr Hamilton¹ testifies) one of the most difficult and dangerous operations in surgery;" and he tells us that "he has seen some of the most eminent practical surgeons of this part of the kingdom foiled in their endeavours to apply the ligature."

I quote, in preference, such opinions from the writings of Professors Burns and Hamilton, because both of these gentlemen were strongly in favour of the operation of deligation.

II.—*Relative Duration of the Operation of Deligation and Excision.*

The process of excision is generally accomplished in the course of two or three minutes at most; sometimes in a shorter period. On the contrary, the deligation of a uterine polypus consists of a succession of operations rather than of one; and is usually protracted through a period, varying from two or three days to two or three weeks. The application itself of the ligature and canula, in the first instance, requires as much, or indeed more, time and pains than the act of excision. But, after its first application, the ligature requires to be tightened and adjusted from time to time. "Twice a day (as Dr Gooch directs), the ligature is to be untwisted from the shoulder of the canula, drawn tighter, and then fixed again round the projecting part; and this is to be done morning and night." "Every day," (observes Sir Charles Clarke, another advocate, like Dr Gooch, for this mode of treatment)—"Every day the practitioner is to examine the state of the ligature, and as often as it is found to be at all slack, it is to be tightened. The mode of tightening it," he continues, "requires particular attention. If the canula should happen to be long, the practitioner should not hold the end of it whilst he tightens the ligature, lest with the force used the ligature should cut through the neck of the tumour, and the other extremity of the canula should be suddenly and forcibly pushed against the internal parts of the woman. The time," he adds, "at which the ligature will come away will depend upon the thickness and firmness of the neck of the tumour, and the tightness with which the ligature is at first applied. The neck of the tumour sometimes is cut through in four days, sometimes ten or twelve days will elapse between the application of the ligature and the removal of the tumour, and occasionally the separation of the tumour will take up three weeks; but this is an uncommon occurrence."² "After an interval," observes Dr Churchill, "varying from six days to three weeks, the canula will be found loose in the vagina, and the stalk of the polypus severed."³

III.—*Relative Care and Management after the two Operations.*

After the operation of excision, the only special treatment in

¹ Hamilton's Practical Observations, p. 40.

² Observations on the Diseases of Females, page 263.

³ On the Diseases of Females, p. 220.

general required is the introduction of a sufficient plug, of sponge or other soft material, into the vagina, to prevent the chance of bleeding; and the withdrawal of this plug after ten or twenty hours. After, however, the application of the ligature in deligation of a uterine polypus, a considerable amount of continuous care and caution is necessary up to the time at which the pedicle is ultimately divided. "The patient is," says Sir Charles Clarke, "to be desired to remain constantly upon her side, and should not be allowed to move from one side to another unless when the practitioner is present. For want of attention to this caution, there is," he adds, "reason to believe that the canula has been inadvertently pressed against, and its extremity pushed through the uterus of the patient, so as to occasion her death."—(p. 262.) "The woman," as Dr Ramsbotham states, "will be obliged to keep her bed during the sloughing process; and she ought to be cautioned, upon attending to her natural calls, to beware of any accidental occurrence which might push the point of the instruments against the internal surface of the uterus."¹ "As the instrument," Dr Gooch remarks, "projects out of the vagina, if the patient was, whilst turning from side to side, to sit down upon it, she might impale herself on it,—an accident which, I have heard, once took place, and terminated fatally."—(p. 264.)

IV.—*Relative chance of Local Irritation of the Vagina and Uterus.*

Local lesion and irritation of the vagina and cervix uteri are not liable to follow upon the practice of excision, unless some local injury has resulted in the operation from very incautious manipulation. But in addition to this danger, there is after deligation, other sources of local disease in the sloughing and putrefaction of the polypus before its complete separation; in the presence of the very foetid and excoriating fluid with which the surface of the vagina and vulva is in consequence constantly bathed; and in the irritation by the ligature itself, as a foreign body, upon the constricted and ulcerating stalk of the tumour,—not to speak of the constant application to this ulcerated surface of the foul and acrid discharges that issue from the dead and decomposing polypoid structure. The polypus usually swells after the first application of the ligature. "On account," observes Chelius, "of the increasing bulk of the polyp, it is generally necessary for the first few days (after deligation) to empty the bladder with the catheter, and the rectum by clysters. The symptoms," he further states, "which may occur after the tie has been made are, violent inflammation and fever, pain, spasm, bleeding, and other symptoms, from the pressure of the swelling polyp. To prevent," he adds, "the effect of the stinking ichor, repeated injections of decoctions of aromatic herbs must be employed."² After the ligature is applied, "When putrefaction

¹ Dr John Ramsbotham's Practical Observations, vol. ii. p. 468.

² System of Surgery, South's Edition, vol. ii., p. 752.

has commenced, the discharge from the vagina (to quote the words of Dr Ramsbotham) becomes fœtid and *highly* offensive. . . It is, indeed, the best sign we can observe, as it proves that decay is going on, that the stem is sufficiently compressed to strangulate the vessels which nourished the diseased growth. If ever (he adds) such a discharge did *not* take place in a day or two, I should be suspicious that the operation would not succeed.”¹

V.—*Relative Danger of the two Operations to the Health and Life of the Patient.*

Those authors who have written in favour of deligation usually quote one solitary case of death from hæmorrhage after excision, recorded by Zacutus Luzitanus, in the 17th century. It was an instance of the fact that the amount of attendant hæmorrhage is not regulated by the mere size of the polypus; for in the case in question it is stated that the amputated polypus was not larger than an almond.² In this instance the operation was performed by an empiric, and no plug or other means for arresting the hæmorrhage appear to have been employed. The patient died, not so much from the operation, as from neglect of all proper means to restrain the hæmorrhage resulting from it. At the same time let me remark, in passing, that the operation of deligation itself is not free from the risk of hæmorrhage, both from the abrasion of the surface of the tumour in working with the canula and ligature, and from the division of the vessels of the stalk, as they are cut through in the process of deligation. “I think,” maintains Dr Meigs of Philadelphia, “the ligature is to be preferred to all other modes of extirpation. It is not in every case to be effected without hæmorrhage. I know (he adds) of two cases here in which the hæmorrhage was *terrible*.”³

But the principal danger to health and life in this, as after other surgical operations, is the danger of phlebitis and surgical fever. Is such a consequence more liable to follow upon the instantaneous resection of the peduncle of a polypus, and the subsequent immediate removal of the amputated polypus itself,—or is it more likely to supervene upon the slow process of disjunctive ulceration being set up in the stalk of the polypus by the ligature, while the gangrenous and putrifying polypus itself, is left decomposing in the cavity of the vagina?

I believe that no physician or surgeon acquainted with modern pathology will have any difficulty in answering, that the danger of

¹ London Medical Gazette for 1835, p. 435.

² Praxis Medica, lib. ii., Obs. 86.

³ Meigs' Females and their Diseases, p. 255. See also Examples in Colombat de l'Isère's Traité des Maladies des Femmes, p. 817.

phlebitis is much greater under the latter circumstances than under the former. The recorded experience of some of those who have written in favour of the ligature, shows strongly enough the occasional liability under deligation to the occurrence of irritative fever and internal inflammations, from phlebitis and the absorption of putrid and purulent matter from the vagina. Dr Hamilton¹ mentions three cases of death which he had seen follow the removal of uterine polypus by ligature. "On a close inquiry," observes Mr Arnott, "I find that even those who use it (the ligature) acknowledge that occasionally cases have been met with, where the ligature in cutting its way through, has excited irritation and fever, and even death. Two cases have been described to me by the practitioners concerned where this occurred, and in casually referring to the interesting works of Boivin and Duges I find two similar ones."² In his lectures on fibrous tumours of the uterus, Dupuytren³ states, "I possess eight or ten observations of women who have perished, from veritable poisoning and absorption of pus, after the application of the ligature for uterine polypus."

I have myself seen a woman die with a ligature still fixed around the partially divided neck of a uterine polypus; and other cases where severe but not fatal attacks of phlegmasia dolens and phlebitis followed deligation. Twelve or thirteen years ago, on a patient of Dr Edgar's of Berwick, I applied a silver wire ligature to the neck of a large polypus, and tightened it from time to time, according to the usual rules. In the course of a few days the polypus was dead and putrifying; there was much heat and irritation in the vagina; and the patient's pulse became rapid under the irritative fever that followed. On strongly tightening the ligature to expedite as much as possible the total amputation of the polypus, the wire broke; and the canula and wire slipped off. I immediately proceeded to remove the polypus by excision instead of making any renewed attempt at deligation; and the result was to me very striking and satisfactory. Within twenty-four hours the local irritation had greatly subsided, and the constitutional disturbance entirely disappeared. From that time to this I have operated on many uterine polypi, but never again by the process of slow deligation. And the more that I have seen of the practice of removing large pediculated uterine polypi by excision, the more deeply has the conviction grown upon my mind, that this method is very superior to the usual method followed in this country, of the removal of them by the canula and ligature.

¹ Hamilton. Pract. Observ., p. 37.

² Arnott in Lond. Med. Gazette, 1836, p. 412. See also notices of two other cases of death from uterine phlebitis after deligation, in *Cyclopedia of Practical Medicine*, vol. iv. p. 393.

³ Lecons Orales. Brussels ed., 1826, p. 237.

ARTICLE III.—*On the Treatment of the more Common Forms of Skin Diseases met with in Edinburgh.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine, and of Clinical Medicine, in the University of Edinburgh.

SINCE the addition of a ward for skin diseases to the clinical department of the Royal Infirmary, I have had ample opportunities of determining what are the more common forms of cutaneous eruption met with in this city, and of trying various kinds of treatment. A short account of the results of my experience in this department, excluding the eruptive fevers, may not be unacceptable to my medical brethren.

Eczema is by far the most common disease met with, both in its acute and chronic forms. The local treatment I have found most efficacious is that which I recommended, in the August number of the Journal for 1849. It consists in keeping the affected part moist, with lint or linen saturated in a very weak alkaline solution, consisting of soda subcarb. ʒss. to a pint of water. For this purpose it is necessary to cover the moistened lint with oil silk, or gutta percha sheeting, which should well overlap the lint below, so as to prevent evaporation. The usual effect is soon to remove all local irritation, and especially the itching or smarting so distressing to the patient; to keep the surface clean, and prevent the accumulation of those scabs and crusts, which in themselves often tend to keep up the disease. After a time, even the indurated parts begin to soften, the margins of the eruption lose their fiery red colour, and merge into that of the healthy skin, and, finally, the whole surface assumes its normal character.

In private practice, it is often a matter of great difficulty to secure a proper application of the lotion. Individuals are slow to accept the idea that constant moisture of the part is absolutely necessary for the treatment, and hence vigilant superintendence and frequent visits are requisite, in order to watch the progress of the case. Even in the hospital constant watchfulness is necessary, to see that nurses properly cover the eruption; and when, as sometimes happens, this task is given to the patients themselves, it almost always fails. Then there are some portions of the surface which it is very difficult to keep moist and well covered, such as the face and axillæ. But, by carefully adapting lint and gutta percha sheeting, attaching strings to the edges of the latter, so as to keep the whole in its place, I have never failed in ultimately carrying out my object.

In addition to stating what I have found to be useful, it is important to say what I have, on careful trial, ascertained to be useless or injurious. Perhaps no remedy is more generally employed in this and a variety of other skin diseases than citrine ointment, an application that I have always found to irritate and make eczematous eruptions worse. At the same time, there are some very chronic forms of the disease, which I have been told are cured by this

preparation, but what these are I have never been able to ascertain. Indeed, all greasy applications whatever, in the majority of cases, are useless, and the patients themselves inform me, are very "heating." In some rebellious chronic instances, I have thought the oil of cade, has been beneficial, applied locally, although I have not yet tried it sufficiently often, to recommend it strongly. In a few cases of acute eczema, I have tried the freezing process recommended by Dr Arnott, but the salt of the frigorific mixture, and the cold itself, has caused apparently so much agony that I have been deterred from using it, especially when the emolient moist alkaline application is so efficacious. This mode of treatment, however, undoubtedly demands further trial, and I propose to report a more extended experience of it on some future occasion.

Herpes.—This disease generally runs its course in about fourteen days, and requires no treatment whatever, further than an acetate of lead lotion to allay the smarting. It is not very common.

Scabies occurs very frequently, and is cured by a host of remedies. A strong lather, made of common soft soap and warm water, twice a day, answers very well. The question with scabies, is not what remedy is useful, but which will cure it in the shortest period. The most extensive experience at St Louis has shown, that the sulphur and alkaline, or Helmerinch's ointment cures itch, on an average, in seven days. That sulphur, however, is not the active remedy, I have satisfied myself of by experiment. Soft soap, as we have seen, which contains alkali, and even simple lard, if pains be taken to keep the parts constantly covered with it, will cure the disease as soon as sulphur ointment. I have tried the Stavesacre ointment, recommended by M. Bourguignon, in only a few cases, but found it to answer very well. Its superiority, however, over other applications, I am not yet prepared to admit.

Pemphigus.—This is rather a rare disease, and when chronic, coming out in successive crops, is very rebellious. Two cases which entered the Infirmary last winter were cured in a few weeks, by the weak alkaline wash, applied as in the case of eczema, combined with generous diet.

Impetigo.—This affection in all its forms is very common, and is best treated by the weak alkaline wash, exactly the same as in eczema. In the chronic forms which attack the chin of men, constituting one of the varieties of mentagra, the same treatment cures the most rebellious cases, if the moisture be constantly preserved. For this purpose the hair must be cautiously cut short with sharp scissors, and the razor carefully avoided. If the side of the cheek covered by the whisker be attacked, removal of the hair from thence also is essential to the treatment. A bag or covering accurately adapted to the part affected must be made of gutta percha sheeting, and tied on with strings. This may be covered with a piece of black silk, to allow the individual to go about and carry on his usual occupations. In this way I have frequently seen chronic impetigo of the chin, of from

sight to ten years' standing, completely removed in a few weeks. But then the surface must be kept *constantly* moist, a circumstance requiring great care and determination on the part of the patient. When it becomes necessary to shave, flour and warm water, or paste, should be used, and not soap. Alkalies applied from time to time only, as in the form of wash or soap, always irritate, although, when employed continuously, they are soothing.

Ecthyma is not a common disease, and usually presents itself as the *E. cacheticum*, requiring in addition to the alkaline wash locally, a generous diet.

Acne is a disease always requiring constitutional rather than local remedies. Although not uncommon in private, it is rare in hospital practice. Careful regulation of the diet, abstinence from wine and stimulating articles of food, watering places, baths, etc., etc., constitute the appropriate treatment.

Rupia.—This disease I have never seen occur but in individuals who have been subjected to the influence of mercurial poisoning. Hydriodate of potassium and tonic remedies, with careful avoidance of mercury in all its forms, is the treatment I have found most successful.

Lichen and Prurigo.—In both these affections constant inunction with lard is as beneficial as constant moisture in the eczematous and impetiginous disorders. In the prurigo of aged persons, the *Ung. Hyd. Precip. Alb.* is a useful application, although the disease is not unfrequently so rebellious as only to admit of palliation. The chronic papular diseases often constitute the despair of the physician.

Psoriasis, and that modification of it known as *lepra*, are very common diseases, and are uniformly treated by me externally with pitch ointment. I have satisfied myself by careful trials that it is the pitch applied to the part that is the beneficial agent, as I have given pitch pills, and infusion of pitch, largely internally without benefit. With the hope of obtaining a less disagreeable remedy, I have frequently tried creosote, and naphtha ointment and washes, but also without benefit. Lastly, I have caused simple lard to be rubbed in for a lengthened time, but without doing the slightest good. The oil of cade is also very useful, especially in psoriasis of the scalp. Internally, I give five drops each of Fowler's solution, and of the tr. cantharidis. It is rare that the internal treatment alone produces any effect on a case of psoriasis of any standing. If a case resists this conjoined external and internal treatment, I have always found it incurable. About a year ago I carefully treated a series of cases internally, with Donovan's solution, without producing the slightest benefit.

Lupus is a constitutional disease, and must be treated by cod liver oil, and all those remedies useful for scrofula, of which it is a local manifestation. The external treatment is surgical, consisting of the occasional application of caustics, red lotion, ointments, etc., according to the appearance of the sore.

Favus is a very common disease in Edinburgh, and is most readily removed, first, by poulticing the crusts till they fall off, and the skin presents a smooth, clean surface; secondly, by shaving the hair; and, thirdly, by keeping the scalp continually covered with oil, so as to exclude the atmosphere, and prevent the growth of the parasitic fungi, which constitutes the disease. For this purpose, a gutta percha or oil silk cap, must be constantly worn. A continuance of this treatment for six weeks produces a cure in young persons, if combined with cod liver oil, generous diet, and anti-scorfulus remedies internally. I have tried the lotion of sulphurous acid, recommended by Dr Jenner, and found it successful in a few cases, but the treatment by oil is so easy as to be far preferable to it. Very chronic cases are cured with difficulty, but so long as the oil is applied the disease never returns, and mere freedom from the disgusting crust is a great gain.

Scalp diseases must be treated according as it depends on eczema, impetigo, psoriasis, or favus, in all cases first removing the crusts with poultices, then keeping the head shaved, and, lastly, applying alkaline washes, pitch ointment, or oil, according to the directions formerly given. Ringworm is a disease I have never seen in Edinburgh, and of what it consists I am ignorant. Some writers apparently consider it to be favus, and others a form of herpes. On two or three occasions I have seen a scaly disease of the scalp, in the form of a ring—that is lepra, which I have cured by pitch ointment, or oil of cade. My friend, Dr Andrew Wood, informed me some time ago, that he banished it from the Heriot's Hospital school by condensing on the eruption the fumes of coarse brown paper, and thus causing an empyreumatic oil, or kind of tar, to fall upon the part. This has led me to suppose that it is a scaly disease, and a form of lepra or psoriasis.

So-called *syphilitic diseases* of the skin, are, in my opinion, the various disorders already alluded to, modified by occurring in individuals who have suffered for periods more or less long, from the poisonous action of mercury. A longer time will be required for their cure, but the same remedies locally, conjoined with hydriodate of potassium, in smaller doses, with bitter infusions, tonics, and a regulated diet, offer the best chance of success.

The great difficulty in the treatment of skin diseases, generally consists in their having been mismanaged in the early stages—a circumstance I attribute to their not having, until a recent period, been much studied by clinical students. Many chronic cases of eczema are continually coming under my notice, which, in their acute forms, have been treated by citrine ointment, or other irritating applications, which almost invariably exasperate the disorder. I shall not easily forget the case of one gentleman, covered all over with acute eczema, who had suffered excessive torture from its having been mistaken for psoriasis, and rubbed for some time with pitch ointment. In the same way I have seen a simple herpes,

rich would have readily got well if left to itself, converted into an ulcerative sore, by the use of mercurial ointment. Nothing is more common than to confound chronic eczemas of the scalp with favus, though the microscope furnishes us with the most exact means of agnosis. I need scarcely say that the correct application of the remedies I have spoken of can only be secured by an accurate discrimination, in the first instance, of the diseases to which they are applicable.

The general constitutional treatment in all these cases seldom demands aperient or lowering remedies except in young and robust individuals with febrile symptoms. In the great majority of cases, cod liver oil, good diet, and tonics are required. In a few instances,atives, both locally and internally, are necessary to overcome excessive itching or irritation. These the judicious practitioner will readily understand how to apply according to circumstances.

ARTICLE IV.—*Remarks on the Treatment of Disease.* By W. O. MARKHAM, M.D., Assistant Physician to St Mary's Hospital, London.

THE study of modern Pathology has advanced in a most surprising manner our knowledge of the diseased conditions of internal organs. It has corrected many erroneous views once held as to the nature of diseases; it has given us surer bases for the establishing a better road to the comprehension of their nature; and, in addition, it has seen the means of giving an unhoped for degree of certainty and extension to our powers of diagnosis.

In an especial manner do these remarks apply to the diseases of the organs contained within the thorax; and if any one should desire to measure the true and real advantages which the physician has reaped in diagnosis from the study of pathology, let him place himself for a moment in the position of those who practised the healing art, before the days of Avenbrugger and Laennec—bring himself face to face with diseases of the lungs and heart, and then try his hand at the treatment of those diseases, guided solely by the general symptoms which they offer, and unaided by the lights of physical diagnosis. Thus insecurely guided, he will soon find how poor his powers of analysing disease are; and in the case of diseases of the lungs, for instance, he will have to admit, with Cullen, that he knows not how to distinguish between them. Pleurisy, bronchitis, and pneumonia were, by that acute observer, all classed under the head of inflammation of the lungs. It is, indeed, most true that any man, at the present day, moderately versed in the use of the diagnostic aids which medical science places at his disposal, can exercise his judgment upon the nature of internal diseases, as they exist during life, with a hundred times greater probability of success, than could by any possibility have rewarded the desires of the

most skilled and practised physicians of old days ; and, as we have already said, for this our thanks are due to modern pathological researches.

Never was pathology more carefully cultivated than at this present time, and certainly never was the study of the signs of disease of internal organs, derivable through their physical diagnosis, more curiously and minutely investigated. It may, perhaps, be even suggested that, beyond a correction and development of facts and principles already laid down and established, neither the one nor the other of these studies, *as hitherto pursued*, have much more information to give us. The knife and the eye of the pathologist have left but little of the tale of disease untold, as far as by them that tale can be told. And, in the physical diagnosis of internal diseases, all we can expect to pick up in this cultivated field are, perchance, some small gleanings of the great things established by those master minds who gave us percussion and auscultation. Avenbrugger and Laennec have left us little either to take from or add to the facts and principles severally taught by them ; both brought their respective methods of diagnosis to a high degree of perfection.

Such being the position of the pathology and diagnosis of internal diseases, one cannot help thinking that the time has arrived when we may venture to consider dispassionately what effect has been exerted on our practical application of medicine to the treatment of disease, through the advances made in these studies ; what new aids or guidance have been given by their light to the therapeutics of the physician ; whether they have furnished him with fresh weapons whereby to combat disease, or have instructed him how more effectually to use those he already possessed ; whether, in short, he has turned to their true account the lessons they have taught him. For, after all, we must remember that this pathology and diagnosis, of which we boast so much, are but two of those many handmaids, whose duties are wholly subservient to the one great object which is the end and purport of the physician's life, viz., the cure of disease.

Scientific therapeutics must of necessity be based upon diagnosis ; the nature of the disease and its seat must be ascertained, or how are we to hope, amidst the many agencies which are at work in the various organs of the human body, to reach by our remedial act the one which is at fault ? In the case of these particular diseases to which we have specially referred, what has this extraordinary improvement in diagnosis done for us ? This is the question which we would ask, and which we would gladly see fairly and conscientiously answered. It is impossible not to admit, amongst the conflicting opinions, the unsettled and changing sentiments which the most valued and esteemed practitioners of our art exhibit in their treatment of acute disease, and I will here take, for special exemplification of my position, the acute diseases of the heart and its

membranes—that a true *ratio medendi* is still to be discovered; that the problem of their treatment has yet to be worked out.

As this assertion may seem strange to some readers, I must state on what grounds I feel justified in making it; and I will take inflammation of the pericardium as my illustration. Now, this disease in its acuter forms may, in an especial manner, be written down as the representative of so-called inflammatory disorders; here we have the thing, whatever it be, denominated inflammation, in its typical form; here is a malady which, if it kill not in the violence of its first onset, will yet leave behind it, what we must ever dread, a disorganized condition of one of the tripods of life—the great central, and ever-active organ of the circulation—a condition which in its turn will give rise to, and be succeeded by other disorders in different organs of the body; the end of which, as sad experience but too truly demonstrates, has, for the most part, but one issue—a fatal, though it may be a late, one. I know not if any disease in its present and remote consequences, is more to be dreaded than this.

What now has medical science, resting upon the accumulated facts, which experience has placed at her disposal, what antidote to such a bane has she to offer? Our pathology and a skilful diagnosis enable us to trace most anxiously, from day to day, and with surprising accuracy, the progress of the malady; we even anticipate its insidious attacks, and scarcely sooner can pathology demonstrate the existence of the inflammation to the eye, than does our diagnosis reveal it to the ear. If ever disease demands the physician's earnest thought, this one surely does; its progress is rapid, and it may kill at once.

And what, now, has medicine to oppose to this vigorous and destructive malady? What are the results of treatment which may be fairly deduced from the accumulated experience of scientific practitioners? Well, we find, that to the violence and energy of this inflammation the most vigorous *antiphlogistic* treatment has been opposed; the lancet has been plied with a most unsparing hand; but does M. Bouillaud find believers in, or imitators of his treatment now? On the contrary, his heroic and *certain* method of arresting the destroying agent—of “extinguishing” the disease, has been convicted of error, and condemned as “uncertain and very dangerous.” The men of authority, also, amongst us, I mean those to whom experience and authority rightly give that title, will be found rather to warn the inexperienced against the lancet, and to arrest his hand, than to tell him of the *benefits* of blood-letting; we find them expressing regrets at the disappointed expectations of their former hopes and faith in this remedy, and bidding others beware of the *dangers* even which their experience had seen resulting from it; they pretend not to giving a true estimate of its value.

And mercury too, that other prime arrester of inflammations—has the voice of observation still to echo the praises which were once

so warmly sung of its powers in combating and arresting this inflammation of the heart's membranes! Ah! no, it is a melancholy fact, but it is one nevertheless, from which, if wisely stored for future use, we may learn wisdom, that in the use of this remedy also, one of the most observant and practical physicians amongst us has found the ground of a firm faith undermined by the truth-telling effects of a more extended experience. "After blood-letting rapid induction of the mercurial action is of the greatest consequence," so writes an authority, in a most unhesitating style, some fifteen years ago. And then, when we turn to the last best authority, who has given the world the benefit of his long and great experience on these subjects, and anxiously hope to find in him a guide amidst these difficulties, what does he tell us consolatory? Why, it must be admitted, that his chapter on the treatment of pericarditis is the most unsatisfactory in the book, but saying this is not to say ill of it, for it may thus perhaps just most truly represent our actual knowledge of that treatment.

And all the other methods of combating this inflammation in its acute form which have been from time to time well recommended, and are now in practice amongst us—purgatives, opium, salines, etc.,—what are we to say of them? Let me put the question in another form, and, appealing to the experience of individuals, ask what treatment they adopt—what is the treatment they adopt, at least, with hopes of success, in arresting the inflammation? The answer to this question will be found in the fact (which any one who has had occasions of observing the practice of many physicians must know), that there is little accordance in opinion and in action as to the course which should be pursued; that the hopes and the confidence of to-day, derived from some happy issue which the disease has found under his hands, are belied by the reverses which await the physician to-morrow. And is it not a fact, also, that the more experience he has gained in the treatment of this disease, the more cautious and quiet—expectant if you please—he becomes in his treatment? Failures and failures in treatment, have driven physicians to the pass of leaving the disease and the system to struggle for the mastery between themselves without interference; and men have done this whose honour, and reputation, and scientific knowledge are above all doubt—they have done it as the conclusion forced upon them by long and well-gathered experience, and it is useless, and worse than useless, for us to pretend to deride such men: they act conscientiously, and they act as they have a right to act; they may fairly turn round to their objector with the desire that he shall give his *proofs* of a better knowledge,—

"Si quid novisti rectius illis candidus imperti;"

and not unfairly add the conclusion,—

"Si non his utere mecum."

.... "Qui vous dit," says Majendie, "que la maladie n'eût pas également guéri si vous ne l'eussiez point employé? Le rhumatisme cède aux saignées, cède

au tartre stibié, cède à tous les traitements imaginables; il cède surtout au simple séjour au lit, et aux boissons adoucissantes, Jamais, à mon hôpital, je n'ai recours à la lancette, ni à l'emetique, ni aux sangsues pour combattre ce genre d'affection, et je ne crains pas de le dire, j'ai vu guérir autant de rhumatismes que j'en ai traité."

Assuredly the very most that the warmest advocate of any particular treatment can say of it is, that the firm conviction which he has of its efficacy is founded on the results of his personal experience, and of what, perhaps, he may have gathered by communication from the experience of others; but let me ask that individual to lay aside that fatal idea, "firm conviction," and analyse the actual facts on which he has based that firm conviction: to compare his facts with the facts of other equally honest and talented observers as himself: not to forget that there is a *post hoc* and a *propter hoc*: that cause and effect, and antecedence and sequence are different affairs: and that better men, it may be, than himself, have had again and again to grieve over "firm convictions" blown to the winds by experience. Let him do this fairly, and can it be doubted that the result will be this, viz., astonishment at the slender foundation on which his *certain* treatment has rested.

I have, hitherto, been keeping in view inflammation of one particular part of the body; but which are its acute diseases to which these remarks will not apply? few indeed I suspect. And let me for a moment longer dwell upon this subject, and ask what pathology and diagnosis have added to the power of medicine in combating those large classes of diseases, which, at times, decimate the population of our cities. We cannot look back on these diseases, historically, without calling to mind the fatal influences which have misguided the physician's judgment: the fatal consequences which have resulted from his blind faith in specious dogmata and captivating doctrines respecting the nature and local habitation of fevers. We may laugh at Dr Sangrado's vagaries, for we read them as a joke; but in what did the results of his "method" differ from those which followed the doctrines inoculated in the medical world by the extraordinary eloquence and enthusiasm of Broussais? These it is impossible to contemplate without a shudder, and the poison is not yet extinct. Pathology has done much for us; we have ceased to give to fever a local habitation; the brain, the lungs, the heart, the abdominal viscera are no longer either individually or collectively the centres from which radiate the phenomena, whose outward manifestations we denominate fever; these are great facts acquired to medicine, and let us be contented with our legitimate gains. Is it nothing that we have determined that typhus is not inflammation of the brain,—that it is not a gastro-enterite,—that it is not inflammation of the lungs? Our ideas concerning fever have undoubtedly become more precise; pathology has cleared away the mists of many errors which surrounded the subject; we no longer commit the grievous errors in treatment which were the necessary consequences

resulting from the localising of the disease spoken of above. But how does our account stand with its treatment now? I would answer this question by asking any one who is anxious to investigate the matter, to consider the different treatments adopted by various practitioners for the cure of this disease—by practitioners not living in different countries, or in separate cities of the same country, but by those exercising their art upon the self-same disease, under the same roof, in the same public hospitals; and he will find them not using unlike, but adopting the most opposite remedies; in the language of the author above referred to, I may say:—

“Ce serait une étude bien affligeante que de passer en revue les divers modes de traitement employés dans chaque localité contre la même affection morbide . . . voyez ce qui se passe de nos jours, au sein même de la capitale, dans nos principales cliniques. Un malade est frappé d’une fièvre typhoïde, eh bien! Suivant qu’il a été dirigé vers tel ou tel hôpital, il sera soumis à un traitement différent: à la Pitié, on n’aura point recours aux mêmes moyens qu’à la Charité, ni qu’à l’Hôtel-Dieu. Tel praticien vante les purgatifs, tel autre préfère la saignée, un troisième s’applaudit de l’emploi des toniques, d’autres enfin, et je suis de ce nombre, laissent la maladie parcourir à peu près librement ses périodes sans chercher à enrayer sa marche.”

It is needless to pretend to speak here of the multitude of specific remedies which have been reported as cures of fever; they tell their own tale, and the last has shared the fate of the first. In France, men were even quarrelling about the authorship of the discovery of the cure of fever by quinine, when the so-called cure itself was shown to be a failure here!

If I wished to illustrate farther the position I assume, I might follow out the subject by canvassing in a like spirit our treatment of almost all other acute diseases, and many diseases likewise not acute. Certainly, it is true, that the more numerous the specific remedies and treatments adapted to any malady are, the more sure we may be that the malady is especially one for which no specific treatment has yet been discovered. The remedies which have been at one time or another declared positive curatives of epilepsy amount to more than a hundred, and fresh ones are still being added to the catalogue; but I suppose the actual experience of the present day differs not from that of Heberden as regards the issue of this malady; he knew no case in which the fits, when present after puberty, were ever got rid of.

Surely some lessons of advantage may be likewise drawn from calling to mind the “cures for the cholera,” with which the Medical and other journals teemed upon this last occasion, as on other occasions, when that fatal epidemic was amongst us. Some men have even had the courage to write with a degree of certainty about the nature of this infliction; but after all the vaunting of cures and illogical reasonings upon the nature of this disease, what do we really know of the matter? The summary which Andral gave of it years gone by, is still well fitted to and descriptive of it: “caractères anatomiques, insuffisantes; causes, mystérieuses; nature, hypothé-

tiques; symptomes, caractéristiques; diagnostic, facile; traitement, douteux." Stripped of all hypotheses, what remains to us of positive knowledge concerning this scourge? absolutely nothing—proud medicine, girt with all her scientific aids, must learn to bow the head.

But, it will be said, are we to infer from all this that the physician is to remain a calm spectator: that he is to sit quietly down, *les bras croisés*, while disease is running riot and committing its worst ravages? By no means, the man of action must act, even though struggling through the mist of errors. But how act? therein lies the pith of the matter; let us consider the point from whence a rational method of action may be derived, and compare it with the manner of proceeding above spoken of. Our art, conjectural as it is, possesses principles, and the harvest of men's experiences has brought rich treasures of undoubted facts into our garner, and these facts lie ready to our hand. Now the physician, in his treatment of this disease, rests his views upon, and is guided by the knowledge he possesses of certain laws of the organism, and of the effects which result from morbid causes acting on that organism, he strives to find analogies, in some of the morbid phenomena which it presents between it and other diseases, into whose nature he has a farther insight, and over whose progress he is justified by experience in believing that he possesses control; and having found such analogies, even though but feebly marked, he is justified, on every true principle of reasoning, in directing his remedies in that sense; and then watching the results, or rather marking, as far as the complex nature of the problem will allow him to mark, the consequences, real or apparent, which ensue, he must be contented patiently to record—simply to record—these consequences, the results of his experience, until at length accumulated experience shall justify him in drawing conclusions, which he may use as principles.

The unhesitating conclusions and inferences which are so often jumped at, in such cases, certainly shows an eager and very natural desire, on the part of those who watch disease, to be forward in the work of doing good, but it exhibits an utter indifference to a rational consideration of the subject.

Let us consider this matter a little more closely, for the arguments which affect it, involve in a higher or less degree the treatment of acute disease in general. What I have said, that the problem before us is most complicated, every one will readily admit; the data upon which we have to operate are hypothetical; positive starting point for inquiry we have none; of the essence, or intimate nature of the choleraic poison, we know nothing; how it enters the human body; where it first fixes its grasp, whether upon the fluids or solids, or whether upon one particular solid or fluid;—all this, science has yet to make manifest. Chemistry, the microscope, and the pathologist have done their best, but for any solution of these things in a positive sense, they have done nothing. And yet

our profession boasts of its cures day after day! Consider the malady and consider the boasted cure! Here is a disease which, at one fell swoop, may strike the whole organism and all its complicated arrangements; in a few hours annihilate its vitality. The shrunken features, their leaden hue and blue pallor, the violent spasms of the muscles, the chilly breath, the ice-cold limbs, the thick breathing, the imperceptible pulse, the exhausting discharges of fluids, the sunken eye, the wasting body—wasting under the eye of the looker-on—all manifest the fact, that a most fearful enemy has obtained possession, not of this or that vital part, but of every part of the living structure. Well, and men will boast, that a disease like this they have cured by the administration of a few grains of mustard!

It is not surprising, when this disease first appeared amongst us, that the whole armentarium of the Pharmacopœia should have been indiscriminately turned against it; and that men should have rushed hastily to the conclusion (their wish being the willing father to the thought), that when the patient died not, his life was saved through their interference; but I must venture to think, that there is something very loose in our medical logic, when, with the abounding results flowing from the treatment of two onslaughts of the epidemic before us, results which give to the unbiassed mind the most positive disproofs of the efficacy of the hundred remedies which have been vaunted and published as cures for the cholera—we find ourselves still boasting of our remedies, and still publishing our cures, and even upon the same insufficient data guiding us; what right have we, who act thus irrationally, attributing consequences to antecedents, where there is no rational link whatever, beyond that of mere sequence, by which to trace the connection—what right have we to express anger and astonishment that empirics impudently advertise their cures, and that good meaning men, in the public prints, under the names of Clericus, etc., insult the doctor's treatment, and magnify their own nostrums? How can we be surprised that quacks boast their specifics, when we, the legitimate sons of medicine thus publish our cures, and give them the sanction of our assertions?

There is no one influence more baneful and grievously obstructive to the progress of the medical art, than the ever hasty desire, so constantly alive amongst us, of generalizing upon, and drawing conclusions from, the results of observation; however isolated a fact, however barren it may yet intrinsically be, the eager mind still strives to show its antecedent connections, and to draw conclusions from it; into what errors and absurdities this hastening to be wise has conducted them, in what fallacious and extraordinary reasonings in practice it has involved them, he who is desirous of learning will find proofs enough written on every page of the history of medicine. By thus casting dust in the eyes of others, and perverting our own vision, we raise up positive barriers to the advance of true knowledge, for now the mists and delusions which our faulty haste has generated, must be swept away, before the honest face of the

simple fact can again be made available to lighten our slow steps along the difficult passes of medical knowledge. And such unphilosophising haste in drawing conclusions from insufficient data, how often does it humiliate us in the sight of the public! When the fallacy is demonstrated and swept away, the deluded hopes and promises to which it gave birth still linger and rankle in the memory.

Let medicine take her stand upon her positive works: they are surely something to be proud of; the ardent labours and devoted energies of those who have struggled and are still struggling in her service have not been unrewarded; the tree is vigorous, though its growth be slow; each fact gained, small and insignificant though it may seem in its isolation, is a fresh evergreen, and persistent leaf—another item added to it. By the combination of individual facts (and each, sooner or later, must find its proper adjustment in the scheme), medicine can alone hope to become a consistent whole. A positive fact—that is, a result which extensive experience demonstrates to us as the invariable consequence flowing from certain given conditions—becomes a principle in our institutes of medicine, which will endure as long as disease afflicts the body; or, if its immediate antecedents and its consequences be yet undiscernible, or dimly seen, there it stands, disconnected as it seems, one more aid acquired, one more solid basis to rest upon, awaiting its true interpretation, which will be manifested either through the illustration it may acquire from other like isolated facts already gathered, or from those which, yet undiscovered, remain to reward the searcher's industry; and who can doubt (however far distant the day may be) that the accumulation of these treasures will, at last, in the hand of some comprehensive genius, be made to yield grand and general principles in medicine?

Much, I believe, of the erroneous method we pursue in the treatment of diseases, as here related, may be traced up to the views we are apt to take of their nature. Such views have, not unnaturally, resulted from the manner in which we have been hitherto compelled to study pathology. The searching spirit with which the physical diagnosis of disease has of late years been prosecuted, may also have to answer, in some degree, for the confined ideas which its cultivation has engendered. Pathological investigations have been necessarily exercised, in the first instance, upon the morbid changes which individual organs and parts present to the anatomist, and this has led us often enough to imagine that the abnormal conditions of this or that diseased organ was, in fact, the disease itself. Our physical diagnosis, too, so carefully applied, has also inclined us to give a locality to disease, and to withdraw our attention from broader ideas of its nature. We must remember, however, that these visible, or, as we call them, organic changes of parts, are, after all, and in almost every case, nothing more than the rude manifestations of some general evil, which has been long working in the system; that what we are pleased to call *the disease* is, in truth, the last conse-

quences of a series of morbid actions, of whose existence we thus obtain the knowledge, but whose intimate nature is hidden from us.

The investigations of the degenerations of the tissues, etc., of the body, which have of late been so acutely carried out, are now beginning to awaken the physician to the necessity of taking wider notions concerning the nature of disease, and must sooner or later work a change in our system of therapeutics.¹ They teach us that we have been wont to take too isolated notions concerning the nature of any one morbid condition, and lead us to see more clearly the mutual relations and dependencies which exist in disease between different organs, or parts of the same organ.

The duty of the mere pathological anatomist consists in accurately describing the character of the changes which healthy structures have undergone, but this does not suffice for the physician; he must embrace, in one general *coup d'œil* the whole of the disordered states which the body at any given time presents, and not let his opinion be swayed by any one particular, however prominent, disorder. It may indeed, and very frequently does happen, that the originally faulty organ—perhaps an injured valve of the heart—which has now given rise to a variety of ailments, is just the one which at the moment demands his attention least; he is not now to be speculating about the curability of the diseased valve, but his art is instantly required to relieve some oppressed organ—for example, the lungs—whose functions are seriously affected. The disease of the heart is forgotten for the moment amidst the disorders to which it has given birth. And so again must our treatment lose its local tendency in proportion as we begin to understand how many of the abnormal changes, which organs undergo, are but expressions, or results, and, for the most part (as far as our agency is concerned) the irremediable results of some diseased action which is at work, not in one organ only, but in many of the organs of the body at the same time.

Every day's new experience in pathology teaches us this: that in the treatment of disease we have much to unlearn; no need is there to tell us how much we have to learn. We cannot expect much progress in a right direction, while error still clings to us and clogs our footsteps. The most discordant opinions, which equally honest, and equally skilled men hold respecting the treatment of acute diseases, must have a meaning; and they have one which is often responded to by our consciences at the bedside of the patient. The follies and wickednesses of others may teach us much; and thus from homœopathy we may learn the mischief which the human body has often suffered from want of discrimination in the use of remedies; to what other cause can be attributed the successes of that cheat? How are we to reconcile, but in one way, the unhesitating opinions which men hold to-day, and the equally unhesitating,

¹ I must specially refer to the labours of my colleague, S. H. Jones.

but exactly opposite opinions on treatment, which they hold to-morrow? We may say diseases change their type, but what proof have we of this? And we may explain the discrepancy in half-a-dozen other ways, but we cannot deceive ourselves this way. Are we to believe that the human body has so changed its conditions, or that the nature of diseases is so totally altered, as thereby to give account of the fact of the wholesale blood-lettings which characterised the treatment of disease in the last generation, and the almost total abstinence from them which characterises the present treatment? Impossible. Is it overstating the bare truth to say, that we have yet to learn the effects of remedies on acute diseases? Have we data by which to answer the question? I think not. And the fair and legitimate inference which we have a right to draw from facts like these which we are bound to draw, I beg to assert, is this, that we have deceived ourselves into a belief that our remedies exercise a greater control over disease than they actually do. Why not confess an error, if error it be? If it be no error, why not prove clearly that it is not so?

(To be continued.)

ARTICLE V.—*Confirmation in two Quarters of the Discovery by Keber, of the Penetration of a Remarkable Body, believed by him to be a Spermatozoon, into the Ovum of the Fresh-Water Mussel.*
By MARTIN BARRY, M.D., F.R.S.

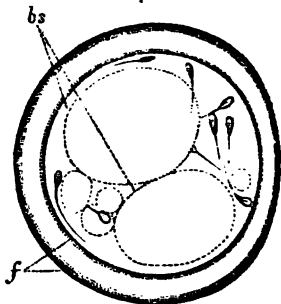
ON the subject of fecundation, the Cyclopædia of Physiology has the following remark:—"The truth is, that 'the how' of the fecundation is as far removed from our knowledge to-day as it was thousands of years ago; this process is still enveloped in what we feel inclined to consider its sacred mystery." There is much in fecundation that will doubtless ever remain a "secret mystery." But the excellent authors of the remark now quoted, add: "It would be different if we could prove that the spermatozoa really yielded the material foundation for the body of the embryo; that they penetrated into the ovum, and were developed into the animal, or else that they became metamorphosed into the central parts of the nervous system."¹

When the article from which I quote was published, *i.e.*, in 1849, a drawing from nature had been before physiologists for six years, representing spermatozoa, not indeed becoming "metamorphosed into the central parts of the nervous system," but *within the ovum*. That drawing was published in 1843.² As it has probably been

¹ Article "*Semen*," by R. Wagner and R. Leuckart. *Lc.* p. 507.

² In the *Edin. New Phil. Journal* for October of that year, Pl. V. fig. 1, with the statement that in an ovum of the same rabbit I saw more than twenty spermatozoa.

seen by very few, or is by this time quite forgotten, and the subject having at length become one of general interest, I now reproduce it in a wood-cut. It represents, in outline, an ovum of the Rabbit of twenty-four hours from the middle of the Fallopian tube; *f* is the zona pellucida; *bs*, the germ, consisting of two large cells. (There were present also several smaller cells. Such minute bodies (nuclei or cells) are very frequent. In 1840 I thought them merely the remains of cells of the so-called "yolk," not yet absorbed, but destined for immediate or speedy absorption. Since



1843, when I saw spermatozoa within the ovum added in *considerable number*, it has occurred to me that possibly the said minute bodies remain, in order to unite in liquefaction with these additional spermatozoa, after which the resulting compound is taken up by the already fecundated germ, *bs*). The spermatozoa are seen lying around and between the germ cells. When the ova were first examined, I thought I saw traces of spermatozoa within the germ cells. In the *Phil. Trans.* for 1843, p. 33, will be found a statement of these facts, as well as the mention of several ova in a somewhat earlier stage from the Fallopian tube of another rabbit, which enabled me to confirm the observation. I at the same time said: "These ova were submitted to the inspection of Professor Owen, and I afterwards showed one of them to Professors Sharpey and Grainger, all of whom agreed that the spermatozoa were contained within the ovum."¹

Now, as in the passage quoted from the *Cyclopædia of Physiology*, the admission was made in 1849, that it would be different "if we could prove that the spermatozoa penetrated into the ovum," it seems that after all "the *how* of the fecundation" had been brought a trace nearer to our knowledge rather less than "thousands of years ago," namely, in the year 1843.

Further evidence of this is furnished in a second postscript added by R. Wagner (one of the authors referred to in the *Cyclopædia of Physiology*) to the article "Generation" (*Zeugung*) in his *Dictionary of Physiology*. In this second postscript he remarks, that although up to almost the latest period there seemed sufficient ground for the belief that the spermatozoon has performed its part by simple contact with the surface of the ovum, yet that the still more recent observations of Keber in Germany, and Newport in England on the spermatozoon, with those of Johannes Müller on a funnel-shaped canal in ova of every species of *Holothuria* comparable to the micropyle of the vegetable ovum, open a field for renewed research, a field wide

¹ *Phil. Trans.* 1843, p. 33.

as the animal kingdom itself, one that will forthwith draw numbers to explore it, and is sure to be productive of a rich return.

He adds: "It is clear that many observations hitherto rejected as incredible, such as those of Barry on the penetration of the spermatozoon into the ovum, deserve renewed consideration, though they may not be confirmed in their whole extent."¹

From what I have stated to be contained in R. Wagner's second postscript, it will be seen that one of the main movers to a renewal of research in this field was Keber, by his discovery of the penetration of what he believed to be a spermatozoon into the ovum of the fresh-water Mussel.

The discovery of Keber, however, has in its turn been denied by Bischoff. (Bischoff is the man who more determinately than any other combated, as "born of the imagination," my fact that the spermatozoon penetrates into the interior of the ovum of the Rabbit. This he did until at the end of about the eighth of a century, *i.e.* in March of the present year, Meissner's confirmation induced him to renew his enquiries, which led to the confession by Bischoff that the mistake had not been Barry's, but his own.²) On hearing of Bischoff's denial of Keber's discovery, I should have investigated the thing myself without delay, but had not health for the labour. I therefore asked a friend to undertake it, one whose acuteness as a microscopical observer I well knew was equalled only by his care to record actual facts or none at all. It was Dr W. W. Webb of Lowestoft. I handed him Keber's work,³ along with many scores of the river Mussel. In due time Dr Webb sent me a report, stating that he must have laid some hundreds of ova under the microscope, and that they had given him "the means of verifying many of Keber's statements, and of witnessing the conditions, or nearly similar ones, represented in his drawings, numbered 38, 40, 42, 44, 48, 50, 56, 58, 64, 65, and 73." "The body believed by Keber to be a spermatozoon," says Dr Webb, "was thus seen repeatedly within the ovum, in various situations relatively to the micropyle and the yolk; and at last was found arrested in the very act of penetration." Particular attention was given by Dr Webb to stages in the existence of the micropyle, without entering into the question whether it was once the pedicle of the ovum. "In the first place," says he, "it is a tube, projecting, like a lipped chimney-pot from, and being a process of, the outer membrane. In a later stage, the chimney-pot has disappeared with the exception of its lip, which now no longer surrounds an aperture—the aperture having been closed up, and obliquely radiating from the lip are corrugations, invariably proceeding from left to right, and stretching out on the surface in undulating lines. Lastly, the corrugations have entirely disappeared, all that remains being the circular lip surrounding a clear closed up

¹ R. Wagner, second postscript above-mentioned, P. 1018^c.

² See the Philosophical Magazine for May 1854.

³ Ueber den Eintritt der Samenzellen in das Ei, Königsberg, 1853.

space." With regard to the question, whether these ova have three membranes (Keber), or two (Bischoff), Dr Webb informs me that he tried the effects of maceration for some hours in water, under a covering of thin glass; after which he observed blisters, with a rounded outline, upon the outer surface of the ovum, leaving with him no doubt of the reality of what is maintained by Keber—the presence of *three* membranes.

My health at length permitting it, I have since examined ova of the river Mussel, day after day, for weeks. The result is, that to Dr Webb's confirmation of Keber's discovery I am enabled to add my own. The remarkable body believed by Keber to be the spermatozoon of this animal does penetrate into the interior of the ovum by a funnel-shaped canal. I have seen it in various stages of penetration; it enters into the yolk; and it therein divides into many parts.

I have since found it so extremely easy to demonstrate this discovery of Keber to others, that I am astonished at its having been denied. That Bischoff's denial, however, has not met with universal confidence in Germany, is shown by the very recent work of Carl Vogt,¹ who not only considers the penetration of the spermatozoon into the ovum of animals in general as an established fact, but also declares his opinion, that "the denial so sweepingly pronounced by Bischoff will by and by dissolve into perfect acquiescence." I certainly should have no reason to be surprised at such a change, seeing that Bischoff, after having written that he would "ever combat the erroneous statements of Barry," that the spermatozoon really penetrates into the interior of the ovum of the Rabbit—in about four months, was compelled to acknowledge that Barry, after all, was right, and Bischoff wrong.

ARTICLE VI.—*Case of Secondary or Blighted Fœtus at the third month, with Fatty Degeneration of the Membranes, Retained and Expelled with its Living Co-twin at the eighth month of Utero Gestation.* By JAMES G. WILSON, M.D., Fellow of the Faculty of Physicians and Surgeons, Glasgow.

(Read before the Glasgow Medical Society.)

THE following case, which I beg to submit to the consideration of the society, is, I venture to say, one of a very important, interesting, and uncommon kind. The subject is one illustrative of many instructive points in intra-uterine pathology, and likewise suggestive of several reflections bearing upon medical jurisprudence. Without occupying the time of the society with any further introductory observations, I shall at once proceed in the briefest possible manner to relate the history of the case, and conclude with a few remarks.

¹ Physiologische Briefe, dritte Abtheilung, Zweite Auflage, 1854. SS. 480, 675.

Mrs T., aged 32, a strong healthy and masculine looking woman, was, on the 11st November last, after a short and easy labour, delivered of a living female child. It presented with the breech in the right anterior dorsal position, and weighed 5½ lbs., which, from appearance as well as from the mother's statement, indicated its age to be about the eighth month. The placenta was expelled entire in about the usual time. Patient states that five years ago she gave birth to twins, both boys, alive and at the full time, and that, with the exception of the present, she has had no children or abortion since. Her catamenia were regular during the interval, and she menstruated for the last time about the beginning of March. When three months or thereby advanced in her present pregnancy she had twice in succession a considerable sanguineous discharge per vaginam, accompanied with slight uterine pains. She, however, paid little attention to these symptoms at the time, as they did not occasion any anxiety or alarm.

The placenta, on examination, seemed generally healthy, with the exception of a few small indurated points of a tubercular character scattered over various parts of its extent. Towards the margin of the placenta there was found a small *foetus* closely enveloped in a yellowish fatty-like mass, which lay in a groove or sulcus formed in the placenta by the overlapping or bulging over of a portion of its foetal surface. The whole was connected together by a prolongation of the placental substance into the inner border of the investing membranes of this small *foetus*. The bond of attachment was by a spot not larger than a shilling, which was very slender, and separated in two places. The fatty-like mass, when opened up, was of an irregular circular form, measuring 8 inches in circumference, and was of the thickness of ordinary plate glass. It appears to be a homogeneous mass on cutting into it, and of a uniform density and consistence throughout. On subjecting a portion of it to microscopic examination it is found to consist of a fibrinous or lymphous substance with numerous oil globules intermixed. It is soluble to some extent in ether. There was no appearance of liquor amnii. The *foetus* appears to be about the third month—is very much flattened out and disfigured, having a white exsanguine look, with no traces of incipient putrefaction. It measures 4½ inches in length. The right arm is to some extent malformed and displaced; the humerus, with its atrophied muscles, is completely incorporated with the side of the head, while the forearm is altogether above the head, and appears as if originating from it. In consequence of the pressure to which the *foetus* has been subjected a large portion of the bowels protrude. The sex of the child cannot be ascertained. Proceeding from the *foetus* is a small thready or filiform umbilical cord 2½ inches long with the spiral convolutions formed by its vessels in great part obliterated. It is inserted into a spongy cellular substance (about the size of half a crown), much resembling placental tissue, situated at the inner border of this mass of fat, and which seemed to be the point of union with the placenta of the living child above referred to. There is, however, no distinct line of division observed between these two textures, as the one passes gradually and imperceptibly into the other. On the supposition that this substance, to which the cord was attached, was the placenta of the *foetus*, the formation of the peculiar groove in which the whole was embedded may be thus explained. The two placenta appear to have been originally united by their edges, the lesser one having ceased to enlarge, the other one gradually extended in circumference, till ultimately a flap-like portion, developed from its foetal surface, covered in the other along with the *foetus*. The relative position, however, of the double set of membranes, in consequence of some accidental laceration either during or after the expulsion of the placenta, could not be accurately ascertained. It is impossible to tell, on inspecting the preparation, what uterine attachment the blighted *foetus*, through the medium of its placenta, originally possessed.

There can be no doubt, I think, but this was primarily a case of twin conception, while one was blighted at or near the third month, and the other carried to the eighth, when both were nearly simul-

taneously expelled from the uterus. I dismiss, therefore, altogether the idea of conception subsequent to that of the living child. For the elucidation of this subject we need not have recourse to the absurd and nearly exploded theory, of superfœtation. The problem can be otherwise satisfactorily explained, by supposing the blighted fœtus, as I have already stated, to be the product of a conception coeval with that of the living child. One peculiarity in this case is the retention of a dead fœtus *in utero* for such a length of time, and the ejection a few minutes after the birth of the living child; yet when the placenta are united in a twin gestation, it is difficult to conceive how one fœtus could be discharged without the expulsion of both. Were the symptoms of threatened abortion, already noticed, an attempt of nature to throw off and get rid of the dead child? or had they any thing to do with its death, or were they the cause or the result of the partial separation that was afterwards found to exist between the two placenta. Whatever view may be taken of these points, one thing seems evident, viz., that the symptoms of miscarriage closely correspond with that supposed period of utero-gestation at which the blighted fœtus had arrived.

The intra-uterine death of the child may take place at any period, and is usually ascribed, either to some maternal emotion, accident, or disease, or to some organic disease, or structural defect in the fœtus itself, or its placenta. Several of these, for any thing that is known to the contrary, may have contributed, directly, in producing the fatal result, yet I am inclined to think that the principal or immediate cause of the child's death was the diseased state of its investing membranes, and consequently that the state of the membranes was not the result of some post-vital change. The morbid condition of the membranes would not permit the necessary amount of yielding and stretching, and would consequently tend to retard and arrest the growth and development of the fœtus, and ultimately deprive it of vitality. It may be said, and it is quite possible to conceive that the cord would, under these circumstances, be necessarily exposed to much pressure, having been it may be supposed, very early deprived of the protecting agency of the liquor amnii. The question naturally suggests itself, where, and by what means was this fatty-like mass deposited? I have already stated my belief, that this abnormal product was not the effect or result of some post-mortem change. From its position and general appearances, I was inclined, at first view, to believe that it consisted principally of the placenta in a state of fatty degeneration. Subsequent examination, however, convinced me that the supposed placenta (unless hypertrophied) was quite disproportionate to the dimensions of the fœtus; and, moreover, the almost glossy smoothness which both surfaces of the mass presented—the doubling up of the mass itself upon the child, and the position of the cord—seem to oppose and subvert this view. These circumstances being considered, it becomes more probable that the adipose mass

deposited between the amnion and chorion, and the product of some morbid condition of one or other, or both. The subject, therefore, resolves itself into the vascularity or non-vascularity of these membranes,—a point which has been long disputed, both by physiologists and pathologists. Although the human amnion and chorion, in their healthy and extended state, possess no blood-vessels, visible either to the naked eye, or by means of the microscope, or by artificial injection, yet the following facts and circumstances suffice to place the vascularity of these membranes beyond the shadow of a doubt. Tubercular, sarcomatous, and other abnormal deposits, have occasionally, though rarely, been found between the amnion and chorion, or between the laminæ of the latter, proving that they must have been developed from blood vessels. The membranes themselves have, in a few instances, been found in a congestive and inflammatory condition (amnio-chorionitis), and also in an opaque and thickened state.¹ The foetal membranes have been observed to be of a decidedly yellowish tinge when the mother was labouring under jaundice at the period of delivery.² Again, one or two cases are recorded where the umbilical cord of the human embryo had been actually inserted into the amnion, showing that the foetus must have derived its nutrient supply of blood principally, if not altogether, from that source.³ Comparative anatomists have demonstrated, that in some of the mammalia the vascularity of these membranes may be rendered quite evident by artificial injection. The villi of the chorion, at an early period of embryonic existence, and the great secreting power which the amnion is known to possess, may also serve to confirm and corroborate these statements. The state of the membranes is doubtless one of the most interesting and peculiar features in the case just detailed. Although fatty degeneration of the placenta, and placental diseases in general, are by no means very uncommon, yet I have reason to believe that disease of the membranes, particularly from fatty deposit, is exceedingly rare. Would the death of the child, in the manner supposed, prevent, impede, or in any way affect the growth of its placenta? Seeing that the placenta, when first examined, was free from all appearance of disease, and was in a position not so likely to be interfered with as the foetus, I think there is every probability that it would—in the same manner as a limb becomes atrophied from disuse, and in the same way as the uterus, containing a single foetus, ceases to enlarge with its death—so the congeries of vessels composing the placenta when their circulation had become arrested and unnecessary for the nutrition of the child, would also cease to enlarge. The cause of the collapsed and

¹ See Granville's *Graphic Illustrations of Abortion*, etc.

² Hardy and M'Lintock, p. 52.

³ There is a preparation of this kind in the museum of the College of Surgeons, London. An analogous case is recorded by M. Lamestre. *Archives Générales de Médecine*, June 1848.

profile appearances which these blighted or secondary fœtuses generally present, is for the most part mechanical in its nature. When the liquor amnii has become discharged, and the dead fœtus having lost all power of resistance, it is quite natural to suppose the flattening to be the result of continuous and increasing compression between the uterine parietes on the one hand, and the progressive development of the living child on the other. It may at first sight appear difficult to conceive why a fœtus that is early blighted and long retained *in utero*, should continue so fresh and free from the putrefactive process. This remarkable immunity from decay in such a case is usually ascribed to the exclusion of, or non-exposure to, the external air. The fact that new-born children are not unfrequently found in a more or less putrid state, (even when the membranes are un-ruptured, and consequently preventing the access of atmospheric air), seem to militate against this explanation. In reference to this subject, Ramsbotham says, "That it perhaps may be accounted for by the powerful vital principle, which is resident in the gravid uterus, and which is in fervid operation for the purpose of bringing to perfection the living being it contains, protecting the dead mass from the ordinary changes of decay."¹ It appears to me that this very process of flattening may prove of itself a preservative cause, in consequence of which the moisture of the fœtus will be gradually squeezed out, so to speak, just as is the case with dried plants. Cases of twins in which one becomes blighted; may terminate in several ways, just as we have seen that their death may result from different causes and occur at various times. Instances are sometimes, though rarely met with, in which one fœtus is blighted and aborted at once, while the other is retained and carried to the full time. Again the blighted fœtus may be retained for a considerable period subsequent to death, and be expelled either some time before, or contemporaneously with the other at any stage of utero-gestation. There is another variety still in which the blighted fœtus is retained for an indefinite period after the birth of its living twin-brother. Of course the longer the blighted fœtus is retained with its fellow, the more interesting and remarkable, *cæteris paribus*, does the case become.

The cases recorded by Ruysch, Mauriceau, Smellie, Denman, Ramsbotham, Montgomery, Rankine, and Siebold, illustrate one or other of these varieties, and although some of them are in many respects analogous to the one I have detailed, yet they differ from it in several essential particulars. Now it does not require much discrimination to see how at first sight one or other of these varieties may create unjust suspicion and reproach on the character of the patient and excite unmerited censure on the attending practitioner for supposed ignorance and neglect. When one fœtus is blighted and aborted, it is no easy matter to diagnose the existence of a second

¹ Principles of Obstetric Medicine and Surgery, p. 509.

tero; and it becomes equally difficult when the living child is first expelled to detect the presence of its blighted twin. These cases are fortunately, however, of rare occurrence.

Part Second.

REVIEWS.

DR WALSH. *On Diseases of the Lungs and Heart.* 2d Edition 1854. Small 8vo, pp. 797.

DR HUGHES' *Clinical Introduction to the Practice of Auscultation, etc.* 2d Edition. 1854. Small 8vo, pp. 302.

DR WEBER. *On Auscultation and Percussion.* Translated by Dr Cockle. 1854. 8vo, pp. 137.

DR HERBERT DAVIES' *Lectures on the Physical Diagnosis of the Diseases of the Lungs and Heart, etc.* 2d Ed. 1854. Small 8vo, pp. 364.

DR HUGHES BENNETT'S *Introduction to Clinical Medicine.* 2d Ed. Foolsap 8vo, pp. 134.

AN extraordinary impulse has been given of recent years to that branch of physical diagnosis which has for its object the detection of internal thoracic and abdominal diseases. No medical subject has of late been more extensively handled by our contemporaries. The list of works at the head of this article serves to illustrate this statement, and yet it seems only yesterday since physicians in high places positively ignored the stethoscope, stamped it as an improper novelty, and treated its revelations as untrue, and useless, and superfluous. Now, however, it has come to pass that we are disputing, not about the use of auscultation, not fighting to give the thing a local standing and a name in the domain of medical science, but disputing upon some of the finer shades and phases of one particular item in the long catalogue of facts which are included under the head of auscultation!

Yes! men are disputing, and auscultation has its schools! We are told of a German, and we hear of a French school; Laennec rules here, and his commentator Skoda there. The collision of opinions has taken place, and we cannot doubt that the end of the conflict will be more perfect methods in this branch of physical diagnosis, and clearer conceptions of the real value of physical signs.

This history of the stethoscope, had we time to indulge in a moment of reflection, might teach us a few useful lessons, which could, perchance, find their application at the present hour. When shall we learn to acknowledge, to a purpose, that dogmatism in

medicine—the practice of which is of necessity mostly empirical—is impossible, in a philosophic sense? When shall we learn to calculate coolly the *quantity* of those words, “I am convinced,” “I am sure,” etc., and reducing to their true value the facts, for instance, resulting from physical diagnosis, make the analysis of them purely scientific, and free them from all imaginary and preconceived admixtures?

Certainly, we may say, that if any one will cast a glance over the past and present history of the stethoscope and its revelations, he will, if he is a wise man, be careful for the future, at least, not to indulge in any extreme opinions affecting the matter of auscultation in *all* its bearings; he will be cautious in the reception of facts; he will be most cautious in his interpretation of the value of those facts, and cautious even in the practical application of his remedial art as guided by those facts. How many particulars are there concerning which “we have no earthly doubt,” whereon observers, equally as learned and capable as ourselves, doubt very considerably?

The number of works on the subject of auscultation, which have of late issued from the press, both in this country and on the continent (the supply indicating the demand), shows that medical observers generally are becoming daily more alive to the value of the subject; and the differences of opinion expressed in them prove how much of it yet remains doubtful and difficult. On this head we will just suggest to our readers, that perhaps that side of the matter which is most worthy of it has hitherto received the smallest degree of attention—we mean the valuation of the signs obtained, their actual worth and interpretation as exponents of disease, and as guides to treatment. It may be very useful for us to know the physical causes which produce bronchophony, but surely when we stand as physicians at the bedside of the patient it is of far greater import for us to know the diseased conditions on which, for instance, those fine crackling bubbles depend—whether they indicate bronchitis, or œdema, or pneumonia—whether the bronchophony heard depends upon pneumonia or results from pleurisy—than to be well informed as to the ins and outs of consonance, unison, resonance, and such like theories. It is of far greater value to know the meaning of a sound than to be disputing about its mode of formation, especially when we know that such sound may be possibly the indicator of very different conditions.

We have occasion to say but little respecting the works named above. Of Dr Walsh's we need do no more than announce the fact of the appearance of a second edition of it, one which he tells us, “has been carefully revised and much enlarged.” We can have no hesitation in saying that it contains the most complete and philosophic summary of the subject of physical diagnosis of diseases of the thorax which we possess in this country.

It is not so fitted for the hand of the student who is commencing the study of auscultation as the works of Dr H. Bennett and Dr Hughes; in these, the student will find himself more at home, and

we should certainly advise him to pick up all the information they can afford him before he betakes himself to the wide work of Dr Walshe, and dips into the sea of controversy it opens to him.

There is much plain and excellent advice given to the student in Dr Hughes' work, advice which we value much, for the reason that authors generally overlook too much the points which it is concerned about. As illustrative of this, we cannot forbear referring to one particular, and shall extract the passage where it occurs, as especially worthy of the practitioner's attention, and as giving a good idea of the clear head and good practical sense of its writer :—

"It cannot be too forcibly impressed on the mind of the student, and it may, therefore, be here again brought to his recollection, that there are few, very few, single signs, if, indeed, there be any, which are purely characteristic or pathognomonic of any particular disease. The more he observes for himself, and the more attentively he studies the products of disease, the more convinced will he become of this truth. Auscultation and its handmaid arts, often enable us, with almost perfect certainty, to predict various morbid changes, but of the exact nature, or rather of the cause of those changes, they tell us little or nothing. Physical signs merely indicate certain physical conditions, which may be produced by two, or by a variety of causes. The physical condition of the organs may be very generally detected by auscultation, and the other different modes of exploration treated of in this work; but information in respect to the morbid process by which it has been produced, must be sought for in other channels, and be determined by other means, as by the general symptoms of disease or by the history of the respective cases. Let the student ever recollect, and excuse us for repeating again and again the axiom which we are so anxious to impress upon his mind, that *physical signs are indicative only of certain physical conditions, and not of particular diseases.*" —P. 130.

There are some matters in Dr Hughes' work which we must recommend to his further consideration. Will Dr Hughes give us his *proof* of the statement that "*in simple venous congestion of the lung, . . . dulness on percussion exists,*" etc.? Has he one single fact which warrants such an assertion? Evidently the demonstration of it during life is almost, if not altogether, impossible; and, after death, has Dr Hughes ever met with a case of *simple venous congestion* where the percussion sound has been thereby altered? We refer to this matter because it is one of daily and practical import, and because we know it to be a very common belief with physicians, that simple congestion of a lung will produce dulness on percussion. Now we venture to deny the correctness of such an opinion, and for the reason that we cannot discover any alteration produced in the percussion sound of a lung after death, however congested the lung may be. If our position be correct, then the corollary which results from it is this, viz., we are not to conclude because the percussion over a lung is unchanged that the lung is not very highly congested, and, moreover, if the percussion of a lung be altered, then may we be certain that the change it has undergone indicates something more than that of simple congestion.

We also trust that our author will, on some future occasion,

reconsider the question of pectoriloquy and bronchophony, as here stated by him. We think, if he will carefully re-read what he has written on this point, he will admit that he has left the subject in a most complete embroglio. Is he not very hard in the following passage on the "recusant" who has the misfortune to differ from him, and does he think that such a statement as this is a lucid summary and clear exposition of facts?

"It must not, however, be supposed that there exists any defined line of demarcation between each, so that it can be said here ends pectoriloquy and here begins bronchophony. They all gradually merge into each other; they are not distinct species, but merely varieties of sound. So that what one auscultator may call imperfect pectoriloquy, another may denominate bronchophony. But to deny the distinction between well-marked pectoriloquy and bronchophony (I speak not of the indications afforded by them, but of the sounds themselves) appears to me tantamount on the part of the recusant to the acknowledgment that he has a bad ear, or is an indifferent auscultator."—P. 127.

Well, *here*, if Dr Hughes is correct, we must confess to a bad ear, and to a bad understanding, for we do deny the distinction Dr Hughes refers to, and perhaps we don't exactly understand his language; if Dr Hughes means to say that the sounds have no special indications, he evidently gives up the whole gist of the position—if he means that well-marked pectoriloquy is *not* characteristic, for instance, of a cavern (and he seems to do so), then it appears to us that all these distinctions become mere *jeux de mots*, and that pectoriloquy and bronchophony are but examples of the same sound, the sound being louder or weaker—and, if so, then certainly the sooner we get rid of one of these words the better. It has indeed been suggested, if we ever arrive at that degree of philosophic modesty which is contented not violently to strain conclusions out of facts, and but to receive just so much as, and no more than, such facts simply express, that pectoriloquy would be a more proper word than bronchophony to express the thing indicated thereby, viz., the voice sounding in the thorax; and there appears much reason in the suggestion, inasmuch as *pectoriloquy* merely indicates the fact, whereas *bronchophony* prejudices it, leading the mind naturally to the conclusion that its origin is necessarily in a bronchial tube, whereas it *might* be in a cavity. But we cannot here pursue this subject further; only this we will add, that it is just one of the many questions which, in the subject of auscultation, requires more complete elucidation, for however quietly practitioners in general submit to these words as indicators of certain conditions, according to the prescription of Laennec, it is certain that they are productive of the most complete confusion in the minds of the majority of us, if Laennec's views in this particular should turn out incorrect?

On the subject of percussion, we must linger for a moment. We wish we could say that the scientific side of this matter had been by some one of our authors laid down clearly, and practically applied to the art of auscultation. It is one which still demands elucida-

tion ; the principles of it are yet to be defined ; its indications to be truly interpreted. Why, it does not appear as though men agreed even as to the source of the sound elicited by percussion ! whether the sounds arise from the vibrations of the thoracic walls, or from those of the air within the thorax ! That the study is full of difficulty we readily admit, but we believe (and the justification of such belief is founded on general observation), that modern scientific researches have placed at the service of the physician certain particulars respecting percussion, which have not as yet been made available in the ordinary practice of this particular method of diagnosis, as fully as they profitably might be. And if certain particulars here referred to have been correctly stated, be really founded on facts, then it would seem to follow that some of the standard opinions of the day concerning the phenomena of percussion must undergo considerable modifications, and that some of them are indeed erroneous ; and if so, then necessarily result these important conclusions, viz. : that our diagnosis of disease, as derived through its aid, does not rest upon the secure basis it is generally assumed to stand upon ; and that observers, in short, do frequently deduce from the physical phenomena obtained by percussion of the body, conclusions which, measured by such new tests of experience, cannot be considered legitimate.

Whatever errors of this kind may have arisen, may be perhaps in part ascribed to this, that the practice of percussion, as a manual art, has been much too lightly esteemed ; that its difficulties have not been sufficiently impressed upon the minds of the student ; and that its phenomena have been laid down in much too exclusive, narrow, and dogmatic a manner.

Now, if any observer will place aside for a time the particular notions on percussion which he may have derived from his students' books, and will apply himself simply to a consideration of the signs or sounds produced by percussion of the body, which present themselves to his senses at the bedside of the patient ; and will compare them with the sounds obtained by percussion of the organs taken from the body after death, and which lay beneath the part percussed during life, and will carefully bring the diagnosis he had made during life into connection with the actual post-mortem condition of the organs which were the subjects of his consideration ; such observer will, we believe, admit the justice of the above conclusions ; he will find also, that percussion, as an art, is a much more difficult and complicated affair than it is generally supposed to be, that very considerable dexterity is required for its effective practice, and especially so in those fine manœuvres from which signs result, whose correct or incorrect appreciation affords important information, or leads to grave error.

If we consider for a moment a little closely the manual operation itself, we shall soon discover why it cannot be of that simplicity and facile execution, which an observer, who witnessed the matter-of-

fact, the off-hand way in which it is ordinarily practised, would deem it to be; we shall find, indeed, that the observation—i.e. the true and accurate appreciation—of the phenomena of percussion, must be an art of even more difficult attainment than that of auscultation. In percussion, the physician has two distinct facts to deal with, namely, 1st, the production of sound; and 2d, its acceptance by the senses. In auscultation, beyond the simple mechanical operation of adjusting duly the ear or the stethoscope to the part investigated, the observer is concerned only with the second of these facts, namely, the acceptance of the sound by which he seeks to judge of the condition of internal organs. The sound requires no art of his to produce it, it is there already, all he has to do is to seize upon it; and as it presents itself, to describe its characters.

Not so is it with percussion, for here, the sound heard, and which is to be reasoned upon and judged of, has to be produced by the observer himself; and we think we may fairly object to our textbooks in general, that they have not laid sufficient stress upon this particular item. It is, truly, one thing to rap the chest, and another to elicit that sound which faithfully represents (as far as percussion permits) the condition of its internal organs, and which essentially belongs to the part percussed. Writers have not sufficiently brought to the attention of the student the complicated nature of the agencies concerned in the production of percussion sounds, nor enforced strongly enough the extreme caution requisite in the practice of the art. The manual part of it requires considerable practice and must be dexterous, to be of use;¹ and when this is well performed, and the *true* effect produced, that true effect has still to be seized upon by the ear, and correctly seized upon; how difficult this is, may be gathered from the fact so continually witnessed, of the different effects the same percussion sound produces on the ears of different individuals, and from this also, that we ourselves have frequently occasion while examining a patient, to modify the opinion, which we had formed the minute before, of the nature of a percussion sound.

Perfectly true it may be, that there is little danger of even an unskilful observer mistaking the nature of percussion sounds when they present extreme characters; a full and clear sound is known

¹ Notwithstanding prejudice against its use, we maintain that the percussion-hammer, which Dr Hughes Bennett has so often forced upon the attention of medical men in this country, is a most valuable aid in the performance of percussion. It would be too long for us to state *all* its merits here, and this has been already done; all we can do is to advise objectors to *use* it, and if after having done so, and given the instrument a fair trial, they still proclaim it a superfluity, we shall be quite contented. Our own conviction of its utility arose in this way: Dr H. Bennett once placed a patient before us for examination in whom the thorax was unyielding; it required most strong percussion—painful to the patient, and I believe still more painful to the operator—to produce under the left clavicle a *Bruit de pôt féfé*—now here, the *gentlest tap* with the hammer on the pleximeter produced that sound in perfection. The fact was decisive—argument was superfluous.

t once, and there can be little mistake about the dull leaden lumpy sound of a thorax filled with fluid for instance; but then, let us remember, it is not in these extremes only, that the indications to be derived through percussion, are valuable; we want to catch the disease on its road—*morbis occurrere*—to stop it before it has arrived at the stage of injury, which that dull leaden sound indicates; and it is in this very attempt that so many lamentable errors are made, and chiefly in consequence of the improper appreciation of the facts of percussion which we are now speaking of. Is it, or is it not true, that simple congestion of the lungs (as Dr Hughes will have it) produces a change in the percussion sound? Now we must say, as far as our own experience goes, that most practitioners take the fact for certain, and yet let any one search this question, and he will be surprised at the little real grounds there are for belief in such an opinion! And is it not also true, that physicians for the most part detect a certain degree of dulness over those parts of the lungs where, by auscultation, and from general symptoms, they are able to surmise the existence of inflammation, even in its first stage? But the proof has yet to be given, that the percussion sound is duller than natural over a lung not merely highly congested, but even where a certain amount of effusion has taken place into its issue.

We do not think it necessary to illustrate here at length, what we cannot but look upon as the deceptions derived from percussion through our faulty method of observation; but we may just refer to a most ordinary one: what is more common, for instance, than to hear the physician say "oh! there is no dulness there, beneath that clavicle," and thence he concludes that the organ beneath is sound; but can any conclusion be more fatally erroneous? One would think this is a question—a matter of fact—which might be readily decided; and indeed it is such, for our best observers from Louis downwards, have admitted that tubercular matter may be deposited, and to a considerable amount, within the lung, and yet produce no change, appreciable by the ear at least, in the character of the percussion sound, and of the fact also any one may most readily obtain the demonstration by percussing comparatively a healthy lung and a lung through whose tissue tubercular matter is distributed in a certain form and manner. Well, does it not follow from this fact that it is unwarrantable to predict an absence of disease in such a case, because the percussion is natural?

We must confess, that when all the incidental complications of this subject of percussion present themselves to our mind, and when we recollect the little certain knowledge we possess of the principles on which its physical phenomena are founded, we are astonished at the unhesitating conclusions which practitioners draw from its use; how often is it, that they are contented with admitting from it neutral results, which tell no tale, and interfere not with their calculations of the problem they are attempting to solve, *i.e.* of the

nature of the disease they are investigating? No, percussion is *made* to speak, and in *every case* to guide opinions; we have no such word as *doubtful* in our language of auscultation; we make no allowance for the imperfection of our human organs.

In the present and actual state of our knowledge concerning the physical facts, which give rise to, and are concerned in, the phenomena of percussion, we must not be too minute in our division or multiplication of them; the problem, as we have stated, is a very complicated one, and has never yet been worked out. We may fairly anticipate, indeed, that it can never be displayed with all the precision of mathematical demonstration; the qualities and accidents of sound itself are many; and the particular materials, from which we obtain the phenomena of percussion, for the most part, numerous and ever varying. Consider for a moment the variety of facts included in the term percussion sound; we have to take account of its pitch, its intensity, its duration, and its timbre—phenomena the explanation of which are still obscure.

Again, remember, when we percuss the thorax, we are not beating an homogeneous sounding-board; but that numerous bodies, in health even, ever-varying in their sounding qualities, interfering and obstructing, or strengthening each other in some one or other of those particular qualities, are the subject-matters of our investigation.

Well, let these positive facts be kept still in view, and we predict that physicians will become more cautious in drawing conclusions as to the condition of internal parts of the human body, and of founding treatment upon such consideration whenever the results of percussion are not clear and manifest, that is, when the sound thereby obtained does not lie somewhere about or near the extremes of that long scale of phenomena which percussion elicits; the perfectly dull sound of a thorax filled with fluid, and the loud, clear sound of a pleura inflated with air are extreme examples of sounds, and as well-defined as any two most opposing qualities of matter; but mark, as we depart from these distant limits of the scale how soon our difficulties begin, and how they increase the farther we recede therefrom; we soon arrive at points where we should confess, if we were unprejudiced, that our powers of discrimination fail, where the most delicate ear is unable to appreciate distinctions upon which correct conclusions can be founded.

It is very unfortunate that the terms made use of by authors to express the phenomena of percussion should have no definite general meaning, and that there is no adopted standard by which their exact value may be measured; the liberty of language indulged in by writers leads to much misapprehension on the part of their readers. Take, for example, the word tympanitic sound; ask half-a-dozen of the first physicians you meet what they mean by that sound, and you will be surprised to find how little agreement there is among them, even in such a familiar instance! At one time it is made to represent what its immediate derivation would indicate,

viz., a drum-like, full sound; at another, and more frequently, it indicates an amphoric, or metallic-echoing sound, such as is often heard over the stomach and intestines; but how different are the conclusions to be derived from these different explanations! perfect health and most advanced disease! Some authors, again (Skoda, for instance), use this term continually, and yet never condescend to explain clearly what they mean by it!

We have dwelt longer than we had intended on this subject, but we have been led on from feeling its importance, and from the hope that our remarks may excite the attention of those who are anxious to enter on a field of observation which will assuredly yield them ample reward if it be well cultivated.

Dr Weber's work, well translated by Dr Cockle, need not detain us. The first part of it may be said, in short, to be a condensed exposition, and not a good one, of Skoda's views and doctrines on auscultation. The "consonating" theory of bronchophony is adopted, as if it were a fact established beyond all kind of doubt.

We have paid some attention to this theory of Skoda, and are bound to say, that there is some doubt whether the phenomenon described by him as such is really consonance of sound at all. The only case which appears to demonstrate the fact of the voice gaining increase of intensity as it passes through tubes surrounded by condensed pulmonary tissue, is that most rare one, *viz.*, where the voice is heard louder at some part of the circumference of the thorax than over the larynx, *i.e.*, nearer to its origin; but a rare phenomenon in the present obscure state of the subject of auscultation, can surely not justify us in drawing conclusions on which to base the explanation of an extensive series of facts, which, after all, *may be* capable of a very different explanation. Skoda's illustrations do not seem to us to be illustrations of consonance at all.

The second portion of the work is occupied with the organs of circulation; it is very interesting, but not fitted for the student's hand, being too physiological and controversial. The opinions of the Germans on this subject will be here found fully discussed.

Dr Davies' book on disease of the lungs and heart has reached a second edition, and its value has already been well acknowledged. It contains a full and clear account of the opinions of Skoda, Zehetmeyer, Kiwisch, Hamernyk, and other German authors, on the special subjects it treats of; there will also be found in it many original observations of the author. One new chapter has been introduced into this edition, *viz.*, a resumé of the morbid anatomy and corresponding physical signs of the diseases of the lungs; upon it Dr Davies has evidently bestowed much trouble.

On this chapter we must remark, that we object entirely to such method of teaching the meaning of physical signs, and we believe that there can be no more fertile source of error, than such a manner of dealing with them. If there are no *special* auscultatory signs

which indicate a given disease, why endeavour to indicate that disease by auscultatory signs? when those signs may be the representatives of half a dozen other diseases; and when we know how frequently it happens, that signs which are usually given as indications of this or that disease, are in reality entirely absent. Dr Davies, for instance, may tell us, "the physical signs of vomica are those which characterize softening and excavation of the pulmonary substance;" but let any one turn to his account of the characteristics of "softening and excavation," and he will soon find into what a bewildering labyrinth Dr Davies has led him. No! we maintain that this teaching of auscultation is radically wrong; of what avail is it, that such and such a disease is represented by such and such signs, when you tell us in the same breath, that the disease may exist, and yet every one of those symptoms be absent; such a defining of diseased processes reminds us of Fuller's definition of inflammation, and, to our idea, is just as ill-fitted to our present state of knowledge.

Dr Davies' volume, however, we have perused with much pleasure and profit. It is an excellent exposition of the opinions of the German school of auscultation, and these opinions have been fairly weighed and considered by the light of the author's own experience. He, like Dr Weber, adopts Skoda's theory of the consonating voice, but we cannot say we think he proves his case; rather the contrary. He tells us, that a sound consonates, for instance, in a bottle, then only when the columns of air in the bottle are of a given height; when more or less than a certain amount of water is poured into the bottle, the air within does not vibrate in unison with the body sounding without. Now if this be true, and we know it is in every case where consonance occurs, how comes it that the voice consonates on *all* occasions in bronchial tubes and cavities, when the requisite conditions, as described by Skoda, are present? how comes it, that in every instance, the walls of the tubes being hardened and the tubes themselves free and pervious, that the columns of air within them are just of that exact breadth and length requisite for consonance? This difficulty seems to us unanswerable, and decisively adverse to the general explanation of bronchophony by "consonance." At the same time, consonance may happily sometimes occur, and if so, why not in that rare case referred to above, where the voice is heard louder at some point of the thorax than at the larynx where it springs?

Two papers have lately appeared in the 6th vol. of Virchow's Archiv, by Dr Hoppe, and we cannot forbear speaking of them. We do so, because we believe that the method of criticism and inquiry there followed, is just the very method which will, if properly carried out, lead us to clearer conceptions of the phenomena of auscultation and percussion. We strongly recommend them to the careful consideration of those interested in these subjects.

We cannot conclude without a word upon the little volume which

stands last on the list of books at the head of this article. Dr Hughes Bennett has paid as much attention to the subjects treated of in it as any living man of his years, and has, therefore, every right to come before the students as their instructor. He has done so very successfully in this instance; the volume is replete with practical information, and the information is given in that form which will make it most useful and acceptable to the student.

Suggestions for the Future Provision of Criminal Lunatics. By CHARLES HOOD, M.D. London, 1854. 8vo. Pp. 174.

THE subject passed under review in the very excellent treatise of Dr Hood is one of increasing interest and importance. The legal provisions for the custody, care, and treatment of criminal lunatics have been hitherto confessedly most imperfect in this country, and little in accordance with the humane and enlightened principles which have been developed and brought into operation for the treatment of other classes of the insane. The most costly structures, with all the improvements in arrangement, heating and ventilation, which science can suggest, have been erected in almost every county in England for the pauper lunatics of the country. These asylums have been surrounded with beautiful pleasure grounds, gardens and farms, and furnished with every possible resource for the occupation, recreation, and amusement of the inmates, while the unfortunate sufferers from this malady who may have committed an act of homicide or assault in their frenzy, or who may have set fire to a hay rick, have been treated like common felons.

Until within the last few years Criminal Lunatics were kept in common gaols, and herded with the worst class of offenders, from whom they suffered every species of indignity and unkindness,—becoming the “objects of sport to their unfeeling fellow-prisoners, they were taunted, ridiculed and tormented by them, until, becoming irritated and goaded to desperation, they often committed the most shocking outrages.”

Instead of their insanity affording them any advantage over responsible criminals, it placed them in a far worse position, as was pointed out by Sir George O. Paul in his evidence before the parliamentary committee in 1807. He adduced two cases in proof of this: in one of which the accused party was acquitted of the charge of perjury on the ground of insanity, and the other of assault on the same plea. In both cases the ordinary punishment would not have exceeded imprisonment for one or two years, but those individuals being ordered to be kept in custody till His Majesty's pleasure was known (as insane), their punishment amounted virtually to imprisonment for life.

In consequence of the report of the select committee of that year, the House of Commons presented an address to His Majesty praying for the erection of a separate prison for Criminal Lunatics. This movement resulted, after eight years' delay, in the addition of wards for the reception of sixty criminal lunatics into Bethlehem Hospital; and afterwards, so late as 1849, in a contract with a private asylum, for the reception of twenty-four patients of this class. These steps have, however, been totally inadequate for the wants of the country, for it appears, from the reports of the Commissioners in Lunacy, that there were in 1852, 436 Criminal Lunatics in England, and, although they are no longer now confined in common gaols, they are distributed through various county asylums and hospitals, private houses, and Bethlehem.

This distribution of criminal lunatics in England, although a great improvement on their condition as compared with that when they were confined in gaols, has led to results of an opposite but very distressing kind. "We have in Bethlehem, in the criminal establishment," says Dr Hood, "patients whose insanity at the time of committing the offence, clearly absolved them from any responsibility; we have others whose insanity prevented them being arraigned upon the charges libelled against them, and who, therefore, cannot, never having been tried, be considered guilty; others, again, have, under the influence of insanity, committed petty misdemeanours; and many of the patients to whom I have just alluded, having partially recovered, conduct themselves rationally and quietly. Among them are persons of good family, officers in the army and navy, literary men and artists, members of the learned professions; and many of these educated persons feel it an extreme hardship to be obliged to associate with convicted felons, whose insanity has only darkened and exaggerated the more revolting features of their character."

If it is a hardship for criminal lunatics of different rank and character to be thus associated together, it is not less a hardship for *other lunatics* who have never committed any offence to be associated with criminals, some of whom have been convicted felons lying in gaol at the time of their becoming insane, and some of whom may have committed acts of homicide or murder. To send criminal lunatics to a general asylum is attended with the worst effects. Many of the inmates of asylums are extremely sensitive, and alive to all the ordinary feelings of humanity, and they feel deeply degraded and humiliated to find themselves placed in the same wards with persons such as those described. The moral effect of such an association upon patients labouring under melancholy, with feelings of remorse and self-condemnation, must, and is known to be, of the most injurious tendency. Such considerations as these led to the erection in Ireland some years ago of a central asylum for the criminal lunatics of all Ireland, at Dundrum near Dublin, and from the reports of the Irish Inspectors of Lunacy, there is every

reason to believe that it has answered the purpose for which it was rected extremely well.¹

In Scotland the evils arising from the association of criminal lunatics with the ordinary inmates of asylums, led the Superintendents of the public asylums there to refuse to receive them for many years ; it led also, about ten years ago, to efforts for the conversion of the eastern department of the Edinburgh Asylum into a Criminal Asylum for Scotland, but at that time the idea was abandoned in consequence of the impression that the number of such lunatics in the country was not so great as to justify such an expense as would be necessarily incurred. Since that period a department of the General Prison at Perth has been set apart for the reception of criminal lunatics, at least for such criminal lunatics as have been found insane upon trial, and there are now in that part of the prison about forty patients of this class. This is a much worse arrangement than that which exists even in England at the present time, where these patients have at least the advantages which an asylum affords for their treatment. It seems contrary to every principle of humanity, and to the enlightened spirit which has animated our legislators in regard to England and Ireland, that the unfortunate victims of insanity who have come under the cognizance of the law, should in Scotland continue to be treated like common prisoners, and sent along with every description of felon, to live within the gloomy precincts of a common prison. We trust the time is not far distant when some steps may be taken to remove this evil and raise this unfortunate class of sufferers to the same enjoyments and means of cure which they enjoy in Ireland, and in a great measure in England also.

In England there is reason to anticipate that, ere long, Government will legislate upon the alleged grievance. The Commissioners in Lunacy have repeatedly called the attention of the Lord Chancellor to the complaints which have been made against lunatic asylums being made receptacles for the class of patients denominated "criminal lunatics." They lately called for returns from all the English asylum superintendents as to the evils resulting from this association, and the objections made to it by patients and their friends. On the 18th of March 1852, the Earl of Shaftesbury in the House of Lords, moved an address to Her Majesty, "praying that Her Majesty would be graciously pleased to take into consideration the expediency of establishing a State Asylum for the care and custody of those who are denominated criminal lunatics." The Earl of Derby, then at the head of Her Majesty's government, admitted the evils complained of, and gave assurances that the subject would not be lost sight of by Government, and under this assurance the motion was withdrawn.

The next session the subject was again brought under the notice

¹ Sixth General Report of the District Criminal and Private Asylums in Ireland. Dublin, 1853.

of the House of Lords by Lord St Leonards, who contended that it "was absolutely necessary that provision should be made for the care and maintenance of Criminal Lunatics;" one asylum in the north and one in the south of England he thought would be sufficient. "It would," he said, "be particularly necessary to meet the case of women afflicted after their confinement, who in a state of aberration of mind, committed, he could not call it a crime, but a deed at which human nature shuddered. As the law now stood, when she ought to be in an hospital, and taken the greatest possible care of, she was treated as a criminal to be tried by the laws of her country." His lordship, furthermore, bore his testimony to having witnessed the ill effects of allowing criminal lunatics to be associated with other harmless unfortunates of the same description, and added, that "those who supposed that persons who had lost their mind were not deeply impressed with everything which affected moral conduct were entirely mistaken."

We have brought this subject thus fully before our readers in the hope that through the efforts of some of them, when the provision for criminal lunatics in England is again brought under discussion before parliament, something may be said or done to urge upon government the much stronger claims of Scotland on behalf of her criminal lunatics now immured in a common prison.

Dr Hood, whose opportunities for forming a sound opinion on the subject, from the position he occupies as physician to Bethlehem Hospital, are very favourable, has given in the work before us a very full view of the question as far as England is concerned, and some excellent practical suggestions as to the best mode of providing for this class of the insane.

We believe a great boon would be conferred upon Scotland if Government could still procure the old part of the Asylum at Morningside for a Criminal Asylum for Scotland. It is admirably adapted both by its internal arrangements and the advantages of the surrounding grounds and scenery for this purpose; and if it were conducted upon the same enlightened and benevolent principles as the present large pauper asylum there, the public of Scotland would have nothing left to wish for in regard to their so-called criminal lunatics. Being built originally for patients of a higher rank of life, its arrangements are such that many of the objections Dr Hood refers to as existing, against the mingling of ranks in Bethlehem Hospital, would be obviated, as patients of the better ranks could readily be associated together and provided with separate apartments suitable to their station in life, while the pleasure grounds, gardens and occupations and amusements could not fail to tend to recovery, and completely to remove from the minds of all that they were regarded as prisoners and criminals.

1 *Memoir on Strangulated Hernia, from Cases occurring in the London Hospital.* By NATHANIEL WARD, F.R.C.S., Assistant Surgeon to the London Hospital, and Demonstrator of Anatomy. 1854. 8vo. Pp. 33.

THIS little memoir is of practical importance, being founded on the observation of cases of hernia operated on during three years in the London Hospital. It consists first of a statistical analysis of 69 cases, viz., 43 femoral, 22 inguinal, and 4 umbilical; stating the side upon which the strangulation occurred, the period of strangulation, and contrasting the results of the cases in which the sac was opened, with those where the stricture was divided without opening the sac. This part of the memoir will be found useful, by enabling surgeons to form an opinion as to the value of the extra-peritoneal operation. At the same time we must keep in mind that when strangulation has existed for any length of time, or in other words, where there is any doubt as to the state of the strangulated intestine, the extra-peritoneal operation is inadmissible. It therefore excludes all the unfavourable cases, whilst the cases for which it is deemed proper, are just those early cases which would be likely to do well under any plan of operation. As to the great risks of peritonitis in hernial operations, we should recollect that it may arise from the injury inflicted on the peritoneal coat of the intestines by the tight constriction, independently of opening the sac, and we have seen fatal peritonitis from this cause in a case in which we performed the extra-peritoneal operation, and where strangulation had only existed for twenty-six hours. We do not make these remarks to disparage that method of operating, for we have often practised it with success, but we believe that the great point in operating for hernia is not so much the method of operating as *early operation*, and avoiding undue attempts at reduction by taxis.

Mr Ward next makes some remarks as to the seat of stricture in femoral hernia, and states that, in his opinion, the fibres known by the name of the ligament of Hey or Hesselbach are merely secondary impediments to reduction, and he considers that unless Poupart's or Gimbernat's ligament be also divided, the division of these fibres would not permit of reduction. We have had very considerable practical experience both in dissections of the parts and in operating for femoral hernia, and we have little hesitation in saying that division of Poupart's ligament to relieve the constriction is very seldom required, and if by the term Gimbernat's ligament be meant the aponeurotic fibres derived from the external oblique tendon as described by that anatomist, that will also seldom require division. The structures which immediately invest and constrict the hernial swelling, are the falciform process, forming the lower margin of the crural arch, and its crescentic margin, the latter corresponding to the base of Gimbernat's ligament. Higher up in the canal, the deep lamina of the falciform process continuous with the fascia

transversalis, and strengthened by aponeurotic fibres derived from the conjoined tendons (Hey's ligament) forms another common seat of stricture. Indeed, as Mr Liston showed long ago, in dissections where Poupart's ligament is fairly divided or dissected away, leaving these structures they can be demonstrated as a firm resisting arch forming the anterior and inner portion of the femoral ring in immediate contact with the hernia, and their division will generally suffice for relieving the constriction. In our own operations we have never required to divide Poupart's ligament except in a few cases, where, from the extremely matted condition of the sac and its contents, we have thought it safest to divide cautiously the lower portion of the external oblique tendon and the falciform process, so as to expose the peritoneal sac above the constricted and adherent parts.

The third, and what we consider the most important part of the memoir is the details of some peculiar and very interesting cases. After all, it is this sort of information which is so valuable to the surgeon, as preparing him by a knowledge of precedents for acting in similar emergencies, and those who have had the most practice in hernial cases will be the readiest to admit the great utility of studying such as those narrated by Mr Ward.

A System of Instruction in Quantitative Chemical Analysis. By Dr C. R. FRESENIUS, Professor of Chemistry and Natural Philosophy, Wiesbaden. Second edition. Edited by J. L. BULLOCK, F.C.S. 1854.

THE provisions now made in our chemical laboratories for the instruction of students in analytical chemistry, are such as to make us at times half regretfully wish that we had been born into this busy world a year or two later than it was our fortune to be. Not very long ago, when laboratory fees were four, and in some cases six times higher than they are now, a pupil found even in university laboratories no special arrangements made for conducting him through a systematic course of instruction in the art of chemical analysis. The pupil was taught analytical chemistry very much after the fashion that agricultural students are, or used to be taught farming. One day the farmer's ploughs were busy, and the youngster got a lesson in ploughing; the next day the threshing-mill was at work and he learned something of threshing; the third was market day, and he made his first attempt at bargaining; on the fourth sheep-shearing, or potato digging or pitting, or training a young colt, was the work in hand, and he helped to clip, or dig, or pit, or act as rough-rider, as the occasion demanded. Our laboratories were conducted after a similar fashion. To-day there was an excise case, and the pupil looked on whilst sugar was being sought for in tobacco, or pounded glass and sal-ammoniac in snuff.

To-morrow an ironstone was undergoing analysis, and he was privileged to powder it in a mortar. The third day there arrived the stomach of a murdered man, and the squeamish novice followed with mingled delight and disgust the steps of the process which was to convict the poisoner. The next day the professor had some mysterious researches in progress (which it was privately whispered among the assistants were to end in a great discovery), and the student was to consider himself favoured if he were permitted to make oxygen or chlorine for the professor. Good chemists were made by this process, but much time was lost, and the progress of the pupil was uncertain and irregular. We have heard of a letter written from a famous laboratory by a pupil who excused himself for epistolising from such a place, because he was "waiting for the platina crucible."

All this is now managed in another way, and to no one are we more indebted than to Fresenius for the admirable system on which well-appointed laboratories are now arranged. He is not a great discoverer, nor has he turned his immense analytical skill to account in the prosecution of researches; but he has done science the greatest service by simplifying, systematising, and carefully testing our methods of analysis, which can now be taught to pupils easily and continuously, so that each shall begin at the beginning, proceed at his own pace, and leisurely train himself.

As a laboratory guide in this training, Fresenius has published two works: the one a volume on *qualitative* analysis now in its third edition; the other a volume on *quantitative* analysis, of which the edition under notice is the second.

It needs no other commendation at our hands than that it is a greatly improved issue of the first edition, which, soon after its appearance, took its place among the highest authorities of our analytical laboratories. It is a sequel to the qualitative analysis, but, nevertheless, is an independent work which may be consulted by those who are ignorant of the former.

Wanderings among the Wild Flowers: How to See and how to Gather them. By SPENCER THOMSON, M.D., F.R.S.C.E. London, 1854. 12mo. Pp. 318.

WE have had much pleasure in perusing this little work, which is intended chiefly to interest the young in the study of botany, by directing them in the examination of the external forms and characters of plants. It will serve as a useful and pleasing accompaniment of botanical rambles, and may be the means of leading ultimately to a deeper and more scientific study of the vegetable kingdom. The whole subject is treated in a popular way, without the employment of many scientific terms.

The author gives in the first part of the work a brief notice of the organs of plants, the root, stem, leaves, and flowers. In the second he indicates the methods of classification, and points out the characters and properties of some of the more important British orders; while in the third part he gives monthly illustrations of British wild flowers. In recommending rambles in the fields, Dr Thomson takes a retrospect of the pleasant excursions he undertook in his early days with Professor Graham of the University of Edinburgh. He remarks, "The success of the Edinburgh system of botanical teaching is perhaps greatly owing, first, to the circumstance, that whilst the lecture-room is not neglected, neither are the fields, and the Saturday excursions give force and interest to the lessons of the past week; and, second, to the almost unrivalled opportunities for botanical explorations which exist in the environs of the beautiful capital of the North." We would call attention to the amusing account given of a trip in the immediate vicinity of Edinburgh in July 1835, at p. 188.

The author gives two short chapters on the economical and medicinal wild plants of Britain, which have been perhaps too much neglected in our anxiety for exotic productions.

The work is illustrated by numerous woodcuts. These, however, are not so distinct and clear as they might have been in these days, when wood-cutting and printing have attained such perfection. Many of the cuts are copied from Maout's work; and they fall far short of the original French woodcuts.

Upon the whole, we recommend the book as one which is calculated to interest the young student in the wild flowers which strew his path. The perusal of it will afford instruction and pleasure alike in the private chamber and in the fields. It is a work calculated to aid in the popular scientific movement of the present day. It is compiled by one who was in former years a zealous student of botany in Edinburgh, who still retains a love for natural science, and who finds that it furnishes a delightful source of recreation, even amidst the cares and anxieties of an arduous professional life.

Part Third.

PERISCOPE.

PHYSIOLOGY.

PROFESSORS VIALE AND LATINI OF ROME ON AMMONIA EVOLVED IN RESPIRATION.

After a series of delicate experiments, conducted with the greatest care and minuteness, these eminent chemists have arrived at the following conclusions:—

1st. That ammonia is evolved in each act of expiration. They consider that a healthy man expires, in 24 hours, 0.763 grammes of ammonia; and in

a year a quantity equal to 278·504 grammes of the gas. They calculate that in a city containing 160,000 inhabitants, 44·56 kilogrammes of ammonia are evolved by respiration in the course of a year.

2d. That the ammonia is given off in the form of a sub-carbonate.

3d. That the lungs cannot expire *pure* carbonic acid gas.

4th. That the *azole*, pronounced by chemists to be a product of respiration, is merely ammonia.

5th. That this ammonia keeps up the supply of the gas existing in the atmosphere, which, dissolved in rain, falls upon and enriches the earth.

6th. That the active principle of contagion is probably merely an ammoniacal salt. Their reasoning on this point is rather curious:—The lungs, skin, and excretions all give off a considerable quantity of ammonia, and contagious epidemics are generally developed where a large number of people are crowded together within a small space, or where the emanations of putrefying animal substances are rife. The latter emanations are always ammoniacal. Fumigations of chlorine, chloride of lime, nitric and acetic acids, are the antiseptics which decompose the ammoniacal salts, and probably also the contagious principle. Heavy rains, and tempestuous winds have been known to arrest the course of epidemics of contagious diseases. Viale and Latini believe that the ammoniacal salts which constitute the principle of contagion may be united either to carbonic acid, or to the metalloids (as sulphur, phosphorus, iodine, etc.), or to animal products, generating cyanogen, or to fatty bodies, fixed and volatile oils, or even to albumen, so as to act as a ferment.

7th. That during respiration a certain quantity of albumen is eliminated.

Professor F. Liebig, of Berlin, has published a letter in which he attaches great weight to the foregoing deductions, and considers the demonstration of ammonia in respired air as a fact of great scientific importance.—*L'Union Médicale*.

VIRCHOW ON THE STRUCTURE OF THE PLACENTA.

In two instances he has found in the uteri of puerperal women who had died soon after delivery, that the entire uterine mucous membrane is not necessarily separated on delivery. In these cases there was a raw surface at the seat of the placenta, whilst the remainder of the surface of the uterus still bore its mucous membrane (decidua). The decidua scrotina is nothing more than that portion of the uterine mucous membrane with which the ovum is in connection upon which it rests. The maternal placenta is clearly formed out of an hypertrophy of the uterine mucous membrane, and of vessels at first simple, and later of a cavernous ectasia of the vessels through the confluence of the dividing walls. In this last, the capillaries, and in part the arteries, but mainly the veins, contribute. Between the ectatic vessels, the tissue of the mucous membrane is, at a later period, for the most part atrophied. As to the structure of the villi, he finds nowhere in the decidua elements which quite correspond with those of the villus-epithelium. When it is, moreover, remembered that the same epithelial layer is present in extra-uterine gestation, it must, he says, be in the highest degree probable, that it is an integral component of the *fœtus*. The author gives as his conclusions, that we must admit that the *fœtal* villi not only *grow through* the decidua, but also through the maternal vessels, and that later the villi hang free and naked in the maternal blood. In all cases there is, as Schroeder van der Kolk and Goodsir have so much insisted upon, a great cell-layer between the maternal blood and the villi, which the materials which have to reach the *fœtal* blood pass through. This structure will naturally determine the exchange of materials, and, according to circumstances, may regulate or disturb it; and we may admit that these cells are not unlike the secretion-cells of glandular organs.—*Verhand. der Phys. Med. Gesell. zu Würzburg, and Brit. and For. Med. Chir. Rev.*

MEDICINE.

THE ASYLUM OF SAN BANDILIO IN SPAIN.

[We know little, perhaps too little, of the state of medicine and of medical affairs in Spain, and the prevailing impression tends to assign to both a very subordinate position in the world of science. We are well aware, however, that, amid the general depression, there are many distinguished men in the Peninsula, who struggle with praise-worthy zeal to maintain the dignity and advance the progress of medical science, in spite of the obstacles interposed by an inefficient government, and by institutions which have, too frequently, either lingered behind the age, or degenerated into complex systems of abuses. A pleasing glimpse of a better order of things appears to be presented in the following notice of a lunatic asylum, newly established in the vicinity of Barcelona, which we willingly transcribe, with a few slight omissions, from a Spanish medical journal recently forwarded to us.]

Lunatic Asylum of San Bandilio de Llobregat.—Under this designation, an asylum for the treatment of the insane has just been opened in the vicinity of Barcelona, through the persevering zeal of our colleague, D. Antonio Pujadas, who has constantly distinguished himself by his enlarged schemes for the behoof of suffering humanity. By him, among the rest, was conceived and executed the magnificent bathing establishment of La Puda; and it is he who now, in spite of a thousand obstacles, has offered to his country the first lunatic asylum, arranged with due reference to those improvements which distinguish the more celebrated of the like institutions in other countries.

The establishment is situated upon an elevated ground, screened by mountain ridges, and on a site which surmounts the town of San Bandilio de Llobregat, as well as the extensive district traversed by the river of the same name. The hygienic advantages it enjoys have been acknowledged by a commission appointed to examine it by his Excellency the governor of the province. The water is excellent, the air pure; and nothing can be more agreeable than the charming prospect extending over the course of the river, and the adjoining country. In the building, numerous compartments, surrounded by galleries and terraces, corridors, courts, and squares, admit of a separate management of the different classes of the insane, so as to have the patients arranged methodically, according to their mental conditions; a plan indispensable for their successful treatment.

In all the corridors there are fountains, and water circulates in abundance; the saloons and apartments are spacious, elegant, and well ventilated; the greater part of them have a beautiful prospect; some have cabinets for private servants, whom the higher class of patients may desire to have in attendance, when their state of mind permits the indulgence. In the departments of either sex are gardens and meadows, an orchard, and an avenue with quadruple rows of trees, where the patients may take exercise and amusement.

As bodily occupations are considered among the most powerful and efficacious means for the cure of mental diseases, the patients are accustomed to employ themselves, under the direction of a gardener and the attendants, in practical horticulture, for which purpose a proper piece of ground is set apart within the walls of the establishment. As to the female patients, a work room is provided for them, in which they engage in every description of needle-work, under the guidance of skilful superintendents. Nothing is neglected in order to keep the inmates in full occupation, that they may be enabled to pass the time without weariness, while in most other institutions it is allowed to hang heavily upon them. The chaplain of the house gives lessons in history, geography, and morals, with the view of apportioning among them recreation and instruction. Another instructor exercises them in music, and gives daily lessons on the piano to the patients who have cultivated that art. There is, besides, a library provided with useful and instructive books, and various

periodicals, so as to assist in passing a few hours profitably. Billiards, chess, draughts, and other games, contribute to the object of keeping the mind diverted. The picturesque environs serve for pedestrian exercise, in which all join daily who are able for the exertion. Once a week, there is an assembly in the concert-room, which continues for two hours, with play, music, and dancing.

In the departments of both sexes, are baths constructed with the most recent improvements, to aid as powerful therapeutic means in the cure of insanity. The church annexed to the edifice is kept open for worship, at which such of the patients as are permitted by their mental condition attend and join in the singing.

There are three classes of boarders. The first pay about L.90 yearly; the second L.62; and the third L.45.—*El Semanario Médico Español*, 1854, p. 67.

DRS MOREL AND MENESTREL ON GOITRE AND CRETINISM.

Dr Morel, chief physician to the lunatic asylum of Maréville, looks upon cretinism as a degeneration of the species due to a special action, exerted by an *intoxicating principle*, or deleterious miasma, upon the cerebro-spinal system, either through the medium of the air respired, or through that of the substances taken into the economy, and which is especially found existing in connection with soils in which the magnesian limestone predominates, although it cannot be absolutely affirmed that it is never found in connection with other geological formations. He has just published some most elaborate and interesting papers on the subject of cretinism and goitre in the *Annales Psychologiques*, and in the last one he prints a letter from Dr Menestrel, Mayor of Sérécourt, which contains some very interesting matters relative to the prophylaxis of these diseases in goitrous localities. Sérécourt is a village, near the small town of Darney, in the department of Vosges, between Haute Saône and Haute-Marne. Dr Menestrel says, that although it has never seemed to him fully proved that the use of water, impregnated with the salts of lime and magnesia, and containing no traces of iodine and bromine, could *alone* induce goitre; he has always felt quite convinced that the dolomitic salts exercised a special morbid action upon the thyroid gland, by causing hypertrophy of its structure. And he is of opinion that this deleterious action can be neutralised by means of the solvent properties of iodine, when the water used for drinking contains a small quantity of this metalloid. He also believes that continued atmospheric humidity, want of sufficient isolation, insalubrity of the dwelling-houses, and neglect of the laws of general public hygiene, are the causes which, in connection with the bad quality of the drinking waters, *necessarily give rise to goitre*, and render it endemic.

In Sérécourt there are 164 goitrous persons out of 736 inhabitants, that is 22 per cent. Of these 143 are females, whose sedentary lives, in unhealthy dwelling-houses, predisposed them to the disease. Sérécourt contains only one principal street, running from east to west, so that one-half of the houses are exposed to the north, and the other half to the south. Of the 164 cases of goitre in the village, 105 were in houses exposed to the north, and 59 only in those fronting the south; facts which are very significant.

Dr Menestrel has solicited the assistance of the state to enable him to carry out a series of experiments, on a large scale, on the whole members of the community, by making them use common salt, slightly impregnated with iodine; and he wishes to be authorized to place, in all the public wells, a small quantity of iodine, enclosed with a capsule of iron, constructed so as to allow of the water becoming slightly impregnated with that mineral. He anticipates immense benefit from these prophylactic measures.—*Annales Médico-Psychologiques*, Oct. 1854.

M. ARAN ON THE APPLICATION OF INTENSE COLD IN ECZEMA.

Victor Denis, a marble-cutter, was admitted into the *Hopital Saint-Antoine*, on the 2d June, having his upper and lower extremities covered with a painful

eczema. Huile de Cade ; collodion, medicated with tannin, iron, and iodine and soothing cataplasms ; all were tried without benefit. There was a considerable amount of inflammation, and an abundant discharge of matter when M. Aran resolved to have recourse to refrigerating applications. Two parts of pounded ice were added to one of common salt, and the mixture was put into a bladder, or between folded linen. The limbs of the patient were surrounded with this freezing mixture, and allowed to remain in contact with it from 10 to 15 minutes. When the application was discontinued, the reddened surface of the skin was found to be completely blanched. The skin was tense, hard, and quite insensible. On the removal of the freezing mixture, in order to prevent excessive reaction, the leg was ordered to be covered with compresses soaked, at first, in very cold water, and thereafter in water of the ordinary temperature. In spite of this precaution, reaction took place in the brief space of 15 minutes, at the end of which time the skin had resumed its natural appearance ; it thereafter became very hot, and was painfully itchy. The sensations, however, were of very short continuance ; they soon passed away and the limb became cooler, paler, and more comfortable than before the freezing application. No evil results of any kind followed. The congelation was repeated several times, at intervals of seven days, and the limbs were kept enveloped in bandages soaked with glycerine. By this treatment a complete cure was rapidly and satisfactorily established.

M. Aran has been in the habit of using refrigerating applications, with great success, for the treatment of various uterine affections ; especially acute inflammations of the uterine mucous membrane, granulations and erosions of the cervix, inflexions, etc.—*Gazette des Hopitaux*.

SULPHURIC ACID, VERSUS URINE.

Mr James E. Huxley has pointed out that in the wards of insane establishments which are wet and dirty, no scrubbing or scalding will effect more than a temporary improvement, as the floor becomes a reservoir for the perpetual exhalation of the volatile ammonia. But if dilute sulphuric acid be poured over such floors, in the proportion of one ounce of acid to twenty-four of water, and allowed to remain twenty-four hours, a white film (sulphate of ammonia) will form, which may be removed by washing, and will leave the room sweet. The principle is that of converting a volatile into a fixed salt that may easily be got rid of.—*Asylum Journal*.

M. LABÉ'S CASE OF SINGLE KIDNEY.

In an old man who died at the Bicêtre, M. Labé found the left kidney absent. There was neither a renal artery nor vein on the left side, and the bladder showed no trace of a left artery ever having existed. The right kidney, considerably hypertrophied, occupied its normal position ; while its ureter was single, and showed no bifurcation.—*Comptes Rendus de la Société de Biologie : and Gaz. des Hôp.* 7th November 1854.

SURGERY.

DISCUSSION ON THE CURABILITY OF CANCER, AND ITS DIAGNOSIS BY MEANS OF THE MICROSCOPE, AT THE FRENCH ACADEMY OF MEDICINE.

(Continued from page 555, vol. xix.)

Nov. 7, 1854.—M. VELPEAU.—At the commencement of this discussion I advanced two propositions ; 1st, That cancer was sometimes curable ; and 2d, That certain cases of cancer could be recognised without the aid of the microscope. To these questions I mean now to confine myself, seeing that the discussion has embraced too many topics for me to overtake ; and I intend also to state by what observations and researches I was led to form the opinions I then expressed.

I have said that the "cell" is not the specific element of cancer, as these gentlemen thought; and that there are epithelial cancers which contain no cells. L. Larrey must have spoken unthinkingly when he appealed to the unanimity of microscopists in this matter, for certain of them admit what others deny. Contrary to the opinion of Lebert and Broca, it is admitted by Mandl, Virchow, Bennett, Paget, and others, that there are epithelial cancers without cells. Microscopists are not all agreed, because cells do not exist in all cancers. There is some other thing in cancer of greater importance than the cell; and in the blood of patients having the cancerous cachexia there certainly exists something peculiar to the condition which we do not as yet know. The cell is one of the manifestations and material characteristics of cancer, but it is not the principle of the disease, or the malady itself. This cancer-cell has been found elsewhere than in cancers, and it existed in a non-cancerous portion of the vesical mucous membrane, lately exhibited to the *Société de Biologie*. It appears to me not to be a sufficiently constant characteristic to enable us to distinguish the different varieties of tumours, and therefore, I have tried to diagnose them by other signs. I have examined a very large number of cancers, and admit the occasional, but by no means the constant existence of the so-called cancer-cells. It may be said that the cases where the cells were absent, were not true cancers; be it so, I am willing to set them aside as doubtful. But I have seen tumours containing cells which were *not* cancerous. It has been asked whether I consider them to have been benign on account of their non-recurrence, and it has been objected that I cannot base my opinion on this character, if I admit that cancer is sometimes curable, and consequently does not constantly recur, but it is not from the non-recurrence of the tumours alone that I judge, but from the *tout ensemble* of their physical characters and clinical signs.

I affirm, then, that cancer-cells have been demonstrated in non-cancerous growths, and that they are often absent in true cancers.

M. Malgaigne has suggested that in these cases of cancer without cells I have mistaken fibro-plastic for schirrous tumours. This I have taken special care to avoid doing, and when I speak of cases of schirrus they were undoubtedly true cases of it. It has been said that the cells were not found, because the whole tumour was not examined. I have never attempted to puzzle the microscopists, and I have always given them the entire tumours for examination. Cancerous tumours, like other pathological specimens, are not equally diseased throughout their whole extent; there is generally a central portion in them in which the disease is most marked; but if we have a section made through this part it is just as good as if we had the entire tumour. If cancer-cells are not found in such a section they are certain not to exist elsewhere, I have cited cases of tumours, which contained no cancer-cells at first, recurring afterwards, and exhibiting these cells in their structure. I have seen two or three such cases. In one of these a man had a cancerous tumour, a portion of which was removed and sent to M. Lebert for examination. Finding no cells in it, he pronounced it to be benign. M. Richet therefore operated on the patient; things went well at first, and the cicatrix looked healthy; but ere long the tumour returned and killed the patient. The second tumour contained cancer-cells. Again, a woman had a mass of vegetations on the *left* breast which resembled neither encephaloma, schirrus, nor fungus. I removed one of these vegetations and found that it contained no cancer-cells; but I was not therefore convinced that it was not a cancer. The woman returned after some time, having the *right* breast affected with all the most marked characters of cancer—partial indurations, and retraction of the nipple. She soon died of the affection. M. Robin found cancer-cells in the tumour of the right breast, and also numerous nodules, in the lungs and liver, containing cancer-cells. Can we say that in this case the tumour of the right side was a cancer, while that of the left was non-malignant? Certainly not. Moreover, the cells were found on the side on which the tumour was least degenerated. M. Leblanc has at-

tempted to explain these differences by admitting the existence of "mixed tumours;" and he believes that cancer may become developed in a tumour not primarily malignant. Microscopists, however, deny the transformations of tumours. I myself am doubtful as to whether benign tumours may degenerate, and I would not be understood either to affirm or to deny it; for facts have led me to believe that such a change may be quite possible.

It appears to me, then, 1st, *that there are cells without cancer*, and 2d, *that there are cancers without cells*; and if these facts be once admitted they go very far to negative the assertions of the microscopists. A new definition of "cancer" is asked, but the remodelling of definitions is a barren and unprofitable work. It is far easier to attack an old definition than to make a new one, even when it relates to something we understand very well. For example, I should be puzzled to define M. Robert with exactitude, yet I can quite well recognise him. I would characterise cancer as a malady, which may appear as an excrescence, a tumour, an ulcer, or a fungosity; the specific nature of which is, when once established, to spread, to invade diseased tissues, and, finally, to cause death.

Let us look now at the tumours which the microscopists refuse to admit as cancers. They deny that epithelial tumours are cancerous, because their tissues are homeomorphous, and such a structure, they say, cannot be malignant. They allege that these tumours do not recur, or if they do, that they affect only the original spot, but that they do not affect the lymphatics, or become generalised. Every-day experience, however, shows that these tumours may recur, not only on the original site, but also in the glands; and that their recurrence, far from being exceptional, is very frequent. Hence M. Lebert and other microscopists have considerably modified their first opinion. Yet they affirm that epithelial tumours never, like cancer, become generalised. Now, M. Robert himself has adduced instances of their generalisation; and others are recorded by Paget, Virchow, and Rokitanaky, in which they become developed in the heart, lungs, and liver. Yet this, say they, happens very rarely. I do not know; but whether it occurs often or seldom does not at all alter the question. We remove a small tumour of this sort from the lip; at the end of two, three, six, or twelve months, we find that it returns; again we excise it, and again it recurs. Finally, we dare not touch it; and it kills the patient as surely as cancer. Wherein lies the difference between them?

Again as to fibro-plastic tumours. To constitute them a distinct group from cancers, the microscopists have been obliged to class with them many tumours to which they have no analogy. How can fibro-plastic tumours, which prove fatal to the patients, be confounded with simple glandular hypertrophies? and in what respects do they resemble each other? Although the anatomical structure of both may be similar, there doubtless exists in them some unknown element which constitutes their essential difference. M. Lebert has said that these tumours like the epithelial, do not recur, or if they do, that it is only locally; but I have shown that gentleman many instances of their reproduction in the viscera. M. Robert has cited cases to prove the non-recurrence of fibro-plastic tumours, but—so great is the blindness which an adherence to a preconceived idea occasions—all these cases occurred before the use of the microscope. For thirty years I have constantly applied the great law of Bichat—cited by M. Robert—to every thing in pathological anatomy. But, in pathology, anatomy, though very necessary, is not all-important; for we must also take into consideration the oftentimes obscure nature and causes of disease. There exists in cancer something special, something not anatomical, which has hitherto escaped all the investigation of our senses, and of which the microscopists have taken no note.

Here, then, is my basis for the distinction of tumours. *Cancerous tumours all possess one character in common:—they have all a tendency to destroy the tissues by becoming generalised.* All that can be said against this proposition lacks demonstration, and proves nothing.

I turn now to the question of its *curability*: and the first difficulty that presents itself is *diagnosis*. And first of all I would remark that it has been said that I lay pretensions to infallibility of diagnosis, and that I boast I am never deceived. God knows that I lay claim to no such lofty pretensions. I have merely said that there are certain tumours, whose clinical characters are so well marked, that it is impossible for *any one* to be deceived. I have taken notes for the past year of 120 cases of tumours; 66 of which were cancers. Among these 66 cases I have removed 49 cancerous mammae; in every case the extirpated tumours were examined by the microscope, and in every instance the microscope confirmed the diagnosis I had made before the operation. Let me add, however, that I have never alleged that a correct diagnosis can *always* be made. When I say that in certain cases I cannot be deceived, I allude to those in which the characters are so well marked that any surgeon could recognise them.

I come now to the important point:—Why may not cancer be curable? When I advanced facts supporting the curability of cancer, it was said that they were inconclusive, because they were anterior to the use of the microscope. But when, in the 49 cases I have mentioned, which were microscopically examined, my diagnosis was in no instance wrong, how can it be said that I was deceived before the introduction of the instrument? As regards my statistics it has been said that 20 cures in 50 cases is too good to be true; but I have not alleged that I cured 20 out of 50 patients, but out of 200, one half of whom died. (Here M. Velpeau cited several cases of persons who were cured of cancer—that is to say, who never had any recurrence of the disease for a greater or less number of years after the extirpation of a cancer—cases which were almost all witnessed and examined by the microscopists). These facts, said he, are authentic; and the curability of cancer cannot be denied. M. Robert has alluded to the case of a lady operated on in 1850; but he has not observed that in detailing this case I made reservations in respect to it.

As to relapses occurring long after the operation, as after ten, fifteen, or twenty years, may we not inquire whether or not it is possible that cancer, like other diseases, may become twice manifested in the same individual, without this being looked upon as a recurrence? I am always asked why we are unwilling that cancer should be curable?—and what interest microscopists have in maintaining that it is so? I have never been able to understand it. Statistics and the authority of highly respectable names—as Munro, Boyer, etc.,—have been cited in support of the incurability of cancer. But the statistics of Munro and Boyer are of great value; those of Hill and Flajani do not militate against mine; and as to the recent ones of MM. Lebert and Broca they do not appear to me a whit more conclusive, as many of the patients enumerated in them were entirely lost sight of.

What we seek here is truth; but we should not make the obtaining of it depend upon a mere theoretical opinion; since theory alone has given rise to the unbelief in the curability of cancer, which I so much deplore. As to the microscope, I maintain that it has done good service to science; I believe that it will render still more; but I am confident that it has also committed several errors. I accept all the new facts which it has revealed to us; and I only contest the false or doubtful explanations which have been given to them. And as regards the question at present under debate, I think that its resolution should be made with great caution and reserve, inasmuch as there probably exists some special element with which we are not yet acquainted. I have ever encouraged, and I will continue to encourage fresh researches; but I will only accept as true what I find to be satisfactorily demonstrated. I hail the march of progress with unmixed delight, so long as the car of science is propelled, but is not overturned.

Nov. 14, 1854.—The secretary read the following letter, addressed by M. Mandl, to the president.

M. Mandl's Letter.

"Mr President.—M. Velpéau in his last address, has done me the honour to class me among the microscopists who do not admit the existence of characteristic cancer-cells. I beg to say a few words to the Academy on my reasons for so thinking:—

"Shortly after Müller, in 1839, discovered cells in cancerous tumours, I published my observations in the *Archives de Médecine*. But in 1843, I owned that the sanguine hopes which I had at first formed of the advantages to arise from the employment of the microscope in the examination of such pathological specimens, had not been realized; and my researches since then have confirmed this opinion. I had soon abundant opportunities of examining tumours which had appeared to clinical observers to be cancerous, from their origin, progress, and fatal termination, but which contained none of the cells considered by Lebert to be characteristic of the malady. I can remember cases of epithelial and fibro-plastic tumours, but I would more particularly direct attention to cancers of the retina. In four out of five tumours of this last kind, extirpated by Sichel and Bérard, it was impossible to discover any specific cell; and yet subsequent general infection left no doubt as to their malignant character.

"On the other hand, in the cells found in cancerous tumours, I have not observed the characters which they are said to present in the majority of cases. According to M. Lebert, the nucleus of the cancer-cell has always a mean diameter of, from the $\frac{1}{1000}$ th to the $\frac{1}{1000}$ th of a millimètre. (*On Cancer*, p. 30). Yet in encephaloid tumours, I have found the nuclei not above the $\frac{1}{1000}$ th or the $\frac{1}{1000}$ th of a millimètre; and in such cases it is impossible to distinguish them from the other nuclei of normal tissues. Again, as to nucleoli, I have often found their characters very imperfectly marked. In M. Lebert's work we find examples of these inferior dimensions in cancers of the bones, the stomach, etc. As good, and even better observers than M. Lebert, believe that it is necessary to take into consideration all the elements, and also to know the history of the disease before pronouncing a judgment upon its nature. Does not this show that the cell is not sufficiently characteristic of cancer?

"The truth of this is proved, not only by observation, but also by the laws governing histogenesis. It seems to me that, in order to solve the problem which we are at present studying, it is indispensably necessary that we engage in the comparative study of normal histology, and especially in the study of the development of the tissues. Space precludes me from entering into any detailed explanation of development, as so far as it regards this subject; but I shall briefly state the principal results of my researches.

"All the tissues of the organism form two grand series; the one composed of cells and scales (*ou lamelles*), the other of fibres: and this essential distinction characterizes the different elements from their very first appearance. Nuclei placed in a soft amorphous substance, became surrounded, in the first instance, by membranes; they increase in size, multiply, and constitute the cells, of which the glandular, epithelial, and other tissues are composed. In the second class, on the other hand, the nuclei are placed in an amorphous, homogeneous, solid substance which splits, and goes on subdividing into more and more delicate fibres, so as eventually to constitute a fibrillated tissue. There is no cell-formation in all this series of organic elements, among which may be classed the fibrous, cellular, and serous tissues, etc.

"This principle of development and of texture, inherent in each series of the tissues, is rigorously maintained in all their various physiological and pathological phases. In the normal renovation which accompanies secretion, growth, etc., as well as in anomalous renovation or in regeneration, we always observe that cells are developed in those places where cells originally existed, and that fibres are developed in the other tissues. Although hypertrophy may occur, the same principle continues to preside over the production of the new elements; fibres do not produce cells, nor cells fibres, but cells are always developed in the midst of cells, and fibres in the midst of fibres.

"Facts have proved to me that these principles hold good in the case of cancer. The cancerous diathesis may affect the blastema, which ought to produce either cells in the glands, scales in the epithelial, or fibres in the fibrous tissues; and according to the nature of the tissue infected by the diathesis, the morbid blastema will produce either cells or fibres. The product will then be characterized according to the species of the affected blastema; and we will have either cancer (*cancer à cellules*), fibro-plastic tumours (*cancer à fibres*), or cancrioid growths (*cancer à lamelles*). But if it be permitted to us in most, though not in all cases, to recognise the alteration produced by the diathesis in the cells—that is to say, if we are allowed to recognise the cancerous cells, and to distinguish them from other analogous elements—why may we not also take into account *fibres* and *scales* as elements of cancerous tumours? So long as we are unaware in what respect fibres and scales are altered when they are produced under the influence of the cancerous diathesis, so long will the microscope continue to give imperfect information to the surgeon in the diagnosis of tumours.

"As to the frequency and gravity of recurrences, all histologists know that there is no element which is increased, developed, multiplied and regenerated, with so much facility and rapidity as the cell. Fibres are much more tardy in the stages of their development. Now, in which of the varieties of cancer are the recurrences the most frequent and the most serious? Which species extends most rapidly, invades neighbouring organs most speedily, and destroys most energetically the whole economy? Undoubtedly it is the *cancer with cells* (*cancer à cellules*). The conditions are completely changed when the cancerous diathesis affect a blastema which ought to produce fibres; and when we see it form incompletely developed fibres, we call it the *fibro-plastic element*. Fibres, in general, are developed very slowly. I have seen a regenerated tendon presenting incomplete fibres at the end of seven years—that is to say, there was a blastema in which fibrillar division had not been completed. Thus the microscope is capable of informing the practitioner of the severity of the affection, and it also enables him to pronounce a proper prognosis as to its course, and as to the greater or less probability of its recurrence, inasmuch as it acquaints him exactly with the nature of the elements which constitute the tumour.

"I believe, then, that cancer, capable of occasioning general infection, can exist without the cancerous cell—that heteromorphous element which has been considered to be a peculiar and specific product of the diathesis—being present. This belief of mine has been long ago expressed by M. Velpeau, as well as by Bennett, Paget, Virchow, Vogel, and others.

"The cancerous diathesis may affect cells, scales (*lamelles*), and fibres, and accordingly there are as many different varieties, yet microscopical anatomy agrees with clinical observation in classing them together as one family."

M. AMUSSAT felt confident that he had cured many very serious cases of cancer, which otherwise must have terminated in a painful and horrible death. The influence of grief appeared to him to be, speaking generally, the most frequent cause of cancer in persons of otherwise good constitution; hence he was led to believe that the seat of cancer lay in the nervous system. He trusted that this discussion on the microscopical characters of cancer would elicit fresh researches, which might elucidate the nature of the disease, and lay the foundation for a rational, efficacious treatment of it. He thought the question of the greatest moment in relation to cancer was, whether or not there existed any hereditary taint. If this was present, the most prompt and energetic measures were necessary. When it was resolved to operate, either by means of the knife or caustic, we should freely destroy the surrounding tissues, as the timidity of the operator often occasioned recurrences. When caustics are preferred to the knife, only the strongest should be used.

M. CLOQUET. Cancer was like inflammation—there were many very different varieties of it, yet all its various forms possessed certain common

characters: they never healed spontaneously; they were very prone to recur, and, unless extirpated, they infallibly occasioned death. He considered that the microscopists had not formed a separate school of their own—they belonged to the anatomico-pathological school, and, being possessed of more powerful means of investigation, they had arrived at most important results. To ensure our obtaining still more certain and important results, it was necessary that all medical men should unite in strengthening the hands of the microscopists, and in placing within their reach everything which they considered likely to advance the interests of science.

M. DELAFOND proposed to examine the question of cancer in relation to comparative pathology, to surgery, and to micrography.

It had been alleged by M. Leblanc that herbivorous animals were less affected by cancer than carnivorous; but this proposition was too wide; and he believed that the herbivora were equally subject to its ravages. It was by no means rare to find cancer affecting the teats of mares and the testicles of horses, and it often occurred in the viscera of these animals. Asses and mules were very subject to it. Cancer was common among oxen—especially affecting the jaw. Pigs were very liable to cancer of the teats, and dogs were often affected by this disease. It also occurred among gallinaceous fowls, and many varieties of birds. Therefore it did not appear that animals deriving their sustenance from the vegetable were less disposed to cancer than those whose nourishment was drawn from the animal kingdom.

He had found, from microscopical investigations, that fibro-plastic tissue recurred like cancer. He did not consider the cell characteristic of cancer. The cancer-cell varied in its appearance according to the stage of the malady. At first, the cells found in a small tumour, not compressed by the surrounding tissues, were of a rounded form, having a nuclei and nucleoli in their centres. Compression subsequently rendered them elongated and ovoid; subsequently they became still more elongated; and in schirrus cancers compression deformed them still further. In old cancers they become infiltrated with calcareous matters; their nuclei become flattened, and the cells become thin, like fibro-plastic tissue. Secondary cells also become developed within the parent cells, both being enclosed within a common membrane. In encephaloma, where there existed abundance of liquids and fatty matters, the cells were rounded or ovular.

He had read that a German pathologist had been able to produce cancer by injecting the juice of a cancerous tumour into the veins of a healthy dog. He had several times tried this, but had never been able to obtain any such successful results, although he had injected cancerous juice *containing abundance of cells* into the veins, and had put it into the trachea, and had also inserted it beneath the integuments of animals. The characteristic element of cancer was not the cell, but *the juice* in which it floated. Inoculation with the pus of glanders produced glanders; that of the varioloid disease of sheep gave rise to the same disease; and the saliva of the hydrophobic dog occasioned canine rabies; yet the pus and the saliva in these diseases presented no special characters when microscopically examined. Thus there existed different properties with identical appearances.

As regarded the *dimensions of the cells*, he considered that the cells of fibro-plastic tissue were the smallest; those of encephaloma the largest; while those of schirrus were of a medium size, between that of the other two. The appearance of the nuclei and nucleoli varied very much, according to the variety of cancer examined;—they were very small in fibro-plastic growths. These facts accorded well with clinical results; for it was known that encephaloid tumours grew more rapidly, spread more certainly, and produced general infection more speedily, than any other variety of cancer.

He described the effects of various reagents on the cancer-cell, and showed the importance of acetic acid in rendering more distinct the nucleus and nucleolus.

He eloquently defended the use of the microscope, and maintained that it had great claims to the attention of all practical men, because if by means of it they perceived the cells which ordinarily accompanied cancer, their diagnosis became more certain, and they knew that they had to deal with either fibro-lastic, schirrous, or encephaloid tissue.

As regarded the treatment of cancer, he recommended early extirpation. He had found that, after the removal of a cancer, there were a great many of he—he would not say characteristic, but *habitual* cells of cancer, disseminated through all the normal tissues near the spot affected, and also through those which were at some distance from it. Hence, in excising cancers, we ought to cut away a considerable portion of the surrounding tissues, even though they appeared healthy and unchanged, and that we ought even to cauterise them afterwards. By microscopically examining the discharges from wounds left by operations for the excision of cancer, he had ascertained that, so long as the pus contained cancer-cells mixed with the pus cells, that cicatrization did not take place; and that, simultaneously with the disappearance of the cancer-cells, the process of reunion commenced. He was therefore of opinion that, after removing a cancer, we should allow the wound to suppurate for a certain time, in order to allow the cancer-cells to be eliminated by this channel.

[This discussion continues to drag its slow length along. That the elder practitioners would be forced some day seriously to consider the labours of histologists was to be expected, but it was scarcely anticipated that they would have spoken at such length about what they avowedly do not understand. The important problems connected with the pathology of cancer, are no more likely to be solved by words and long speeches, than they were capable of being crushed by sneers and ridicule. The facts of M. Velpeau, however, are to the histologist of extreme value; and the observations of M. Delafond on the lower animals, are useful. M. Mandl is the only one who has brought forward a new idea, during the whole discussion.]

MIDWIFERY.

MAUER ON THE EFFECT OF LABOUR PAINS ON THE PULSE.

This author made a series of interesting observations on 100 women, in Professor Martin's Clinique, in order to verify Professor Hohl's statements concerning the increased frequency of the pulse, occasioned by the contractions of the parturient uterus. In these researches he was assisted by a friend; and one of the observers devoted his attention to the radial pulse, while the other studied that existing in the uterus. To feel the contractions, and judge of their force, one finger of the observer was inserted into the cervix, while the other was placed upon the abdomen.

Mauer's researches have convinced him that, at each pain, the pulse is first accelerated, and then diminished in frequency; that it speedily thereafter becomes normal, and remains so until the next uterine contraction. Its increase of frequency varies at different periods of the labour; and Mauer found that, in the first four of the five stages into which Naegele divides labour, the acceleration went on progressively increasing, but that it was sensibly diminished in the last or fifth stage. The time during which the pulse remains increased varies according to the intensity of the pain; and the frequency of the pulse during the intervals between the pains becomes increased as the labour advances. The length of the interval between the pains is in inverse ratio to the time during which the rhythm of the pulse is altered. It has already been observed that the length of the intervals diminishes gradually as the labour proceeds.—(*Hohl, Explor. Obstet. Halle, 1833, p. 36.*)

When the pains are feeble or convulsive, these phenomena are not observed to occur with regularity. The cries, etc., of the patient have a great influence

over the number of the arterial pulsations. Excitants—as ipecacuanha, sinapisms, and heat—increase the pulse, but only during the intervals, not during the pains. Ergot, however, accelerates the pulse, even during the pains. Although, under the influence of chloroform, the heart's action becomes somewhat slackened, the pulse remains as frequent during the uterine contractions as during the intervals between the pains. The more regularly the acceleration of the pulse, and its subsequent return to its normal rhythm, take place, the more perfect and efficient are the labour pains.

The force of the pulse is usually decreased during the contractions of the uterus.

Mauer does not consider these modifications in the frequency of the pulse attributable to muscular contraction. It cannot be explained by modifications of the respiration; for although Weber has shown that deep inspirations produce analogous effects, Mauer has satisfied himself that an increased amount of respiratory action was not always coincident with these phenomena, which were so regular in their occurrence in the arteries. Nor can an impediment to the circulation—compression of the uterine vessels—reacting on the heart—be considered the cause of the phenomenon. Mauer believes the acceleration of the pulse to be due to excitation of one of the nervous centres; and he believes that the great sympathetic is the seat of this special action.—*Archiv. für Physiol. Heilkunde.*

WEBER'S CASE OF FŒTUS IN FŒTU.

M. Weber relates a case which he says is peculiarly interesting from the fact that the tumour containing the fœtus was successfully removed by operation, which had never occurred before.

The tumour, which was soft, immobile, firmly adherent, and as large as a child's head, was situated upon the sacrum, and it increased in size so much that nine weeks after its first appearance it was resolved to resort to the hazardous operation of removing it. It was found to contain two well formed fingers. A ligature was placed round it, and it was removed without much hemorrhage. The wound was perfectly cicatrised in a month after the operation.

Each of the fingers within the tumour contained three phalanges and a perfect nail, and they were articulated to a bony mass (produced probably by the union of the metacarpal bones), and also to some smaller semi-cartilaginous pieces of bones. The greater portion of the tumour consisted of fatty matters, and of a cyst which contained a yellowish fluid, as well as blood-globules and the rudiments of epithelial tissues.—*Virchow's Archiv. für. Pathol. und Physiol., Bd. vi., Hft. 4, 1854.*

PORCHAT AND HESCHL ON DEVIATIONS OF THE UTERUS IN CHILDHOOD.

M. Porchat is "interne" to the Parisian Foundling Hospital, and has had many opportunities of examining the bodies of young female children dying in that institution. His observations were chiefly made upon children about two years old, and often younger. He found that ante flexion of the uterus existed in most cases; and hence he concludes, with Boulard, that this is the normal direction of the organ in childhood. He has also observed the organ retroflected, which condition he considers due to an abnormal deviation of the rectum. He does not regard flexion of the uterus in children as dependent upon intestinal distension from flatus, or upon any cadaveric alternations; but thinks that the tissue of the organ is so soft at this period of life, that it has an inherent disposition to become doubled upon itself.—*Comptes Rendus of Société de Biologie. August 1854. And Gaz. Med. de Paris. Nov. 25, 1854.*

At a meeting of the Viennese Imperial Academy of Medicine, held on the 3d February 1854, Dr Heschel said, that he had never seen deviation or dis-

placement of the uterus in the autopsies of young virgin girls when the bladder was full. On the contrary, he had remarked its frequency when the bladder was empty.—*Zeitsch. der K. K. Gesells. der Aerzte zu Wien. March and April. P. 365.*

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

December 6, 1854.—J. Y. SIMPSON, M.D., President, in the Chair.

ANEURISM OF THE SUBCLAVIAN ARTERY WITH CONTRACTION OF THE PUPIL. BY W. T. GAIRDNER, M.D.—The patient, a middle-aged man, was shown to the Society. *Dr Gairdner* remarked that he had no doubt that the aneurism involved the inner third of the left subclavian artery; possibly also the adjoining portion of the aortic arch. The interesting fact connected with the case was the contraction of the pupil on the side corresponding to the aneurism. This was not an accidental coincidence, for the recent experiments of Budge and Waller showed that the pupil was under three distinct sources of nervous influence. The circular fibres which contract the pupil are supplied from the third nerve (oculomotor) and the fifth (trigeminus). The radiating fibres which dilate the pupil are supplied, according to Budge and Waller, by sympathetic filaments which pass from the ganglia of the neck and upper part of the thorax upwards into the skull and join the ophthalmic branch of the fifth (trigeminus) after it passes the Casserian ganglion. Hence stimulating the sympathetic in the neck dilates the pupil, and cutting it causes permanent contraction of the pupil. These experiments further show that the whole of the sympathetic fibres which go to the pupil from the cervical ganglion are originally derived from the spinal cord in the lower cervical and upper dorsal region. If the cord be destroyed in this region, or if the emerging spinal nerves be cut near their roots, the effect on the pupil is the same as if the sympathetic nerve or its ganglia in the neck were destroyed, i.e., permanent contraction of the pupil follows. The portion of the cord involved in the motions of the iris is called by Budge and Waller the *regio cilio spinalis*. It extends from the fifth cervical to the sixth dorsal vertebra.

Mr Spence said that while the contraction of the pupil after division of the sympathetic in the neck had been very generally noticed in the course of *Dr John Reid's* experiments, yet from the numerous sources of fallacy, he knew that *Dr Reid* at the time did not wish to attach undue importance to this apparently casual sequence.

Dr Struthers remarked that *Dr Reid's* experiments showed that compression or division of the sympathetic in the upper part of the neck in the cat and dog had an immediate effect on the pupil, causing its contraction. Whatever the explanation might be, there could be no doubt as to the fact. *Dr Struthers* had himself experimented on the cat with similar results. The explanation given that evening might be fair enough as a theory, but we must be careful in deducing physiological conclusions from these experiments. Observers had shown that, in the dog at least, the brachial nerves, and the part of the spinal marrow from which they arise, exerted a similar influence; and, besides, the results of various experiments were not uniform on different animals. The anatomical explanation, also, was not free from difficulty. The communication between the sympathetic and third nerve in the cavernous sinus was but a slight one, and was not constantly to be found, while it was the sixth or abducens nerve which formed the large and constant communication; but it was the third and not the sixth nerve which supplied the iris. In

confirmation of the theory that the third nerve did not furnish the dilating power of the pupil, Dr Struthers had found that in paralysis of the third nerve, in which the pupil remains fixed in a somewhat dilated condition, the iris still retained its dilating power under the influence of belladonna. But this might be, independent of the nervous forces altogether, as we do not yet understand the mode in which belladonna acts on the iris in causing its dilation.

Dr Gairdner said that these experiments of Budge and Waller were remarkable, as they tended to clear up the differences in Dr Reid's experiments, and especially those which related to the different results obtained in dogs and rabbits, and to which allusion had been made by Dr Struthers. Dr Gairdner suggested that a series of experiments should be made on his patient when the effect of various reagents on the pupil might be ascertained.

TWO CASES OF ANEURISM UNSUCCESSFULLY TREATED BY COMPRESSION. BY JAMES MILLER, ESQ.—The author was of opinion that the question as to the treatment of aneurism by compression was still *sub judice*, and must be settled by the production of facts, rather than by mere reasoning. As an advocate for this form of treatment, however, he held it to be necessary that all facts bearing on the question should be adduced, and, acting on this belief, he brought forward these cases of failure.

CASE I. was one which Mr Miller had had under his own care in the Royal Infirmary. The patient was a shoemaker, aged forty-two, from Galashiels, and was admitted March 23, 1854. His general health was good, and the popliteal tumour was about the size of a hen's egg. Next day, Carte's compressor was applied on the femoral at its upper and lower portions. The pressure was moderate, and antiphlogistic regimen was enjoined. On the 27th, the upper compressor was removed, in consequence of the complaint of pain; and on the 5th of April it was noted that there was much pain, swelling, and œdema of the limb. A lump of lead, with an elastic band attached, as used in the Dublin hospitals, was now tried. August 1st, seventeen weeks since the commencement of the treatment, the tumour was harder and slightly diminished in size; pulsation was distinct. Flannel was ordered to be applied to the limb. The lead weights had up to this time been kept constantly applied above, and Carte's apparatus to the lower part of the limb. Aug. 3, the patient suffered much; anxious to have the artery tied. Carte's apparatus alone was now kept applied at night. Aug. 10, pressure was abandoned; the tumour was harder, but still pulsating, becoming caudate towards its head, and increased in size towards the inner side. Aug. 24, five months after admission, the artery was tied. The parts in its neighbourhood were found more matted together than usual. At 8 P.M., a slight thrill was perceptible in the tumour. There was sickness from the chloroform. Pain of the back was complained of. Aug. 25, the thrill was still less. Colchicum was given internally; and chloroform was applied locally to the back. Aug. 26, the thrill was scarcely to be felt; and on the 27th it was gone. Aug. 30, the thrill had returned; and on the 15th September it was noted that the thrill was still present; the ligature was yet in the wound; it came away on the 18th. October 10, the tumour was at length silent, and diminished in size. The patient was dismissed on the 14th.

CASE II. had been under the care of Dr Johnston of Montrose, and was admitted into the Montrose Infirmary, March 24, 1854. The tumour was about the size of a small orange, and had been first noticed in the previous July, and gradually increased in size. The pain at the time of admission was severe, the symptoms well marked. The health was good, and the heart sound. Low diet, etc., was ordered, and digitalis given internally. Carte's apparatus was applied at intervals, and soon another instrument alternately with it. Up to the 16th of August the tumour had become flattened and diminished in size to that of a walnut. The pulsation was weakened, but

was still apparent. The health now began to flag, and on the 23d a ligature was applied. Slight pulsation was felt in the tumour on the 28th, but never returned. The ligature came away on September 14th; and on the 15th of October the patient was dismissed from the hospital. Dr Johnston attributed the successful issue to the fact of the pressure employed having developed the collateral circulation of the limb.

Mr Miller drew attention first to the fact of the pressure not having been well borne. He believed that a still simpler method of applying it would yet be discovered. Secondly, careful watching was requisite during its application, as the patient, under the mistaken idea of expediting the cure, was apt to undue pressure to excoriate the parts and induce erysipelas. Any swelling of the limb, etc., demands attention, but cessation of the pressure appeared to suffice to dispel such dangers. Care to be taken that the pressure was not applied too low down. Third, it was to be noted that the collateral circulation was much affected. In the first case, the tumour first became harder, and then pulsation appeared in it from the collateral circulation. In such circumstances, Mr Miller would recommend instant application of the ligature, and if this was not done, difficulties were met with afterwards, even when the artery was tied. As it was, in this case the pulsation continued for six weeks. In Dr Johnston's case, however, the collateral circulation gave no trouble after the deligation of the vessel. In future, if after long-continued pressure the collateral circulation increased greatly, Mr Miller would cease compression, and after an interval of rest would apply a ligature, without, however, any risk of gangrene supervening. Fourth, as to the effect on compressed parts. In the first case, pain was always complained of, and a hardness could certainly be felt. On dissection, the parts were found much condensed, and difficulty was experienced in passing the needle, but this was to be avoided by cautious dissection. Fifth, after prolonged pressure, the state in which the system is, is one very favourable for a subsequent operation. In the first case, from accidental exposure while on the operating table, an attack of lumbago came on. Lastly, Mr Miller pointed out the odd parallelism which existed between the two cases as to the dates of admission and of their mental progress. These histories furnished the following arguments for and against the treatment by compression: *Against*. 1. The pressure was not well borne. 2. The effect produced on the collateral circulation, and on the parts immediately subjected to the pressure was very troublesome in the first case. 3. In both cases the plan failed after a trial of five months. *For*. 1. Compression can be maintained so long, it must be a comparatively safe procedure. 2. The free collateral circulation induced was favourable to any subsequent operation, as there is less risk of gangrene of the limb. 3. The constitution was better prepared for the operation by ligature, should that become necessary.

Mr Spence was an opponent of the treatment of popliteal aneurism by compression; but he felt that the Society and the profession were deeply indebted to Mr Miller for his candour in publishing these cases: and he only wished that all the cases of this treatment had been as faithfully recorded, as he was convinced that the question as to the respective merits of the operations in question would long ago have been set at rest. As to the increase of the collateral circulation which Mr Miller deemed advantageous, he (Mr Spence) was of opinion, that it might become too free, and endanger the subsequent progress of the case. He did not think that much was to be gained by it; and we must remember, that in this disease the collateral circulation had a natural tendency to increase. Gangrene, we must remember, also occurred after compression, and, in Mr Spence's opinion, was due more to over stimulation of the limb, than to any want of development of the collateral circulation which Mr Miller feared so much. This form of treatment, too, had always, to his mind, presented one great danger, viz., that many who did not consider themselves

capable of placing a ligature on the vessel would be tempted to try compression, a mode of treatment, as was well illustrated in the cases read that evening, which required very careful management. He felt convinced that the ligature was the simplest and safest method of procedure.

Dr J. D. Gillespie concurred in the remarks made by Mr Spence. Another danger which he (Dr Gillespie) thought incident to the treatment by compression, was that of secondary hæmorrhage from the matting together of the parts, and the consequent difficulty of the dissection when the vessel ultimately required deligation.

Dr John Struthers remarked, that a considerable part of the blood of the collateral circulation was poured into the artery above the aneurism, i.e., between the ligature and the aneurism. This also held true with regard to femoral and axillary aneurisms. The operation by ligature, therefore, still allowed of a considerable current along the vessel at the site of the aneurism. The current was not stopped, but only very much retarded; thus favouring coagulation. The principle involved in the two operations was thus very similar; but the results, as seen in Mr Miller's cases, were widely different. The advocates of compression had represented the danger of the operation by ligature as much greater than it really was, by an unfair use of statistics. These statistics had been made up from cases performed at a time when the operation was a bad one, before surgeons were alive to the importance of denuding only the smallest possible extent of the artery. Dr Struthers referred to Scarpa's directions as to the dissection of the vessels, viz., that the surgeon was to pass his thumb below the artery, unless he had very large thumbs, in which case he was to pass his forefinger instead. Now, this involved the laying completely bare an inch and a half, to two inches of the vessel. Although the operation of Scarpa was modified in accordance with the kind of ligature he used, the reference above still showed that in times past the importance of not disturbing the connections of the artery was not recognised. It was only in comparatively recent times, chiefly through the influence of Mr Syme, that surgeons had recognised the importance of careful manipulation, in the later stages of the operation, as determining its ultimate success. The results had been highly successful. Mr Syme relates upward of twenty cases operated on with success. The operation, now-a-days especially, was a mere trifle compared with the protracted suffering from continual compression,

Mr Spence reminded Dr Struthers that Hunter, in stating the object of his operation, distinctly declared that it was a mere slowing of the current, and not complete stoppage, which he desiderated. He (Mr Spence) recognised that the principle of the two operations was essentially the same; but the methods of putting it into execution were widely different.

Mr Miller was still of opinion that the establishment of a free collateral circulation was attended with many advantages. Gangrene might, no doubt, be caused by over stimulation of the limb; yet that it might be directly produced by the ligature, he referred to a case which he had published. (*See Association Journal*, May 19th, 1854.) He (Mr Miller) agreed with Mr Spence that compression was not so suitable for employment in private as in hospital practice, where a staff of assistants could be obtained. As to the danger of secondary hæmorrhage, the parts in the first case was not so matted as he had expected; and such a condition he regarded as no drawback other things being equal. He declared himself in favour of compression.

The *President* said, that the question was curious and interesting, why compression was so successful in Dublin, and so much the reverse in Edinburgh. It had been remarked that, as a general rule, the proportion of stout men was less in the Irish hospitals than with us, the patients there being more spare. Might there thus exist a difference in the respective coagulability of our and our neighbours' blood? In a surgical work published the other day by Guérin, he (Dr Simpson) had seen it stated that, of a hundred and fifty cases

reated by compression, one in five had died. In his (Dr Simpson's) opinion, the previous preparation of the constitution during the compression was very important; and he for one, did he labour under the disease in question, would undoubtedly give compression a fair trial. Travers mentions that the success which a Dr Young obtained in the removal of cartilaginous bodies from joints was generally ascribed to his keeping his patients for two or three weeks in bed, until the bed-fever was over. Dr Simpson thought the treatment of aneurism without knife or ligature a fair problem yet to be solved; and perhaps the difficulty would be solved by attacking the aneurism itself.

CASE OF CANCER SUCCESSFULLY TREATED BY EXTERNAL APPLICATION OF CAUSTIC. By J. D. GILLESPIE, M.D.—A gentleman, aged seventy-nine, of a large robust frame, in 1847 received a slight abrasion of the heel, which speedily cicatrised, but the cicatrix soon after gave way and ulcerated. The ulcer dried up and left a hard warty substance in its place, which continued to increase. In 1851, it had assumed the appearance of a hard cancerous growth, fully half-an-inch in depth. By the advice of Mr Syme, caustic was applied. Erythema of the leg soon after set in and reached as far as the groin, and the tumour increased in size. A second attack of erythema occurred a few weeks after. The tumour continued to increase in size; the glands in the groin became enlarged; the general health began to be impaired, and the mental faculties were affected. It was in October 1851 that caustic had been first applied; the tumour was then the size of a walnut, spongy, and occasionally bleeding. Chloride of zinc was had recourse to, and the tumour came away, but after cicatrization had commenced, the tumour reappeared. The application was continued for a year with similar results, repeated hæmorrhages took place and the glands in the groin were permanently enlarged. Potassa fusa was applied on three different occasions, but the chloride of zinc was decidedly more efficacious. On September 18th, a year after the first application, chloride of zinc was tried for the twentieth time, and the tumour to the extent of three inches was destroyed; a part of the os calcis exfoliated. Two years have now elapsed, and there had been no tendency to reproduction. Dr Gillespie considered the case interesting, as it was just such a one which was occasionally vaunted by some quack as a victory gained over modern surgery, and this one certainly explained their so-called cures, and at the same time it was fraught with caution to the surgeon not to lose an opportunity of giving similar cases a trial with the caustic before dooming them irrevocably to the knife.

* Professor Miller referred to cases of soft cancer of the breast where a cure was sometimes obtained by the diseased product sloughing *en masse*. He could corroborate the statement of Dr Gillespie with reference to the efficacy of the chloride of zinc; as a caustic it was thorough, and easily kept under command.

The following officers were elected for the ensuing year:—

President: William Sellar, M.D.; *Vice-Presidents:* John Taylor, M.D., Samuel A. Pagan, M.D., Robert Omond, M.D.; *Councillors:* Alexander Zeigler, M.D., James D. Gillespie, M.D., J. Matthews Duncan, M.D., Andrew Wood, M.D., Archibald W. Dickson, Esq., James Miller, Esq., David Gordon, M.D., Daniel R. Haldane, M.D.; *Treasurer:* John Struthers, M.D.; *Secretaries:* William T. Gairdner, M.D., J. Warburton Begbie, M.D.

BIOGRAPHY OF THE LATE PROFESSOR EDWARD FORBES.

SINCE the publication of our last number so many able sketches of the career of the late Professor Edward Forbes have appeared in the different newspapers and literary journals, that the chief incidents of his life, and the general tenor of his writings, must be familiar to most of our readers. We cannot, however, on this account, refrain from the melancholy satisfaction of

offering our meed of homage to the memory of one whose intimate friend we are proud to have been for the long period of twenty-one years, or from attempting to sketch that character, and those great acquirements in science, literature, and art which we have had every opportunity of studying in their early dawn, as well as of appreciating in their full maturity.

It was at the commencement of the Edinburgh academic session 1833-34 we first met Forbes. He was then 18 years of age, and had already spent one year in the University as a medical student. When a boy in his native place of Douglas, in the Isle of Man, he had exhibited a taste for drawing and for forming collections of natural objects, so that, when his school education was over, it was a question whether he would follow art or science as a profession. The latter did not appear to his friends to offer any brilliant advantages, and he was accordingly induced to visit London, and commence regularly the student-life of an artist. Six months' training, however, in the school of the late Mr Sasse, convinced him that something more was required fully to occupy his mind. But as pure science was still considered by his friends a very unproductive method of obtaining a livelihood, he was obliged to consent to study medicine, as the means of enabling him at the same time to combine practical aims with the indulgence of his tastes. This brought him to Edinburgh, where he first matriculated in 1832, and attended the usual routine of the medical classes for three successive years. During this period he could never conquer his dislike to medicine as a profession. He was seldom seen in the dissecting-room or Infirmary. Even his attendance on the purely medical classes was of no great use to him, as he did little but sketch the features of the professor or of the surrounding students. We still, fortunately, possess some of the rough pen and ink sketches he then made, and here give one of the late Professor Hamilton, which he drew at our side when we attended together the lectures of that distinguished teacher of midwifery:—



But, whilst inattentive to the more strictly medical studies, he was enthusiastic in his attendance on the botanical and natural history lectures and excursions of Professors Graham and Jameson, with both of whom he became very intimate. His tendencies were to ramble over the country, and explore the neighbourhood of Edinburgh to a considerable distance, collecting plants, shells, insects, minerals, and every other object interesting to a naturalist. Many of the struggles we have had to resist his invitations on these occasions, in order

to follow our own more special subjects of study. With the results of those excursions his rooms were always littered—specimens of rocks, shells, plants, books, sketches and scraps of poetry, being intermingled in admired confusion.

Nothing could be more diversified than the mental characteristics of the various young men who constituted the privileged set of Forbes' friends during the years 1834-5 and 35-36. To suppose, as some do, that they were all naturalists like himself, is very erroneous. They were chosen from the literary, theological, legal, and medical faculties of the University, and therein was the chief charm of their society. They were all ardent students, however, in their respective departments, and leaders in the Speculative, Physical, and Medical societies. Though hard workers, they by no means despised recreation—they did not "scorn delights" though they lived "laborious days." Hence there was a geniality and feeling of good fellowship flung over their scientific, literary, and professional discussions; an intermingling of wit, poetry, song, and good sense at their convivial meetings; a total absence of jealousy, and a strong desire for one another's advancement, which not only cemented their friendship, but exercised a great influence on their subsequent career. Theirs, indeed, was the true enjoyment of the student's life, in which the *wile* and the *dulce*—hard study and refined enjoyment—mingled together in glorious harmony.

"Denkt oft, ihr Brüder
An unsre Jugendfröchlichkeit!
Sie kehrt nicht wieder
Die goldne zeit."

Such, indeed, were the strong feelings of friendship and unity of sentiment which existed amongst this group of students, that a society or order was at length formed, the members of which were distinguished in the University by wearing a red ribbon across their breasts. This association aimed at bringing together every earnest student, who also possessed feelings of good fellowship, so that mutual assistance might be afforded in their various pursuits. It embodied rules for testing and admitting candidates, and put forth the following manifesto, chiefly drawn up by Forbes, as expressive of its principles:—

"The highest aim of man is the discovery of the truth; the search after truth is his noblest occupation. It is more—it is his duty. Every step onwards we take in science and learning tells us how nearly all sciences are connected. There is a deep philosophy in that connection yet undeveloped—a philosophy of the utmost moment to man—let us seek it out. The world in which we live is a beautiful world, and the Spirit of Omnipotence has given us many pleasures and blessings—shall we not enjoy them? Let us refresh ourselves with them thankfully, whilst we go forth in our search after truth. We are all brethren, but it has pleased God variously to endow our minds: some delight in one thing, some in another; some work for the good of the body, and some for the good of the soul; let us all work together in fellowship for our mutual happiness and joy. Wherefore should men quarrel one with another, because they hold different doctrines? Such as seek for truth in the right spirit sympathise with each other, and however opposite may be their present opinions, revile them not, but assist in their development, knowing, however wide apart may seem the paths they have chosen, one goal is aimed at, and if persevering, both must meet in the one wished-for temple. Let those who feel the spirit to develop the wisdom of creation, and to act for the good of their fellow-men, strong within them, unite together in a bond of fellowship, each brother devoting his time and his energies to the department for which he feels and proves himself best fitted, communicating his knowledge to all, so that all may benefit thereby, casting away selfishness and enforcing precepts of love. By such means glory shall accrue to his order, so that it may wax powerful in intellectual strength, and become a mental and a moral

safeguard to the world, and a bond of union among all nations. Such is our brotherhood."

Of this brotherhood Charles E. Stewart was president, and Donald Mackaskill and Edward Forbes vice-presidents. Stewart died in 1838, and was succeeded by Forbes, who assuredly has ever carried out the principles of the order, and been actively engaged in bringing about co-operation among earnest men for the advancement of truth.

In the session 1834-5, he and three others of the brethren were associated in bringing out a weekly publication, called the "University Maga." It was illustrated by sketches of several prominent men about College, by Forbes, and contains several of his poetical effusions. The following is one of his songs at this period, and bore relation to a public meeting of the medical students, having reference to the then imperfect working of Mr Warburton's Anatomy Bill.

THE ANATOMY BILL.

"O come, ye grieving Medicals, and listen to my lay,
Warburton's bill the subject is, a bad one, too, you'll say,—
But what else can I sing about, since in the rooms around,
Than that curs'd bill, no other *subject* happens to be found!

O measureless the evils are that measure hath brought on :
Anatomy is cut up quite, and surgery is done !
The demonstrators, too, are now all at their wits' end set,
And though they're at *extremities*, not one limb can they get.

Should one by chance a thorax get, one's *parted* from the *part*,
By that *disheartening* bill ere yet one can cut up the heart ;
Our bones are *boned* and buried too, ere *bonus* we may gain,
And to examine arteries, we try it all in *vain* !

The lecturers deserted are amid their empty rooms,
And *grave* as dead men lying fast enshrouded in their tombs !
No resurrectionist dare take his digger in his paws,
Lest that cursed *bill* should *hook* him with its prohibition *clause*.

The bill which burking should prevent has burked anatomy !
The bill for keeping murder down has murdered surgery !
The operation's *capital*, as some old fools have said,
Since operations of all sorts it knock'd hath on the *head* !

O *Billy, Billy* Warburton ! what have you been about ?
More subjects far had been brought in, had your bill been thrown out ;
And if with better *measure* you don't furnish us, I ween,
Soon in the schools, as well as rooms, no *body* will be seen !

Then rouse ye, suffering Medicals, your sentiments declare ;
The *dead* weight of Warburton's bill no longer calmly bear ;
Petition ! meet ! and speechify ! seize all sorts of occasions
To demonstrate how much you stand in need of *demonstrations* !

On the approach of spring, 1836, it became necessary for him to prepare for his examination, at the prospect of which he manifested, on all occasions, the greatest repugnance. But as the necessity of "going up" was strongly urged upon him by his friends, and as he was deficient in the requisite knowledge, the writer of this sketch (being at the moment considered chief *medicus* of the set), undertook to grind him in anatomy and physiology. With great trouble we at length forced him to write out his schedule of study and send it in to the secretary's office. Then commenced many dismal evenings and yawnings over the bones, and anatomical books, of which he soon became weary, often asking friends to come in at the time when he thought he should be tired of such work. We need scarcely say that Cloquet's Anatomy and the bones were

then thrown aside, for an evening of gaiety and philosophical discussion. His ideas of an examination at this time were described in the following fragment of a

VISION OF ONE "GOING UP."

"Methought that most eventful day had come,
 When I before professors most austere
 Must go, and undergo examination,
 And that I sate me trembling and afraid,
 On the stone steps of famed Physician's Hall.
 When as I sate, there came from out the door
 A poor rejected student, whose pale looks
 And palpitating heart, bespoke his fate,
 Still stronger told by sight most horrible
 Of prussic acid bottle in his hand,
 The final finisher of all his woes ;
 And at his heels a fierce examiner
 Rush'd reckless, with a loud resounding laugh,
 When me espying, in he bade me come,
 And meet my fate—entranced by the gaze
 Of his fierce eye, and by the solemn sound
 Of voice used to imperative command,
 I follow'd him instinctively, and saw
 A sight which sickens me to recollect.
 There, in a lofty and a lengthy hall,
 Around a table covered with green baize,
 Sate the examiners—animals
 Of wond'rous shapes, with horns, and bills, and claws,
 And hoofs, and asses' ears, and grinding teeth,
 Wherewith to torment and to terrify
 The luckless student, who, unknowing what
 A horrid fate awaited him, came there,
 In Sunday clothes, to seek for a degree.
 * * * * *
 There came a horrid shriek across my brain,
 And in excess of terror I awoke,
 Then thankful found myself once more alive,
 In my arm-chair's embraces, by the side
 Of half-exhausted fire—so breathed a prayer,
 And rang my bell for a supply of coals,
 Then reading Cloquet, fell asleep again !"

After this it need not be wondered at, that when summoned to appear on a certain afternoon, he at the appointed time was *non inventus*. He shortly afterwards, however, assisted actively in the formation of the Botanical Society, in which he was especially a supporter of the plan for exchanging specimens of plants, whereby his knowledge of their localities might be extended.

He spent the winter session of 1836-7 in Paris, attending the professors of the Jardin des Plantes and the Sorbonne. Here he drew around him English and French students, and propagated the principles of the order already referred to, a branch of which he organised there, and which we found in full operation on our visiting Paris in the following year. In the autumn of 1836 he roamed over the south of France, and crossed the Mediterranean to Algiers. He had previously visited Norway, and from this time he was in the habit of making autumnal excursions to various parts of Europe, "botanizing, geologizing, and zoologizing," as he used to express it. The results of these expeditions were published in numerous papers which he communicated to the various scientific journals, and they laid the foundation of that extensive knowledge of nature, and of that brilliant reputation which he subsequently attained.

He passed the session 1837-8 in Edinburgh, a winter memorable for the serious snow-ball riot among the students, to quell which the military were called out, who entered the College at the point of the bayonet. This disturbance gave rise to an immense number of songs and pasquinades, many of which were written by Forbes, who afterwards published the greater number as a pamphlet, under the name of "The Snowdrop." He illustrated it with sketches of the present Lord Robertson, who successfully defended the accused students on their trial, and of the accused and accusers who figured in court. Amidst all his gaieties and lighter productions, however, science was not forgotten, for in 1838 he published his "Malacologia Monensia, or Catalogue of the Mollusca inhabiting the Isle of Man and the neighbouring sea." This, his first work, was dedicated to Professor Jameson, "by his sincere admirer and attached pupil."

The years 1839 and 1840 were occupied principally in extending his knowledge of marine zoology, although other parts of natural history were not neglected. He lived for many months together with his friend, Mr Goodair, at Anstruther, in Fife, in whose company he dredged, not only the neighbouring seas, but extended his excursions to the Shetland and Orkney Islands, and also to the Hebrides. In the winter seasons he gave numerous lectures to popular institutions in Edinburgh, St Andrews, Cupar, and Dundee, and commenced a systematic course to the students of Edinburgh, on natural history, which for want of encouragement was not concluded.

In those investigations into marine zoology, he employed the dredge, an instrument which he was accustomed to say is as useful to the naturalist, as the thermometer is to the natural philosopher. Certainly he and Mr Goodair used it continually in their numerous joint excursions, and through Forbes it was elevated into a very important means of research. When the British Association met in Glasgow, in 1840, he proposed that a dredging committee should be formed, the establishment of which he celebrated by the following song. Its style may be judged of by the first three verses:—

THE DREDGING SONG.

BY A MEMBER OF THE DREDGING COMMITTEE OF SECT. D.

"Hurrah for the dredge, with its iron edge,
And its mystical triangle,
And its hid net with meshes set,
Odd fishes to entangle!
The ship may move through the waves above,
Mid scenes exciting wonder;
But braver sights the dredge delights,
As it rove the waters under!

Chorus—Then a-dredging we will go, wise boys!
Then a-dredging we will go!

Down in the deep, where the Mermen sleep,
Our gallant dredge is sinking;
Each funny shape in a precious scrape
Will find itself in a twinkling!
They may twirl and twist, and writhe as they wist,
And break themselves into sections;
But up they all, at the dredge's call,
Must come to fill collections!

Chorus—Then a-dredging, etc.

The creatures strange the sea that range,
Though mighty in their stations,
To the dredge must yield the briny field
Of their loves and depredations:

The crab so bold, like a knight of old,
 In scaly armour plated;
 And the slimy snail, with a shell on his tail,
 And the star-fish radiated!

Chorus—Then a-dredging, etc.

In 1841, he published his "History of British Star Fishes, and other animals of the class Echinodermata," in which many new species he had discovered in his dredging expeditions were, for the first time, described. This book is beautifully illustrated from his own designs, many of which exhibit a playful humour or a sentiment that gives an exquisite zest and interest to a subject, which at the time was little understood. In April of the same year, at the invitation of Captain Graves, he joined, as naturalist, H.M. ship "Beacon," which was commissioned to bring from Syria the remains of antiquity discovered at Xanthus by Sir Charles Fellows. On this expedition he, in conjunction with the Rev. Mr. Daniel and Lieutenant Spratt, carefully examined the country and coast of Lycia. They discovered no fewer than eighteen ancient cities, the sites of which had been unknown to geographers. They traced the marches of Alexander the Great and of the Consul Manlius through this part of Asia Minor, and indicated many of the spots said to have been visited by St Paul. The natural history of the whole district was carefully described by Forbes in the second volume of the work, which he and Lieutenant Spratt subsequently published, giving an account of their travels. Mr Daniel fell a victim to the endemic remittent fever of the district, and Forbes himself afterwards had a narrow escape from its grasp, as at the commencement of the attack, although suffering from great prostration, he insisted on visiting the tomb of Hippocrates in the island of Cos. So much enthusiasm was, at all events, engendered by his medical studies, but to that illness may be ascribed much of the occasional languor and feverishness under which he ever afterwards laboured.

It was now that his good training in Edinburgh as a naturalist became of such value to science; for not only as a botanist and zoologist did he observe the flora and fauna of the regions he traversed, but as a geologist, he connected them with the rocks and minerals with which they were associated. These combined observations led him to that beautiful generalization which at once placed him in the foremost rank of living naturalists, and indicated the law which regulated the development of animal and vegetable life in the depths of the ocean. His researches were made public at the Cork meeting of the British Association in 1843, when he read his "Report on the Mollusca and Radiata of the Ægean Sea, and on their distribution, considered as bearing on Geology." This paper was drawn up from the results of 100 fully recorded dredging operations in various depths from 1 to 130 fathoms, and in many localities from the shores of the Morea to those of Asia Minor, besides numerous vast observations whenever opportunity offered. In this paper, he observes that the Ægean Sea, although most interesting to the naturalist, as the scene of the labours of Aristotle, has been but little investigated since his time.

During 1842 he remained attached to the "Beacon," working with his dredge in the Ægean Sea, and among the islands of the Grecian Archipelago, multiplying observations, collecting plants, minerals and animals, and sketching the beautiful scenery and the picturesque groups of men and women which attracted his attention. He, of course, encountered numerous adventures, the relation of which in after years constituted one of the charms of his conversation. One story he used to tell with infinite humour of how, at some remote island, he was presented to an invalid Greek lady as a distinguished physician from Edinburgh—how embarrassed he felt at his complete ignorance of the complaint, at how, notwithstanding, by means of sundry shakes of the head and saying nothing, he established for himself a high reputation as a doctor, and ulti-

mately took his leave, without a fee it is true, but accompanied by the good wishes and blessings of the husband and relatives.

The purposes for which the "Beacon" were sent out could not be accomplished, so, in the spring of 1843, Forbes returned to England. He had previously left instructions with his friend, Mr Goodsir, that should any situation become vacant, which he could worthily occupy, to apply for it in his name. Accordingly when the Professorship of Botany in King's College, London became vacant, application was thus made for it, and Forbes was appointed so that on returning a few days afterwards, he unexpectedly found himself metropolitan professor. He visited Edinburgh, however, before entering upon the duties of the chair, and we shall not easily forget the pleasure we then experienced on looking over the rich portfolio of drawings he brought with him. Water-colour drawings, sketches in pencil and chalk of Eastern landscape, marine views, temples, and ruins—groups of Turks and Greeks, picturesque costumes, comic incidents, mingled with copies of plants, shells, fishes, and other objects of natural history—the whole forming a characteristic medley, indicative of the character of his observations, and skilful method of treating them. On the 8th of May he gave his introductory lecture at King's College, which was subsequently published, and he afterwards introduced the Edinburgh plan of teaching botany practically in the fields, lanes, and open country. "Those who attended his class," says the *Athenæum*, "will ever remember the charm he threw around the study of vegetable structure, and the delightful hour they spent in his company during the periodical excursions, which he made a point of taking with his pupils in the neighbourhood of London. Nor were these excursions attended by pupils alone. Many are the distinguished men of science in London who sought this opportunity of availing themselves of his great practical knowledge of every department of natural history. It was during the delivery of his first course of lectures on botany, that he worked out the interesting relations that exist between the morphology of the reproductive system of the Sertularian zoophytes, and its analogy with that of flowering plants. His paper on this subject was read at the British Association at York in 1844."

Shortly after becoming a professor in King's College, he accepted the appointment of librarian and curator to the Geological Society of London. "In this position," says a friend of his, writing in the *Scotsman* of Nov. 22, 1854 "his extended knowledge of recent vegetable and animal species, and his remarkable acquaintance with the laws of their distribution (particularly as regards invertebrate animals), became available for general palæontological research. Here, too, he was enabled to apply to geological research that peculiar knowledge of the conditions of existence of species which his continuous operations with the dredge had led him to."

In 1845 he resigned this office, and accepted that of palæontologist and lecturer on natural history to the Government School of Mines, in connection with the Ordnance Geological Survey, under the direction of Sir Henry De la Beche. From this period his life was occupied in arranging and describing the great accumulation of fossils and geological specimens which the survey was continually bringing to light,—in making excursions in connection with it, or in autumnal trips to different parts of Europe,—in attending the scientific meetings of the Royal, Linnæan, Zoological, Geological, and other Societies, and the British Association, to all of which he read many valuable papers, being co-operating with the fellows in their committees and councils. He was continually giving courses of lectures at King's College or in the School of Mines, besides frequently lecturing at the Royal and other popular institutions. On one occasion, having three months previously promised to lecture at Iddington, he was dining at a friend's house in Surrey when he received a telegram message saying the audience were waiting for him. He had quite forgotten his engagement, and, whilst hurrying to the place by railway, vainly endeavoured to recall to his remembrance on what subject he had been ad-

tised to lecture. The audience were very much out of humour when he appeared, but, without a moment's hesitation, he gave them a most brilliant extempore discussion, illustrated, as he alone could do it, by rapid drawings of animals with chalk on the black board, which soon excited the deepest interest. He has often since declared that that was the very best lecture he ever delivered.

His writings during this time were many and important. Among others may be noticed his contributions to the "Memoirs of the Geological Survey of Great Britain," including a valuable paper, or, as it may more justly be considered, a complete original work—"On the Connection between the Distribution of the Existing Fauna and Flora of the British Isles, and the Geological Changes which have affected their Area." In this Memoir is exhibited his intimate and extensive knowledge of the three kingdoms of nature, whereby he communicated a great impulse to geological science, which its cultivators even now are only commencing to appreciate. His beautiful figures of new fossil species of shells also, are among the most perfect things of the kind ever published. In 1848 the Ray Society brought out his "Monograph on the British Naked-eyed Medusæ." Of this book, though it exhausted the subject at the time it was written, and was most beautifully illustrated by thirteen folio plates, the figures in which were drawn by himself, he modestly observes, "Even now I can offer only an outline of a most curious and interesting, though neglected, department of British zoology. The greater part of the matter in this essay is new. With one exception, kindly communicated by Mr Alder, every species has been examined by myself. Every figure is original. Any defects in the engravings must be laid to my charge; their merits are due to my friends, Mr W. Bailey and Mr C. R. Bone, for whose exertions I have to return many thanks." He next occupied himself, in conjunction with Mr Hanley, in a large work on the "History of British Mollusca," in which the wonderful power he possessed of delineating animals, at once accurately and artistically, was again brought into play. From the preface of this work also may be gathered how numerous and widely scattered were the naturalists who aided him in his labours, and how careful and conscientious he was in ascribing to all the merits belonging to them. In this respect, indeed, always fearful of doing injustice, he was accustomed to over rather than under-rate the scientific qualities of his friends. This work was completed in 1853, in four large volumes, with numerous plates.

To him is mainly owing the arrangement of specimens in his own department of the Museum in Jernyn Street, and the natural history collection of plants and animals now exhibited in the Crystal Palace at Sydenham. He also took a deep interest in the exhibition of 1851, and wrote an account of its vegetable productions for the *Art Union Journal*. Indeed he contributed, in addition to his scientific papers, an almost endless number of reviews and poetical pieces to the various literary and artistic periodicals. His article on "Shell Fishes, their Ways and Works," in the first number of the New Series of the *Westminster Review*, is a beautiful specimen of easy writing; and the brilliant article on "Siluria," in the last number of the *Quarterly*, is from his pen.

Forbes entered largely into society, in which he was at all times popular. His wit and innocent humour served to light up the social circle, where he shone as brightly as in philosophical discussion or scientific investigation. Although he had not the most remote idea of tune, he used to chaunt his various poetical effusions, or songs as they were called, to a species of recitative that rendered them highly amusing. At the first meeting of the British Association in Birmingham the great expenses of the ordinary led him and a few scientific friends to dine daily at a small tavern which presented the sign of the "Red Lion." Before the conclusion of the meeting, the Red Lion diners became so famous that the tenement would scarcely hold the guests, and was resolved to continue them wherever afterwards the Association should

meet. Gradually a club was formed, called the Red Lion Club, the members of which still assemble regularly in London, and amongst whom may be found some of the most distinguished cultivators of science, literature, and art, of the metropolis. It was at these meetings he chaunted his songs, which were always highly relished from the ease and gaiety that distinguished them, and from their reference to recent scientific or public events. Thus, at the meeting of the British Association at Oxford, in 1846, Mr Strickland gave a long evening lecture on the Dodo, a bird found on three islands of the Indian Archipelago by the early Portuguese navigators, and only extinct within the last two centuries. The subject was discussed for several hours the following morning in Section D, and gave rise to much amusement, when, in spite of the Prince of Canino's contending for the Dodo being of the cock-a-doodle species, it was generally voted to be a gigantic pigeon and a percher, though destitute of flying wings. At the next Red Lion dinner Forbes chaunted a long "ornithological romance, giving a history and full account of the discovery and opinions concerning the Do-do. The following are some of the best verses of this song :"—

THE FATE OF THE DO-DO.¹

Do-do ! Although we can't see him,
His picture is hung in the British Museum ;
For the creature itself, we may judge what a loss it is,
When its claws and its bill are such great curiosities.

Do-do ! Do-do !

Ornithologists all have been puzzled by you !

Do-do ! Monsieur de Blainville—

Who hits very hard all the nails on his anvil,
Maintains that the bird was a vulture rapacious,
And neither a wader, nor else gallinaceous :

A Do-do ; a Do-do,

And not a cock-a-doodle doo !

Do-do ! John Edward Gray, sir,
Doubted what Mr de Blainville did say, sir,
And held that the bird was a vile imposition,
And that the old Dutchman had seen but a vision—

A Do-do ; a regular do !

And didn't believe one word was true !

Do-do ! alas for our wisdom !

Strickland has come to the judgment and his doom,
From a hole in the head, and a bone with a ridge on,
Is that our *rara avis* was only a pigeon,

Our Do-do only a doo,

A regular doo, like a turtle-doo !

Do-do ! Alas there are left us

No more remains of the *Didus ineptus*,

And so, in the progress of science, all prodigies

Must die as the palm-trees will, some day at Loddiges,

And, like our wonderful Do-do,

Turn out not worth the hullabaloo !"

But during all this time, though incessantly occupied in writing papers and books, arranging species, making excursions, multiplying observations and deducing laws, he ever considered all these as preparatory to the one great object of his life—namely, the occupation of the chair of Natural History in the University of Edinburgh. Even his marriage, which occurred in 1848, to the youngest daughter of the late General Sir C. Ash-

¹ He pronounced these syllables as in the verb "To do."

worth, did not, as with most men, induce him to take a house and furnish it. Though in London he was not of it, and he perseveringly refused to hamper himself with any incumbrances which might interfere with or ultimately prevent his elevation to the great professorship in the North. All his hopes and future plans pointed to Edinburgh as the only appropriate place for developing that vast amount of natural history acquirement he had attained. There, in the centre of the well-explored scenes of his youth, and surrounded by the friends and brethren of his student-days, was to be systematized the extraordinary mass of isolated observations which he had all his life been laboriously accumulating. There he was to give forth to crowded audiences the great generalizations which his penetrating intellect had enabled him to form. There he was to obtain that leisure which would enable him to finish those splendidly-illustrated works that were to hand his name down to posterity. There was to be formed a magnificent museum, to be arranged after a method of his own, and for which, during many years, he had been silently making preparations. Whole collections formed by naturalists in various parts of Europe were to have been absorbed in this great undertaking, and to him many jealous cultivators of science had promised to surrender treasures which no other man now is likely ever to command. So consistently and ably had he carried out those principles of scientific brotherhood which we have seen he developed in his early youth, that there can be no doubt that that Edinburgh Museum, which he had long determined to erect into a great monument to his name, would have ultimately equalled, if not surpassed, all similar institutions of the kind. Those and like plans he developed to us in the conversations we had with him on his first arrival in Edinburgh after his appointment to the chair; and all who knew him will readily believe that they were not mere dreams or idle imaginings. For ourselves, we are firmly persuaded that, had he lived other ten years, his name would have descended to posterity as illustrious as those of Aristotle, Linnæus, and Cuvier.

At length he arrived at the goal of his wishes. How did his old friends rejoice on the 15th of last May, when, at his inaugural lecture, they spontaneously hastened from all parts of the country to welcome him and do him honour! None of them will soon forget the splendid auditory which assembled in the largest class-room of the University, crowded to overflowing with the learning, talent, and science of the modern Athens. To them with what meaning did the following passages teem, in the noble discourse he then delivered, and which he afterwards revised for publication in the June No. of this Journal:—

“If any spot on earth is peculiarly adapted for the study of natural history it is this—the district in whose centre we are now assembled. Everywhere about us are abundant and admirable illustrations of zoology, botany, and geology. Of its excellent and well-explored flora I leave my eminent colleague and old friend, the Professor of Botany, to speak. Amid the rich materials of its fauna I learned some of my earliest and best zoological lessons. To dredge the Frith of Forth under the guidance of shrewd, strong-handed, and strong-armed Newhaven fishermen, was an early ambition of mine, and one never too often gratified. I know the riches of the living treasures that lie in its submarine deeps, and along its shores; and though, since the time I ventured to print occasional notices of these embryo efforts, I have explored most part of the coasts of the British Isles, and our seas far out, and foreign seas and estuaries, famous for their productions, I have not found any marine region with a population more varied within its limits, and better calculated to illustrate effectively the subjects of a naturalist's studies.

“As to geology, where can there be a better district for practising the student in field observation? The leading phenomena of rock-masses are brought almost to the door of our class-rooms. Sedimentary and igneous rocks contend to show us their anatomy and conformation. If a considerable part of the series of strata that constitute the geological scale be wanting, their very absence,

through comparison, may be made a theme of instructive discourse, whilst there are many of the most interesting formations, both fossiliferous and unfossiliferous, within the limits of a day's excursion. The variety and beauty of the mineral contents of the igneous rocks around Edinburgh have long been famous, and have imbued every geologist, who received his early training in this University, with a respect for mineralogical evidence, and a habit of readily perceiving and using mineral characters—a great advantage.

"The tastes of most men can be traced back to the habits of their youth, and these habits are, in a great measure, moulded by the circumstances, physical as well as intellectual, amid which that youth has been passed. Grand scenery suggests grand thoughts, and every ennobling thought elevates, not merely momentarily, but permanently, the mind in which it glows. It is a great gain to a university to be placed like this, amid scenes of unrivalled beauty. The youth whose hours of relaxation are spent in the presence of those magnificent prospects, so ripe and many around us, carries with him in after-life the memory of their beauty and their grandeur. The man who has gazed upon and felt the worthy delineation of a glorious landscape, a grand Turner or a luxuriant Claude, never forgets the genial, wholesome, glow of admiration that pervades his spirit at the time. How much more must he feel this ennobling sensation when he gazes on the reality of majestic landscape? And if, with all this precious accumulation of the vast and beautiful, there be combined that which is admirable in the minute—if nature, in her smallest elements, be prolific in objects of study and reflection, it is not to be wondered at that this University has been a hot-bed of naturalists, and that their philosophy has been one catholic in essence and far-extending in its range."

And then his graceful conclusion and eulogium on his old master and predecessor :—

"After many years of study, and travel, and precious opportunities for acquiring experience, I return to the city where I was first initiated into the science of nature, and where within these walls I learned those lessons of patient inquiry and minute observation, to whose working and training I am indebted for the place that I now hold among the Professors of my *Alma Mater*. To my illustrious predecessor and master who passed from amongst us ripe in years, honours, and fame, so lately, I gratefully record my acknowledgments for the encouragement of those tastes and the founding of that knowledge which have proved to me a chief delight. Who, that in time past was his pupil and found pleasure in the study of any department of Natural History, can ever forget his enthusiastic zeal, his wonderful acquaintance with scientific literature, his affection for all among his friends and pupils who manifested a sincere interest in his favourite studies. When, in after life, their fates scattered them far and wide over the world, some settling amid the civilized obscurity of rural seclusion ; some rambling to the far ends of the earth to sift and explore wild savage regions ; some plunging into the boiling and noisy whirlpool of metropolitan activity ; none who remained constant to the beautiful studies of his pupilhood was ever forgotten by the kind and wise philosopher, whose quick and cheering perception of early merit had perpetuated tastes that might have speedily perished if unobserved and unencouraged. The value of professorial worth should chiefly be estimated by the number and excellence of disciples. A large share of the best naturalists of the day received their first instruction in the science that was afterwards to prove their fountain of honour from Professor Jameson. Not even his own famous master, the eloquent and illustrious Werner, could equal him in this genesis of investigators. Under his auspices, too, were lasting friendships and unions of kindred minds formed that have been productive in good to the cause of knowledge. Valuable as were his writings—each when estimated with regard to the position of science at the time of its issue, an effective advance—his pupils were even more valuable. The greatest praise of a great professor is that which proclaims he has founded a school. And where else in the British empire,

except here, has there been for the last half century a school of Natural History?"

The course which he thus commenced was attended to its conclusion by a class of students and amateurs so numerous that it could scarcely be accommodated on the benches, one hundred and fifty professional students alone having enrolled their names. What a contrast did this present to the hard worked for pittance afforded to him as a man of science by Government. How he revelled in the idea of the easy income and the time at his own disposal which the possession of the Edinburgh chair would give him—and how apparent it was, that although grateful for the encouragement and advantages afforded to him by official appointments in London, his genius and scientific spirit had long pined to throw off the trammels which had been imposed upon them. These and similar feelings he has recorded in the following mock-heroic stanzas:—

A DOLEFUL BALLAD ABOUT THE RED TAPE WORM.

Written by a Government Clerk, who at an advanced age had awakened to a knowledge of the fallacy of the Superannuation Fund.

"Oh the Red Tape Worm is munching my soul!
Oh the Red Tape Worm is crunching my poll!
Spirit and body—substance and form—
All chew'd up by the Red Tape Worm!

The Red Tape Worm, though wondrously wise,
Is very shorted-sighted, or has no eyes;
And the best anatomists all would fail
To make of the animal head or tail!

I know a Treasury clerk or two,
Who love that worm as its mother would do;
Who'd rather see Newton and Shakspeare fry
Before they would let one Tape Worm die.

In Downing Street the Tape Worms thrive;
In Somerset House they are all alive;
And slimy tracks mark where they crawl
In and out along Whitehall.

A very young Tape Worm we may meet
In Marlborough House and Jermyn Street,
Rearing to play its parent's part
On a milky mixture of science and art.

The Red Tape Worm is especially fat
When lodged in the brain of a diplomat.
'Tis there he'd coil and suck for ever
His loved tit-bit of Turkey's liver.

* * * *

When I'm dead and yield my ghost,
Mark not my grave by a Government post;
Let mild Earth Worms with me play,
But keep vile Tape Worms far away.

And if I deserve to rise
To a good place in paradise,
May my soul kind angels guide,
And keep it from the official side!

[*The Government Clerk revels for a moment in this dream of celestial bliss, but suddenly awakes to the maddening reality, and sings,—*]

Oh the Red Tape Worm is munching my soul!
 Oh the Red Tape Worm is crunching my poll!
 Spirit and body—substance and form—
 All chew'd up by the Red Tape Worm!"

. It would be easy for us to introduce a vast number of humorous songs by Forbes, more or less related to his natural history studies, not to speak of others not connected with them. Thus, "The Sea-Serpent," "The Potato," the "Chanson Microscopique," and "Chloroform and the Dead Pig," "John Chinaman," "Father Matthew," are all excellent. We cannot, however, forbear from introducing "The Oyster," not only on account of its merits, but because it is the last we heard him sing. We sat next him at the annual dinner given by the Promoter of the Medical Faculty of the Edinburgh University last August, on the graduation day, when he introduced the following rhymes, by saying they were a report of a lecture by an Irishman on

THE ANATOMY OF THE OYSTER.

"Of all the conchiferous shell-fish,
 The oyster is surely the king;
 Arrah, Mick! call the people who sell fish,
 And tell them a dozen to bring.
 For its I that intend to demonstrate,
 The cratur's phenomena strange;
 Its functions to set every one straight,
 And exhibit their structure and range,
 In sweet rhyme!

Now boys, I beseech, be attentive,
 On this Carlingford fasten your eyes,
 As I spread it before you so pensive,
 Its gape opened wide with surprise;
 See that small purple spot in the centre,
 That's its heart, which is all on the move;
 For though looking as deep as a mentor,
 Its tenderly bateing with love

All the while!

Like a Chesterfield peacoat, its liver
 (Of fusty brown Petersham made)
 It folds round its stomach, to give a
 Supply of fresh bile when there's need.
 For though *we*, when we swallow our oyster
 Like it raw, and by cooks undefiled,
 The cratur itself is much choicer,
 Preferring its condiments biled!

It's so nice.

The fringes that circle its body,
 Which epicures think should be clear'd,
 Are the animal's lungs—for 'tis odd he,
 Like a foreigner, breathes through his beard.
 And among all its memorabilia,
 Than this structure there's none half so queer,
 Though Sharpey may say they are *cilia*,
 A wiser contrivance to speer.

Let him try!!

Now these are the facts in the history
 Of an oyster I'd on you impress;
 I've sarved them up plain without mystery,
 To cook them would just make a mess.

So now, boys, we'll fetch in the whisky,
 Since the water is hot on the hob ;
 Whilst we stir up our native so frisky,
 By sticking a knife in his gob !!
 Dear auld fish."

It was observed by a Professor present at the dinner, that the last line reminded him of certain ineffectual efforts to inject the oyster, by the late Dr Barclay, who concluded his attempts by saying, "Well, you brute, if I can't inject you, I'll eat you," suiting the action to the word. At this same dinner also, Forbes communicated a fact illustrating how lecturers are sometimes suddenly manufactured in London. At the commencement of one of his courses on Botany, in King's College, he observed a gentleman assiduously writing notes day after day. He seized an opportunity of remarking to this individual, that he seemed to be taking a great amount of trouble. Whereupon it was frankly explained to him, that the supposed student had been recently appointed lecturer on Botany in one of the London medical schools, and knowing very little of the subject, found it convenient to deliver to his own students in the afternoon the lecture he heard Forbes give in the morning.

Last autumn, after residing for a short period in London, and making final arrangements for the transmission of his private collections, books, etc., to Edinburgh, he joined the meeting of the British Association at Liverpool, where he was elected President of the Geological Section. We have been informed that nothing could exceed the tact and judgment with which, during one of the most stormy debates that ever occurred, and in which his many friends were engaged on opposite sides, he contrived not only to prevent rancour and ill-feeling, but even to introduce a genial humour, and extract harmony from discord.

He commenced the winter session in Edinburgh with a fatigued and jaded look, and informed us that, on a geological trip before the Liverpool meeting, he had very imprudently continued to walk and drive four hours, after being thoroughly wet through in a heavy shower of rain. He complained of chills and feverishness, which he insisted indicated a return of his old enemy, the remittent fever he had caught in Greece, and for which he took quinine. On Sunday, November 5, we were summoned to his bedside, and found him labouring under slightly febrile symptoms, with an accelerated pulse. Notwithstanding our urgent remonstrances to the contrary, he insisted on going to the College next day as usual, and such was his anxiety regarding the formation of his class, that he continued to lecture up to the Thursday following. Then hoping a few days' rest would restore him, he announced his intention of suspending his lectures until the following Monday. The febrile symptoms however continued to increase, and on Sunday he complained of obscure pain in his back. Notwithstanding the application of leeches, and the use of aperients and diaphoretics, the lumbar pain continued. On Monday the urine became tinged with blood and loaded with pus, when the real character of the disease could no longer be doubted. On Tuesday when rising, he experienced sudden and acute agony over the lower and left lateral half of the abdomen, the pulse became rapid and weak, and the skin assumed the tawny aspect indicative of pyæmia. Although there might possibly be a calculus in the kidney, these and the preceding symptoms too closely resembled those of a renal abscess bursting into the peritoneum to leave us long in doubt as to the grave nature of the case. Mr Goodsir and the writer, who were in constant attendance on our dear friend, now sought the further advice in consultation of Dr Christison. We need not say how anxiously every point was considered, nor how despondingly we were obliged to confess that little hope existed of his recovery. On Thursday the progress of the disease destroyed even the little hope that remained to us. He was informed of his approaching end, and received the intelligence with calmness, made provision for his wife and family

left his scientific papers to R. Godwin Austen, Esq., secretary of the Geological Society, and all his collections of natural history to the University which he honoured as a student, and adorned as a professor. He died on Saturday, November 18, at the early age of 39 years.

We were then informed that he had frequently expressed a desire that after his death the body should be examined, for the benefit of science; a circumstance we are proud to record of him, as indicative of the ardent love of knowledge by which he was ever distinguished. His wishes were fulfilled and the morbid changes which had occurred ascertained to be, *1st*, Chronic pyelitis of the left kidney, evidently of some years' standing, with great distension of the pelvis; *2d*, A circumscribed abscess the size of a walnut in the lower third of the organ, apparently of some weeks' standing, as it was lined throughout with a thick pyogenic membrane; *3d*, A recent purulent infiltration, to the extent of a hazel nut, in the upper part of the kidney, which, together with the more chronic abscess formerly mentioned, communicated with the capsule, and through it with the parts external to the organ; *4th*, Perinephritis, the capsule of the kidney being thickened and glued by recent adhesions to the cellular tissue behind and peritoneum in front; *5th*, Extensive cellulitis and infiltration of pus behind the left kidney, extending down to the pelvis, and passing under the psoæ muscles and along the spermatic cord to the scrotum; *6th*, Peritonitis anteriorly and laterally, glueing several coils of intestines together by recent layers of lymph. All the other organs, and especially the prostate, were perfectly healthy.

To describe the sensation which the death of Professor E. Forbes occasioned is scarcely possible. That he who had of late filled so large a space in the eye of the scientific world—one of the council of the Royal Society, President of the Royal Geological Society, and of the Geological Section of the British Association, member of the Linnean, Zoological, Geographical, and other Societies—that he who, at the age of 39, had succeeded to the chair of Jameson in the Edinburgh University, and for whom a long career fruitful in new discoveries, honours and emoluments seemed so certain, should be thus suddenly cut off, was not only most unexpected, but appeared to be a misfortune too great to be readily realised. But so it was; and although the great systematic works he projected will never appear, still so powerful has been the influence of his genius, that wherever natural history is cultivated—wherever the union of botany, geology, and zoology can be appreciated—wherever science, literature, and art are acknowledged to be capable of elevating the mind and purifying the heart, his loss will be mourned as that of one of their most earnest and truthful disciples. His remains were accompanied to the Dean Cemetery by the members of the University Senate in their robes, with the venerable Principal at their head; by the Lord Provost, Magistrates, and Town Council of the city; by a large number of students, and a great concourse of his friends and brother naturalists, who assembled from all quarters to pay this tribute of respect to his memory.

The following extract from the *Literary Gazette* seems to us so truthful an analysis of his character, that we cannot do better than simply transcribe it.

"Edward Forbes had a great intellect. He was an acute and subtle thinker, and the broad philosophical tone and comprehensive grasp of his many-sided mind enabled him to appreciate and to understand the labours of others in fields of inquiry far different from his own. A naturalist by inclination and by profession; a close observer in the museum and in the field; possessed of a vast acquaintance with the details of those branches of science which he had made his especial study; no less capable of the widest generalizations, as his *Ægean* researches more especially show; in speculation a Platonist, delighting in Henry More; in literature and in art blessed with a solidity of judgment and a refinement of taste such as fall to the lot of few; in social life a humorist of the order of Yorick: gifts like these are alone sufficient to raise a man to eminence, and to lead us to lament, as a great calamity, his sudden and early

leath. But it was not these qualities which distinguished him so highly beyond his fellows. To say that he had them will not enable those who stood without the privileged circle of his friends to comprehend why, within that circle, the old mourn him as a son, and the young as a brother. It is not because he was so gifted that the veterans of science one and all affirm his loss to be irreparable, and the aspirants know that they may succeed but cannot replace him. Our affections cling to character and not to intellect; and rare as was the genius of Edward Forbes, his character was rarer still. The petty vanities and heart-burnings which are the besetting sins of men of science and of men of letters, had no hold upon his large and generous nature—he did not even understand them in others. A thorough spirit of charity—a complete toleration for everything but empiricism and pettiness—seemed to hide from him all but the good and worthy points in his fellow-men. If he ever wronged a man, it was by making him fancy himself better than he was. Worked to leath, his time and his knowledge were at the disposal of all comers; and though his published works have been comparatively few, his ideas have been as the grain of mustard-seed in the parable—they have grown into trees, and brought forth fruit an hundred-fold.”

We are happy to say that his friends have found among his papers a most methodical list of all his writings, which literally amount to hundreds in number, commencing with “Notes of Experiments on Animals of the Genus *Gymnæa*,” read to the Royal Physical Society of Edinburgh, June 24, 1832, and terminating with the article *Siluria*, in the last October number of the *Quarterly Review*. There is also a Journal of his Voyage to Asia Minor and Greece, kept with the utmost regularity. We have been allowed to look over some of these papers, and have seen with astonishment the amount of labour of various kinds which he had accomplished. In science there are his long catalogues of plants and animals of different regions; his numerous contributions to different journals, and his published works. Several books, including “*The Rambles of a Naturalist*,” “*The Zoology of the European Seas*,” and various papers, are in a forward state of preparation, and many of the blocks already cut for illustration are now in the hands of his publisher, Mr Van Voorst. In literature there are endless lists of various papers and contributions to journals, besides a host of poetical compositions on almost every subject, and some serious pieces in verse of great beauty, of which latter we append to this sketch a short and pleasing sketch. In art, there are endless sketches of men and things, poems illustrated in outline, water-colour drawings, and careful designs for future scientific publications. There is a very extensive series of landscape sketches in water-colour, depicting the influence of geological structure on scenery, which it was his intention to have brought out as a separate publication. His collected works would, if judiciously edited, serve to show the world what Forbes had already done, and of how much he was capable; and we sincerely trust that some kindred spirit in science and literature will still, for the sake of his memory, combine those already published with the fragments he has left, and present us with a work worthy of his genius and equal to his fame.

But who can do justice to the wonderful power he possessed of inspiring confidence and respect in all with whom he came in contact—to that earnestness which evidently guided his own proceedings, and which he infused into the labours of others—to the remarkable quality of availing himself of the investigations of his fellow-naturalists, and, while thus carrying out his own generalizations, elevating the isolated observer in the opinion of himself and of the public—or to the genial humour which presided over his intercourse with others, and especially gave a charm to his lectures, and even to his scientific publications? Who that has heard him unfolding his beautiful theories, and seen him illustrating them by sketches on the board, now rapidly producing all sorts of animals—now, with a steady artist’s hand, displaying the graceful

curves of a shell, and then, by a touch, imparting animation to what had previously been dead and inert,—which of his audience have not recognised that his philosophy was “not harsh and crabbed, as dull fools suppose,” but capable of at once exciting enthusiasm and creating admiration? In him, indeed, was a union of qualities so valuable and varied, that we doubt the probability of our meeting with it again; for he possessed the investigating and generalizing power of the man of science, the skilful hand of the artist, the imaginative mind of the poet, and the truthfulness and simplicity of an upright and noble nature.

We conclude with a few lines of poetry, illustrative of Forbes' more serious compositions :—

TO A STAR.

“A night sky overhead;
One solitary star
Shining amid
A little cloud of blue, for dark clouds hid
Its sister-sunlets. On its azure bed
It seemed a sun, for there
No jealous planet shone with which it to compare.
The dark clouds rolled away,
And all night's shining train
Of suns and stars,
And the great moon, poured forth their silvery light.
Where, then, was that fair star which shone so bright?
Where was it? none could say;
Yet there it surely was, although it seemed away.
So lustrous shall we find
On earth each living soul
When seen alone;
And though, when brighter forms around it press,
We lose its form and doubt its loveliness,
Still should we bear in mind
That it is not less bright although it be outshined!”

E. F., Feb. 19, 1850.

J. H. B.



Tail piece by Forbes, from his book on Star Fishes.

SCOTCH REGISTRATION ACT.

THE new Registration Act comes into operation on 1st January 1855. The section (XLI.) specially referring to medical men is as follows :—“The medical person who shall have been in attendance during the last illness, and until the death of any person shall, within fourteen days after the death of such person, and under a penalty not exceeding forty shillings in case of failure, transmit to the registrar a certificate of such death in the form of the schedule (G) hereto annexed, the particulars of which shall forthwith be entered by the registrar in the register, and the registrar shall from time to time furnish, gratis, to every medical person within his pariah or district known to him, or

who shall require the same, the necessary copies of such certificate." The schedule referred to is as under.

(G.) MEDICAL CERTIFICATE— WITH CAUSE OF DEATH. Name. _____ DATE. _____ had continued _____	(G.) MEDICAL CERTIFICATE—WITH CAUSE OF DEATH. To the Registrar of the _____ of _____ in the _____ of _____. I hereby certify that I attended _____ who was apparently aged _____ years; that I last saw h— on the _____ day of _____ 18—; that he died on the _____ day of _____ 18—, at _____; that the cause of h— death was _____ and that the disease Witness my hand this _____ day of _____ 18—.
---	---

Signed, _____
 Profession, _____
 Residence, _____

Besides the medical attendant, householders, relatives, and the undertaker, are held bound to give notice of death. In the case of births the medical man incurs no responsibility; the parents, and failing them, the nurse, etc., being required to give the necessary intimation to the registrar. One grand defect runs through the whole Act, namely, that while the many different parties interested in the three events of birth, death, and marriage, incur penalties for non-intimation to the registrar, no provision is made except in two cases for receipt or acknowledgment by that functionary that the original information has been lodged with him. To obviate this defect, so far as the medical profession is concerned, we would suggest that the registrar should be asked to initial the margin of schedule G.

VARIETIES.

ADVERTISEMENTS IN THE WEEKLY MEDICAL JOURNALS.—If flaring and extravagant advertisements lower the character of common newspapers, they must degrade medical journals still more, devoted as they are to topics of a serious nature, and treating of matters peculiar to their readers. Yet the advertising sheets of the medical journals present more examples of the transition from the sublime to the ridiculous than those of any other periodicals: indeed people can scarcely refrain from laughter when they view the pictorial illustrations which ornament them. Here is the gentleman with the tail of his shirt drawn over his pubis who sells the "mock-main" truss; and there the lady's *torso* in *cuero*, with the elastic appliance called "the abdominal supporter, most valuable to those ladies who are anxious for the look of their waists;" while another female figure displays the beauties of "an air-pad for prolapsis uteri" *in situ*, "highly appreciated by the most eminent members of the profession." Then comes "a bed-pan of cunning construction," followed by "railway urinals for male and female." We say nothing of the picture of the nursemaids rolling their babies along in three-wheeled barrows, nor yet again of the "grype and grimly beast" which warns us to "mark our linen." We do not even object to the pair of breeches, with boots and spurs attached, they are at least harmless; all we venture to do is to suggest as much decency respecting such matters as may prevent people from talking of "nasty doctors," as they view these pictures, or even something worse than them, in shop windows. We pretend not to dictate to our contemporaries, they are the best judges of what is fitting, but we see no harm in saying so much.—*Dublin Med. Press.*

ROYAL COLLEGE OF PHYSICIANS OF EDINBURGH.—At the late election meeting of this College Dr Begbie was placed in the President's chair, over the heads of eighteen of his senior Fellows. We regret to say that this was entirely owing to the active exertions of the party now predominant in its affairs, who, to the number of twenty, had private meetings, and sent round circulars to their friends directing them how to vote, so as to place their own leaders and Dr Begbie in the council. Among so small a body as the Edinburgh College of Physicians a secret cabal or Junto of this kind, formed principally at supper parties among the juniors, must of course always secure a majority of votes. It would therefore be wiser and more dignified for the senior and distinguished Fellows (all of whom, we are happy to say, opposed the late proceedings), not to mix themselves up in any way at present with College affairs. The Junto have already so mismanaged the question of medical reform in Scotland, that all the sister colleges and institutions have shrunk from it in distrust. Its present forlorn condition will certainly not be improved by the late election, which is simply an acknowledgment that elderly general practitioners of the College of Surgeons, who find it convenient to provide for their sons by surrendering to them their general practice, and by retaining what they are pleased to call consulting business, are in future to be elevated by the Junto, over men who have been consistent supporters of the College all their lives.

THE PRACTICE OF PHYSIC SHOULD BE FOUNDED ON KNOWLEDGE RATHER THAN EXPERIENCE.—It is not unworthy of remark that many physicians profess to hold chemistry in contempt, exactly as they do physiology; that medicine reproaches physiology, with the same injustice, that she reproaches chemistry. The physician who has learned medicine, not as a science, but as empirical art, acknowledges no principles, but only *rules* derived from experience. The object of his inquiries is only whether a remedy, in any given case, had a good or a bad effect. This is all the empiric cares about. He never asks *why*? He never inquires into the *causes* of what he observes! How differently would the treatment of diseases be conducted if we had perfectly clear notions of the processes of digestion, assimilation, and excretion. Without just views of force, cause, and effect,—without a clear insight into the very essence of natural phenomena—without a solid physiological and chemical education, is it to be wondered at that men, in other respects rational, should defend the most absurd notions; that the doctrines of Hahnemann should prevail in Germany, and find disciples in all countries? Reason alone will not prevent whole nations from falling into the most abject superstitions, whilst even a child whose mind has been duly developed and instructed will repudiate the fear of ghosts and hobgoblins.—*Liebig's Letters on Chemistry*.

M. CAPURON.—M. Dubois says, in his *éloge* of M. Capuron, who left a considerable fortune in charitable bequests, besides L.6000 to his relations, and L.40 annually to the Academy of Medicine, for a prize, that—"He passed his entire life in the *Pays Latin*, near the schools and hospitals. For many years he followed the clinique of Duputren at the Hotel Dieu, and then that of M. Bouillaud at La Charité. His grey hairs were seen towering over all the young heads by the bedside where he disputed for the best place with the students. For some time he was an obstinate supporter of the doctrines of Bousais, at whose lectures he sat on the first bench. No student ever lived harder than he. His room presented the poverty and severity of a monk's cell; a thick dust covered his books and furniture, which latter consisted only of an old bureau and a few straw chairs. It is not known whether a little fire ever enlivened the gloom. The use of a carriage was to him completely unknown. He was to be seen, in the depth of winter, every morning hastening to the cliniques, his face red with cold, and he himself only clothed in a simple frock coat that no one ever remembered to have seen new. He died 28th April, aged eighty-three years.—*Gazette Medicale*.

TOWN PURIFICATION.—Every house in which tenants die of typhus fever should be held suspect, examined by the authorities, and, if need be, like a foul rave-yard, summarily closed until it has been put into a wholesome state. Many small owners, no doubt, could not afford the immediate expense of renovation; but a principle already introduced as part of sanitary discipline might be extended: an efficient Board of Health might be empowered to effect all necessary alteration, and distributing its charge on each house-owner over thirty years, saddle him with no more than a small terminable tax upon his premises. By the adoption of a policy like this, carried out strictly and carefully, how much might be done in the course even of a single generation for the cure of our towns—done too at no real cost to the nation, by the mere guidance of house-owners into a path of justice profitable even in the most worldly sense to themselves, and by lending them such power to fulfil necessary injunctions as they may not have immediately at command.—*Household Words.*

MIS-STATEMENTS OF THE "MEDICAL TIMES."—We took occasion to point out during the Gay affair, when the *Medical Times* was full of rounded sentences about the honour and dignity of medicine, how incompatible its practice was with its professions, in admitting anonymous letters, full of malicious statements, and attacks on medical men in Edinburgh. These letters have lately been resumed, and abound in false assertions, with a view of puffing the writer's friends and injuring his opponents. We beg to caution our readers against believing any portion whatever of the "gossip" of the 'provincial correspondent' for Scotland, and to express our regret that a journal should find it worth its while thus to pander to those mean and secret attacks on character, so disgraceful in one belonging to the profession of medicine.

BANQUET TO A BONE-SETTER.—In November last a public banquet was given to a country bone-setter called Mason, in Wisbeach, in Cambridgeshire, which was presided over by the Mayor, when a silver cup and a sum of money amounting to L.225, 8s. were presented to him. "It was remarkable that just as Mr Mason was proceeding to attend the dinner, he was called upon by a lady to reduce a dislocation of the coliar bone. After leaving her he was stopped by a case of fractured leg, which he also set. The dinner was kept waiting in consequence, but the delay was inconsiderable."—*Cambridge Independent Press.*—This transaction will constitute an important illustration of the manners of the 19th century, for some future historian.

THE LONDON WEEKLIES AND THE EDINBURGH SCHOOL OF MEDICINE.—The necessity of finding matter to fill four weekly medical journals, seems now to have exhausted the numerous schools of London of those lectures on which these periodicals mainly depend for matter. Hence the *Lancet* has had recourse to Mr Syme of Edinburgh, and the *Medical Times* to Dr Stokes of Dublin. The *Times*, in addition, has a correspondent in Edinburgh, who evidently imposes upon the editor, by sending him the merest twaddle and petty malice of his own under the head of "news." The *Association Journal*, again, reports the meetings of societies, and any other scraps of Edinburgh intelligence it can pick up, in order to communicate interest to its pages.

LINES ON THE DEATH OF PROFESSOR E. FORBES.—A gentleman who attended the course of Natural History last summer, and who is favourably known in the literary world, observes on the death of Professor Forbes:—"One could almost fancy that Truth perceived he would outrun his age and silenced a tongue which had also anticipated the future:—

Nature, a jealous mistress, laid him low.

He woo'd and won her; and, by love made bold,

She showed him more than mortal man should know;

Then slew him lest her secrets should be told."—*Scottish Press.*

We are happy to announce that Dr George Wilson has undertaken to write

an extended biography of the late Professor E. Forbes, for which purpose all the papers and information necessary, will be placed at his disposal by the family, and will, we trust, also be afforded him by the friends of Forbes generally. The biographer of John Reid and of Cavendish will, we are satisfied, execute the task with all that elegance and judgment characteristic of his literary productions, urged thereto as he is by a sincere friendship and long and intimate acquaintance with the subject of his memoir.

THANKS OF PARLIAMENT TO THE MEDICAL OFFICERS OF THE ARMY AND NAVY.—The Minister of War, in proposing the thanks of the country to the army, observed of the medical officers (especial allusion having been made to Dr Thomson),—"I must say, my Lords, that if it has not been usual for Parliament to thank such men as these, I consider that it is not wrong for a Minister of the Crown in this House to acknowledge their services." ("Hear, hear," and cheers.) When Lord Palmerston *does* think it expedient to fulfil his promise, and put the medical profession in this country on a proper footing, it will be better for the public, as well as the civil services. Then it will not be unusual for the medical officers of the army and navy to be treated in every respect like their equals in rank.

HENBANE CIGARS.—Dr Seifert of Vienna, from personal experience, recommends, in some chest affections, the use of cigars containing a proportion of henbane leaves—the *Hyosciamus Niger*. He directs these medicated cigars to be prepared in the ordinary manner from tobacco deprived of its acrid principle, to each of which from five to eight grains of the powder of the leaves of the common henbane are to be added. He states that patients may smoke from four to eight such cigars daily.—*Dublin Med. Press*.

DROPPING FLUIDS FROM VIALS.—Dr Coale has found a very simple and ready means of dropping fluids from vials. Unless the lip projects very much, the fluid is apt to run back on the neck, and remain attached until several drops have accumulated. Greasing the neck of the vial (not the lip) will prevent this, and give a clean uniform drop.—*Proceedings of Boston Med. Society*.

ROYAL MEDICAL SOCIETY OF EDINBURGH.—The following gentlemen have been elected office-bearers for the Session 1854-55 :—*Presidents*—John Jardine Murray, Robert Rhind, Henry Marshall, David B. Smith. *Curator of Library*—Almeric W. Seymour, B.A. Cantab. *Sub-Librarian*—William Thompson. *Treasurer*—John F. Macfarlane. *Secretaries*—John Somerville, William James Otto.

PUBLICATIONS RECEIVED.

The Brain in relation to the Mind. By Joseph Swan. London. 1854. 8vo. Pp. 113.
On the Construction, Organization, and General Arrangements of Hospitals for the Insane. By Thomas S. Kirkbride, M.D., Physician to the Pennsylvania Hospital for the Insane. Philadelphia. 1854. 8vo. Pp. 80.

Unsoundness of Mind in relation to Criminal Acts, etc. By John Charles Bucknill, M.D., London, Physician to the Devon County Lunatic Asylum. London. 1854. 12mo. Pp. 148.

The Practitioner's Pharmacopœia and Universal Formulary, etc. By John Foote, M.R.C.S., London, etc. London. 1853. 12mo. Pp. 368.

Remarks on the Examining Medical Board for Indian Appointments, etc. By James Bird, M.D., F.R.C.S., etc. London. 1854. 8vo. Pp. 15.

The Piratical Specific. A New and Infalible Mode of Treatment for the Asiatic Cholera. By Dr F. Wilson, of Mauritius. London. 1854. 8vo. Pp. 27.

We are obliged to postpone numerous papers and reviews until our next Number. Professor Allen Thomson's communication has been necessarily delayed, from the impossibility of getting the illustrations ready for the present Number. Dr Young's paper has been received.



OVUM OF BIRD AND MAMMAL COMPARED

Fig 1.

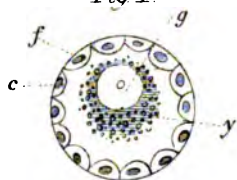


Fig 2.

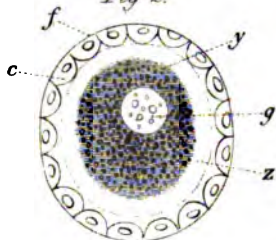


Fig 3.

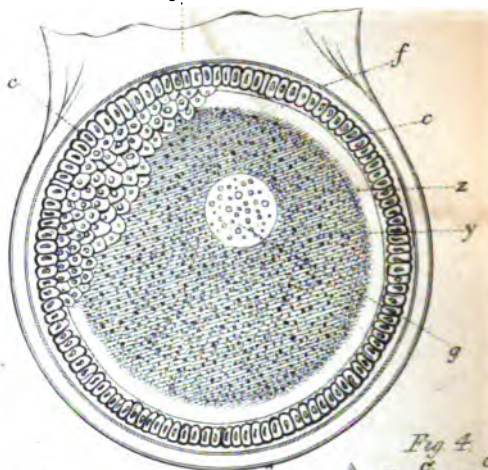


Fig 4.

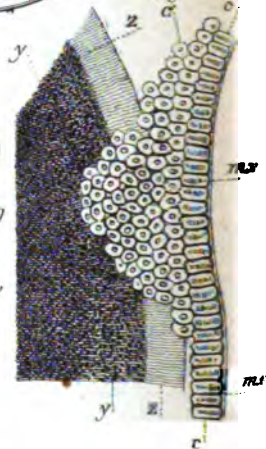


Fig 5.

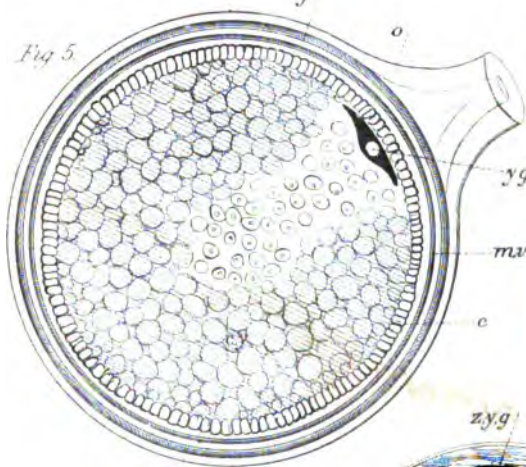
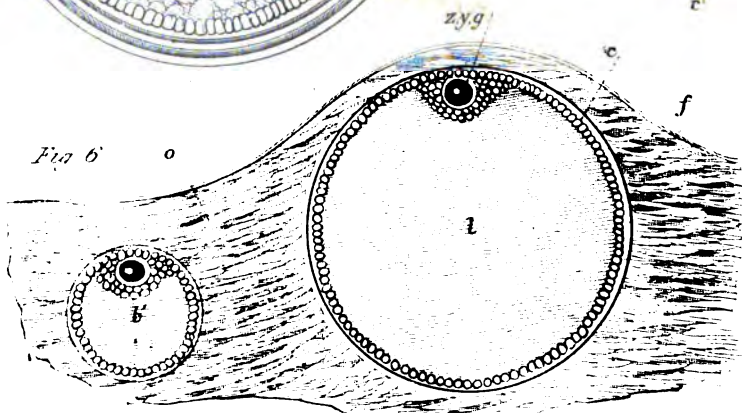


Fig 6.



Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Remarks on the Comparison of the Ovarian Ovum of Birds and Mammiferous Animals.* By ALLEN THOMSON, M.D., F.R.SS. L. and E., Professor of Anatomy in the University of Glasgow.

EVER since the discovery of the mammiferous ovum by Von Baer, in 1827, some degree of uncertainty has prevailed among physiologists as to the nature and extent of the analogy which may be admitted to exist between its several parts and those of the bird's egg. With respect to the ovary itself no doubt prevails; for all appear to be agreed that, notwithstanding some differences of size and minute structure, the pediculated capsules in which the yolks of the bird are formed are essentially homologous with the Graafian follicles of the mammiferous ovary. But the relation of the ovum to the ovi-capsule is well known to be widely different in the two classes of animals; for in the bird the larger mass of the yolk, or ovarian ovum, completely fills the pediculated capsule, while in the mammal the spherical ovum, of very small size as compared with the follicle, lies embedded in the proligerous disc of the layer of granular cells which lines the Graafian follicle; and the wall of the follicle, with the granular cellular lining, are distended by a considerable quantity of a clear fluid, which occupies the space within.

Von Baer regarded the large mass of the yolk in birds as the same with the cellular and fluid contents of the Graafian follicle in mammalia, and compared the whole mammiferous ovum—that is, the zona and parts within it—to the germinal vesicle alone (vesicle of Purkinje) of the bird's egg. The discovery, in 1834, of a germinal vesicle or germ-cell in the mammiferous ovum, similar to that of birds and other animals, withdrew the greater number of physiologists from V. Baer's view, and led them to consider the several component parts of the ovum, viz., the enclosing or vitelline membrane, the yolk substance, and the germinal vesicle, as corresponding or homologous structures in the two classes of animals. I have elsewhere shown¹ that in this view they may have been carried too far, and that the tendency of more recent observation has been to demon-

¹ Article Ovum of the Cyclopædia of Anatomy and Physiology, p. 77.

strate that although a general correspondence may be admitted to exist in the structure of the ova of almost all animals, still there are so great differences among them that the establishment of an exact homology of their parts must in the meantime be abandoned. It is my object, in the following remarks, to point out what appear to be the most important circumstances of resemblance and difference between the parts of the egg in the bird and in the mammiferous animal.

Among vertebrate animals three different groups may be distinguished, according to the type of construction of the ova in each, viz., 1st, The oviparous animals with large yolked ova, and a limited germinal disc, such as birds, scaly reptiles, and cartilaginous fishes; 2d, Those oviparous animals, such as the amphibious reptiles and the osseous fishes, in which the ova and yolks are of middle size, with a more extended germinal part than in animals of the first group; and, 3d, The strictly viviparous animals, such as mammalia, in which the ovum and yolk mass are comparatively small, but the whole of the latter is germinal.

Of the three parts which form the essential component structures of the ovarian ovum in all these animals, the germinal vesicle is least subject to deviation from the general type, while the yolk or vitelline substance and the enclosing membrane both present considerable varieties. The following is a very general statement of the nature of these several parts and of their most marked differences in the several groups of animals above distinguished:—

1st, The germinal vesicle or germ-cell is in all animals the original or first produced part of the ovum. In mammalia it presents the essential characters of a microscopic animal cell, varying in diameter in most instances from $\frac{1}{1000}$ th to $\frac{1}{300}$ th of an inch, and possessing within it a single nucleus, the so-called macula germinativa. In both the groups of ovipara it is of a much larger size (about ten times), varying from $\frac{1}{100}$ th to $\frac{1}{30}$ th in diameter, or even larger; and in these animals also, the nucleus or macula, though at the very first production usually single, soon changes its character, so that during the greater part of its growth, and in the mature state, it is either subdivided into very numerous spots or nuclei, or becomes uniformly diffused as a fine molecular deposit in the fluid of the germinal vesicle.

2d, The name of yolk or vitellus, originally given to the yellow coloured substance of the fowl's egg, was for a long time indiscriminately employed, in all animals, to designate the whole substance which fills the ovarian ovum, excepting the germinal vesicle, and excepting also, in the larger ova of the ovipara, the cicatricula, or germinal disc, in the centre of which, when mature, the germ-cell is placed. The vitelline substance consists of oleo-albuminous ingredients, of which part is in solution in the clear fluid of the yolk, and part is in suspension in the form of molecules, globules, or cells of various magnitude, from the most minute point up to $\frac{1}{200}$ th of an inch in diameter. In the greater number of animals the solid or

ganised particles are in such quantity as to render the yolk quite opaque; but in a few animals, as osseous fishes, the clear fluid preponderates, and the larger or smaller globules are seen floating freely in it, while the germinal mass is confined to a particular region of the cavity.

But the most important distinction to be observed in the nature of the yolk substance, in the three groups of animals referred to, is connected with the relation subsisting between this substance, or some part of it, and the formation from the previously unconnected elements of the yolk of a united layer of organised matter out of which the embryo takes its first origin; more particularly the process of yolk-cleavage or segmentation, or, as it ought perhaps rather to be designated, germ-cleavage, which immediately succeeds to the fecundation of the ovum, and is the antecedent of the first embryonic genetic process. In the group of mammalia, the whole mass of the finely granular yolk-substance undergoes the process of segmentation, and contributes directly by that process to the production of the organised materials of which the embryo and its first accompanying structures are formed; the whole, therefore, of the yolk substance is directly *germinal*, without distinction of its parts, excepting as respects the position of the embryo in relation to the axis of the ovum and seat of the first cleavage. In the group of birds, scaly reptiles, etc., the small light-coloured disc or cicatrix situated on the upper surface of the larger yolk, and composed of the finely granular substance, is alone the seat of cleavage, and alone takes immediate part in the genetic process preceding embryo formation; the remainder of the yolk, comprising the larger mass of deeply coloured substance, together with the lighter coloured substance of the centre or yolk cavity, and which consists of larger corpuscles of $\frac{1}{250}$ th to $\frac{1}{600}$ th of an inch in diameter, and presenting some of the characters of organised cells, is not immediately concerned in the formative process, but contributes only secondarily and indirectly to the subsequent formation of the organised parts of the embryo and its dependencies. This latter part may be looked upon, in fact, as an accessory supply of nutriment for the embryo after it has taken origin from the germinal part, constituting thus the *nutritive* yolk.

In the group of amphibia and osseous fishes the structure and relations of the yolk substance are in some measure intermediate between those of the other two groups, and the cleavage or first organising process affects a variable extent of the yolk substance; in some, as osseous fishes, scarcely more than a sixth; in others, as alytes obstetricans, about a half; and in others, as the common frog, toad and newt, nearly the whole, but yet in such a manner that the segmentation is not in any complete, but affects more fully that portion of the ovum on which the germ is situated. A variable extent, therefore, of the yolk is germinal or formative.

The distinction now indicated between a germinal or formative, and a nutritive part of the yolk has long been known to physiolo-

gists in a general way; but recent inquiries have added greatly to the precision of their knowledge of its relation to the structure of the yolk. It has been well described in several of its aspects by Barry, Reichert, and Coste. We shall presently see that the observation of the earlier stages of formation of the ovarian ovum in birds and mammals tends to place in a still more prominent point of view the distinction between the germinal or formative and the nutritive substance of the vitellus.

3d, The external vesicular covering of the ovarian ovum of animals has in general been named the vitelline membrane, from an assumed analogy with the covering of the yolk of the fowl's egg. In mammalia, however, an exception has been made to this, and the peculiar thickness and dense consistence of this membrane, and the doubts that have been entertained as to the true relation of the mammiferous ovum to that of birds and other animals, have led to the very general adoption of Von Baer's designation of *zona pellucida*, as applied to the external covering of the minute mammiferous ovum, when it leaves the ovary.

In mammalia, as already remarked, the *zona* closely surrounds a yolk which is entirely germinal, and finely granular; and the membrane appears to take its origin at a very early period of formation, by the consolidation of the external portion of the clear substance in which the yolk granules are deposited. But in birds and other animals of the same group, the vitelline membrane of the mature ovum is a delicate homogeneous structure, enclosing the nutritive as well as the formative parts of the yolk. In the fully ripe state, it is almost entirely without visible demarcation of structure in its substance; but in less advanced ova, as, for example, in ovarian ova of the fowl of a quarter of an inch in diameter, we find it to consist in part of distinct cells united by their edges, and it is obvious, therefore, that the vitelline membrane of the fowl's egg is very differently formed, and has altogether different relations from that of the mammiferous ovum.¹

With this preliminary statement of the principal varieties in the form and structure of the essential parts of the ovarian ovum, I return to the consideration of the question, What is the true relation subsisting between these parts in the ova of the oviparous and viviparous groups, upon which physiologists have hitherto been in some doubt? I ought, however, to state here, that although the views of physiologists have differed on this subject, some have approached very near the correct view of its bearing; and among these, I am

¹ It is not my intention at this place to enter more fully into the consideration of the structure of the vitelline membrane in general. Recent interesting observations have pointed out the existence in that membrane in the ova of some animals, of peculiar apertures, either of a permanent or temporary kind, and apparently designed to facilitate the process of fecundation. My present object is limited to the attempt to show the true relation of these membranes (*zona* and vitelline covering) to the germinal and nutritive yolk.

happy to quote the opinions of Dr Martin Barry, as expressed in a postscript to the second series of his *Researches on Embryology* (*Philos. Trans.*¹ 1839, pp. 369, 370), and thus to repair what has appeared to Dr Barry to be an omission on my part, in the article *Ovum* recently published. In the memoir quoted, Dr Barry, after referring to the facts, that while segmentation affects the whole yolk of the mammiferous ovum, it occurs only in a limited part of that of the osseous fishes, puts the question, "Is not the discus vitellinus in the ovum of the bird, the seat of similar divisions?" A remarkable anticipation, certainly, of what has since been proved to be the fact, by Bergmann, Coste, and myself; and he very justly continues: "If so, it will perhaps appear that the so-called yolk-ball in the mammiferous ovum, corresponds more particularly to the discus vitellinus (with its germinal vesicle) in the ovum of the bird."

With this view I entirely coincide, and the observations to which I am about to call the attention of the reader, will afford the most satisfactory proof of its correctness, though perhaps in a somewhat different manner than was anticipated.

In February 1852, Dr H. Meckel, of Hemsbach in Halle, published in the *Zeitschrift für Wissenschaftliche Zoologie* (vol. ii. p. 420), an account of some interesting observations on the formation of the ovum in birds, from which it appeared, that in the early condition of the ovarian ovum of birds, while its structure resembled more closely that of the mammiferous animal, its finely granular or primitive yolk substance is immediately invested by a covering corresponding to the zona pellucida of the mammiferous ovum; but this covering is only of temporary existence, subsequently disappearing on the formation of the cellular or nutritive yolk; and the whole yolk, that is, the larger mass of highly coloured or cellular vitellus, together with the primitive granular yolk substance, now disposed in the form of the vitelline disc, and containing the germinal vesicle, is at a later period enclosed by a membrane of newer formation, that, viz., to which the name of vitelline membrane is usually given in the bird's egg. H. Meckel farther showed, that the cellular part of the yolk of the bird's egg proceeds from the cellular contents of

¹ While I gladly avail myself of this opportunity to quote the above views of Dr Barry, of the omission to do which in my article *Ovum* he complains in a short paper published in the *Philos. Magazine* for October 1854, I must observe that Dr Barry has misunderstood some part of the remarks made by me at p. 77 of that article, and that more particularly he is in error in supposing that the remarks of mine which he has extracted and placed in parallel column with the statement of his own views, as above quoted, are the statement of H. Meckel's new observations on the formation of the ovum. The remarks so quoted were intended to express my own views of the relation of the parts of the ovum to each other, previous to the statement of H. Meckel's newer observations, which follows along with my own. Having with others long entertained these views, I did not feel myself called upon to give a historical relation of opinions in connection with this subject

the ovarian capsule, and that a part of these cells continues to the last to surround the vitelline disc, as well as the other parts of the yolk substance; but very unfortunately for the recognition of the value of these observations, and of the deductions to be made from them as to the morphology of the ovum, H. Meckel was led to adopt some untenable views of the relations of the several parts in the bird and mammal. More especially, he appears to have fallen into error in regarding the yellow or cellular yolk-substance of the bird's egg, as homologous with the corpus luteum of the mammal, instead of, as Von Baer rightly thought, and as seems more simple and natural, with the tunica granulosa or cellular contents of the Graafian follicle previous to its rupture.

Having confirmed and extended H. Meckel's observations on this subject, in the fowl and several other birds, I have thought it proper to call the attention of ovologists more particularly to them in this paper, especially as their correctness has been called in question by one of the most accurate and learned recent writers on the history of the ovum. In the article *Zeugung* in R. Wagner's *Handwörterbuch*,¹ Professor Leuckart states that he has not been able to perceive the distinct zona pellucida described by H. Meckel, and he therefore refuses his assent to the view of the morphology of the ovum, founded on the existence of a zona enclosing the primitive yolk of birds; but he adds,² that were the existence of such a membrane proved, a very different view might be entertained from that adopted by him, and generally received by physiologists. It does not appear superfluous, therefore, to bring forward a statement in confirmation and extension of H. Meckel's observations.

In birds, as in mammals, and indeed in most other animals, soon after the germinal vesicle is distinctly seen within the simple ovarian follicle (ovisac of Barry) two additions to the structure take place, the one more immediately surrounding the germinal vesicle, the other more closely connected with the inner surface of the ovisac. The first of these is connected with the deposit of the primitive yolk substance; the second with the formation of the granular cells of the ovarian follicle. In most animals the primitive yolk appears to consist of a somewhat clear basement substance, probably albuminous in its nature (see plate, figure 1st, *y*); and in this, either from the very first or very soon after its appearance, small dark or highly refracting granules, probably of an oily nature, are deposited. These granules are in greatest quantity near the surface of the germinal vesicle, and generally leave more or less of the clear fluid or basement substance without granules externally. This external clear part is not at first a membrane, as it has been described by some; but after a time it undergoes consolidation, so as to form a vesicular membrane or covering of the parts within. In mammalia

¹ Vol. iv., p. 792. Victor Carus agrees with Leuckart on this point, (see his *System der Thierische Morphologie*, p. 186).

² *Ibid.*, p. 818.

this covering, which is consolidated at a comparatively early period, is at first thin, it gradually acquires greater thickness and tenacity, becomes soon capable of being detached from the granular or opaque yolk substance, and then constitutes the zona pellucida, or permanent external envelope of the ovarian ovum of these animals. In many invertebrate animals, the external covering of the yolk takes its origin in the same manner, and is to be regarded as the same with the zona, though great differences afterwards arise in its extent, thickness, and structure. In Batrachia and Osseous fishes I am inclined to believe the membrane of the yolk takes its origin in the same manner; in the latter animals this membrane, which has generally been called chorion, undergoes considerable changes in the progress of the formation of the ovum, being, in many, perforated with a number of minute canals through its whole substance.

In birds and scaly reptiles a similar clear marginal space occurs round the primitive yolk (see figure 2d, z), and this has sometimes been described and figured as the vitelline membrane (as by M. Barry, 1st series, figure 26), but this space never attains to the same degree of clearness that is perceived in mammalia, and it never forms a membrane of the same firmness with that which is called zona pellucida in these animals. Nevertheless, according to H. Meckel's discovery, which I have fully confirmed, when the ovarian ovum of the bird, consisting as yet only of the germinal vesicle and primitive or granular yolk substance, has attained the size of $\frac{1}{40}$ th or $\frac{1}{30}$ th of an inch in diameter, there exists all round the surface of the granular or opaque yolk, a much clearer ring of substance, of considerable thickness and some degree of solidity, which maintains its shape, and may be isolated under pressure, and which has the same relations to the other parts as the zona pellucida of the mammiferous ovum. My observations differ from those of H. Meckel only in respect to the amount of clearness of the substance of this zona, the smoothness of its outline, and the degree of its separability from the granular yolk substance. It appears to me to have more the appearance of the superficial part of the basement substance of the yolk, left clear, but not altogether transparent, but rather turbid or minutely molecular in its aspect, and adhering to the opaque granular yolk substance, and moving with it, and therefore not so separable or so distinct from that substance, nor so like a membrane with a well defined outline, as H. Meckel's description would lead us to believe (see figure 3 and 4, z.) It has, however, considerable consistence, and its relations are so entirely analogous, that I cannot but regard it as essentially corresponding with the zona pellucida or *primitive vitelline membrane* of mammalia.

This last is, in fact, its proper designation. It appears then that the first formed yolk in mammalia and birds is finely granular, and that the primitive yolk with the germinal vesicle or primitive ovum, is in both enclosed by a more or less clear and condensed external portion of the primitive yolk substance.

the result of recent research has been to show, that while a general analogy in the constitution of the ova of animals may be admitted, the deviations from a common type are such as to render it inexpedient to insist, in the meantime, upon a complete homology of the component parts. Thus, the general relations of germ-cell, vitelline substance, and vitelline membrane, are the same in most animals; but, to say nothing of differences in the first of these parts and its macula, it is apparent from what has been previously stated, that the vitelline substance is of two kinds at least in animals, viz., the primitive, finely granular, or formative; and the secondary, largely cellular, or nutritive yolk; and the vitelline membrane is either a primary zona or membranous condensation of the outer part of the primitive yolk substance, or it is formed secondarily outside the nutritive yolk from the external layer of the granular cells of the ovarian follicle. I am glad to have in support of this view similar opinions from Professor Owen privately communicated to me.

Description of the Figures in the Plate.

Fig. 1. Early condition of the ovum in the ovarian follicle of a bird or mammal; *f*, wall of the primitive follicle (ovisac of M. Barry); *c*, cells which line it, forming afterwards membrana granulosa, etc.; *g*, germinal vesicle or germ-cell, with a single macula or nucleus; *y*, primitive yolk substance, consisting of small highly-refracting granules round the germinal vesicle, and deposited in a clear gelatinous matter.

Fig. 2. A somewhat more advanced stage in the bird—about $\frac{1}{200}$ " in diameter; *y*, granular or primitive yolk substance increased in quantity, and collected into a globular form; *z*, clear external portion, bounded by a dotted line, indicating its outer surface, which afterwards becomes consolidated into the zona pellucida, or covering of the primitive ovum; *g*, the germinal vesicle; its macula now become subdivided.

Fig. 3. Ovarian ovum of the fowl in its ovi-capsule, about $\frac{1}{20}$ " in diameter; *o*, substance of the ovary; *f*, inner surface of the ovarian follicle; *c*, the nucleated cells of the follicle, become prismatic, surrounding completely the primitive ovum; at *c'*, a part of these cells is represented as they are seen when brought into focus over the ovum; elsewhere they are represented only in profile, or as in a section; *y*, finely granular primitive yolk substance; *z*, zona or primitive vitelline membrane, which is of temporary existence in the bird, but permanent in the mammal; *g*, germinal vesicle.

Fig. 4. Small portion of the ovarian ovum of the fowl from near the surface, diameter of the ovum about $\frac{1}{200}$ "; *y*, the primitive or finely granular yolk substance, which is still in the form of a spherical mass, and encloses the germinal vesicle; *z*, the zona pellucida or primitive vitelline membrane, analogous to that of the mammiferous ovum, but not presenting the same clearness nor consistence; *c, c*, the prismatic cells of the ovarian follicle seen in profile; *c'*, a small portion of the same seen endways on the surface; at *c'* this layer is partially drawn off from the surface of the zona; *m, v*, a thin clear film visible on the outer surface of the prismatic cells, in the place afterwards occupied by the secondary or outer vitelline membrane.

Figs. 5 and 6 are diagrammatic views in section of the ovarian capsules, etc. in the bird and mammal near the stage of maturity, intended to show the relation of position of the parts to each other, but without any attempt to represent the true relative size of the parts.

Fig. 5. Bird; *o*, ovarian capsule at its pedicle; *f*, the wall of the ovarian follicle; *m, v*, secondary or permanent vitelline membrane; *c*, layer of prismatic

cells covering the germ and yolk, now proportionally much expanded and surrounding *c, y*, the large mass of the cellular or nutritive yolk; *y, g*, germinal vesicle in the vitelline disc (formative yolk), or remains of the primitive ovum.

Fig. 6. Portion of the ovary of a mammal; *o*, ovarian substance; *f*, inner surface of the ovarian follicle; *c*, the cells of the membrana granulosa; *l*, the fluid of the follicle; *x, y, g*, the ovum with zona, granular (or formative) yolk substance and germinal vesicle; *l'*, a less advanced follicle and ovum.

ARTICLE II.—*Notes of Hospital Practice.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine, and of Clinical Medicine, in the University of Edinburgh.

EXPULSION OF THE *TENIA SOLIUM* BY THE ÆTHERIAL EXTRACT OF THE MALE SHIELD FERN.

OF all the vermifuge remedies proposed for the expulsion of tape-worm, I have found the ætherial extract of the male shield fern the most effectual. That it readily dislodges large masses of the parasite, has been witnessed by all who have tried it, although in every instance it has not succeeded in permanently preventing a return. In the following case it seems, at all events for a time, to have been most effectual:—

Catherine Watt, æt. 25, married, with children, admitted into the clinical ward November 20th, 1854. She has always enjoyed good health, until three years ago, when joints of tape-worm passed from her involuntarily when out working, and they have continued to pass from her involuntarily, and sometimes in large quantities by stool ever since. On one occasion she passed blood at stool, with portions of tape-worm. Has taken various kinds of medicine, but, with the exception of turpentine, does not know what they were. They have all been ineffectual. On admission, complained of tenderness in the left iliac region, and of tenesmus when at stool; but, with the further exception of the frequent passage of joints of tape-worm, the functions of the body were performed with regularity. She was ordered ℥ij of the ætherial extract of the male shield fern, to be followed in the morning by ℥j of castor oil. This caused the evacuation of seven joints of the worm, each of which were longer than they were broad. Another ℥j dose of the extract were ordered at night, also to be followed by ℥j of castor oil in the morning. Nov. 22.—Only three joints of the worm passed. To have this evening 5ss. of the extract. Nov. 23.—This morning, after taking the oz. dose of castor oil, she passed many separate joints, and several long portions of *tœnia*. The whole together, when measured, was calculated to be about fifteen yards long. One portion was evidently formed of the joints of the worm near the head, as they were broader than they were long, and not above the tenth of an inch in breadth. Some joints were square, and others longer than they were broad, measuring from half an inch to three quarters of an inch in length. No head could be discovered, though carefully searched after. She remained in the house till the 6th of December; but although she took 5ss. of the extract three times, and one dose of ℥ij, no more joints of the worm came away. This woman has returned to the hospital, twice since her dismissal, to say that she is quite well.

In the last volume of this Journal, p. 35, Dr Paterson of Tiverton has recorded three cases of tape-worm, which resisted the action of the male shield fern, of the kouso, and of turpentine. It would

be interesting to ascertain whether the parasite is more common in Devonshire than it is in Scotland. In Devon, pork is a very common article of diet, whilst in Scotland certainly it is not much employed as food. I carefully interrogated the woman, whose case is given above, as to whether she had eaten pork, and she admitted, that about the time the disease commenced, her husband being out of work, her diet had been very poor, had consisted in some measure of salt pork, and occasionally of rabbits. Whether the *cysticercus cellulosa*, commonly found in the flesh of pigs, could have retained its vitality in the salt pork eaten by this woman, cannot of course be stated with certainty. But it is worthy of remark, that the flesh of pork is frequently sold cheap to the lower orders, after it has been laid in brine for a very short period, so that the tenacious vitality of these cysticerci, or of the ova of *taenia*, is by no means necessarily destroyed. Then rabbits are known to be very commonly infested with cysticerci, so that her indulgence in either kind of animal food may have been the means of introducing *taenia* into the economy.

The recent observations of Siebold, Van Benedin, Nelson, Kuchenmeister, and others,¹ have now satisfactorily shown that the ova of tape-worms, often find their way into the alimentary canal of various animals, and are, in the different tissues of their bodies, converted into cystic worms. Those, existing in the blood and flesh of such animals as are used for the food of man, are, in his alimentary canal further transformed into *taenia*. Thus the flesh of sheep, pigs, or rabbits, and perhaps the water of certain pools to which infected animals have access, may be made the medium of conveying *taenia*, in an early stage of development, into the human intestines. In future, therefore, the observations of medical men should be more particularly directed to the food and drink of their patients, so as to cut off the origin of the disease. Is it not possible that in such cases as have been recorded by Dr Paterson, where the patients, after taking vermifuge remedies, enjoyed immunity for several months, that an indulgence in pork may have caused a re-introduction of *cysticerci*, and a new formation of tape-worms? This is a point which, in pork-eating counties, such as Devonshire, seems to me worthy of further investigation. Should this supposition prove correct, it would explain the fact, that whilst, in the experience of Dr Paterson, the worm is difficult to eradicate—in Scotland, according to the observations of Dr Christison, and of myself, the oleo-resin of the male shield fern is more effectual in its operation.

EPILEPTIC CONVULSIONS—HEMIPLEGIA OF THE RIGHT SIDE—LOSS OF SMELL—BLINDNESS OF THE LEFT EYE—AMYLOID BODIES IN THE BRAIN.

John Bookless, æt. 48, a plasterer, admitted January 7, 1855. He has en-

¹ Monthly Journal, June 1852, p. 561.

joyed good health until two years ago, when he first complained of giddiness and gradual impairment of sight, and of smell. Twelve months ago he was attacked with "fits," three or four appearing in the course of the first night. They have occurred occasionally, at considerable but irregular intervals, ever since. His general health has remained good, until the 3d instant, when, about 12 o'clock at night, a violent "fit" appeared, which was repeated from eighteen to twenty times before six o'clock on the following morning. On the 4th and 5th he was comparatively free from them; but, on the 6th, during the night, they recurred more frequently. On the morning of the 7th, it was observed that the right arm and leg were paralyzed, and he was sent into the Infirmary.

On admission, it was observed that the body was tolerably robust; that he was hemiplegic on the right side; that the head was obstinately kept turned towards the right side; that speech was slow and thick; and that although conscious, he was some time in framing an answer to a question. *To have 3j of castor oil.* Careful investigation on the following day elicited the following facts, viz., complete blindness of the left eye—sight in the right eye perfect—smell absent—cephalgia—frequently applies his left hand to the left side of the head—other special senses normal—loss of voluntary motion over right side, with considerable impairment, but not absence of sensibility—left side normal—pulse 96, full—other functions healthy. Bowels have been freely open, from the action of the castor oil. Whilst I was examining the patient he passed through two attacks of an epileptic character—there was no scream, only a slight groan—the muscles of all the limbs became rigid—the toes and fingers incurved—the face flushed, and the head tetanically twisted towards the right side—the mouth was drawn somewhat to the left—the left arm and leg convulsed, the right arm and leg rigid and trembling—there was complete loss of consciousness. This state continued about one minute, when the face became pale, there was foaming at the mouth, the rigidity and convulsions subsided, and in another minute he was again conscious and fully restored to his former condition. *To be cupped in the neck to the extent of 8 oz.—ice to be applied to the head.* From this period he lay, in the intervals of the attacks, tolerably tranquil—the evacuations were passed involuntarily—took nourishment without difficulty. The whole of the 12th was free from convulsive attacks, but on the 13th they returned—pulse 106, soft. *A blister to the neck, and 3iv of wine.* On the 14th the epileptic attacks returned every ten minutes, until 1 o'clock in the morning of the 15th. From this time he remained free from them. At the visit he was still conscious, slowly answered questions, put out his tongue, etc. The respirations, however, were slightly laboured, and gradually became more so, until he sank, at 9 P.M., on the 16th.

Sectio Cadaveris, January 17.—On removing the calvarium, the subarachnoid cellular tissue was infiltrated with serum, which elevated the arachnoid in some places above the level of the convolutions. On slicing the brain from above downwards, its substance was healthy. Both lateral ventricles were distended with clear serum, which, on being carefully removed with a pipette, measured 1 oz. and 7 drachms. The ventricles were somewhat enlarged, but their lining walls healthy. The foramen of Monro was the size of a fourpenny piece, its edges very thin. White substance of the fornix and central portion of the brain healthy. The left corpus striatum atrophied and shrunk throughout, externally of a dull mahogany colour, and, on section, composed of a diffuent fawn-coloured substance, which flowed out, leaving an irregular cavity the size of a hazel nut. Below the left corpus striatum, the optic thalamus presented, on section, a cribiform appearance, over a space the size of a shilling, dependent on chronic enlargement and thickening of small vessels, the open mouths of which, on being cut, were retracted into its substance. In the anterior portion of the right corpus striatum there was also a diffuent softening, occupying a space about the size of a pea. On removing the cerebral lobes from the cranium, a dense chronic adhesion, which it was necessary to cut through,

existed between the inferior surface of the left anterior lobe and the dura mater, which involved the optic and olfactory nerves of that side, and extended so far on the right side as to include also the right olfactory nerve. The portion of brain in immediate connection with this adhesion was unusually indurated to the feel, throughout a portion of substance in the left lobe, about the size of a nutmeg; but, in the right, confined to a thin layer of cerebral substance externally, about an eighth of an inch in thickness, and about the size of a shilling in its area. On cutting through the indurated substance on the left side, it felt like soft bees-wax under the knife, was of a very pale straw colour, gradually disappearing, as did the induration into the healthy structure, without any obvious limit whatever. About another oz. of sanguineous serum was found collected in the depending portions of the cranial cavity after the brain was removed. The other portions of the brain were healthy.

Thoracic and abdominal viscera healthy.

Microscopic Examination.—The fawn-coloured softenings in the corpora striata consisted of numerous molecules, granules, granular masses, and cells, mingled with vessels coated with granular exudation, and fragments of the tubes of the cerebral substance. In and around the cribriform alteration of the left optic thalamus, numerous round colourless transparent bodies were observed, which refracted light strongly, and were apparently solid. They varied in size, from the $\frac{1}{1000}$ th to the $\frac{1}{2000}$ th of an inch in diameter. Some contained an included globular body, around which faint concentric circles were discernible. On the addition of diluted sulphuric acid and iodine, they did not give the reaction of starch or cellulose. They were unaffected by water, acetic and nitric acids. Here and there they seemed to split up, not unlike starch bodies. The indurated portion of brain in the anterior lobes presented an obscure amorphous appearance, consisting apparently of the normal elements, infiltrated with a brownish, exceedingly fine, molecular substance. The serum of the ventricles only contained a few epithelial cells, distended with water by endosmose.

The symptoms observed during the life of this man were all clearly explained by the morbid changes demonstrated after death. Before the post-mortem examination took place, I ventured to diagnose chronic softening of the left corpus striatum, with a tumour so situated below it as to press upon the left optic nerve, and both olfactory nerves. Such were the principal lesions discovered, as the indurated brain and dense adhesion may in one sense be looked upon as a tumour, producing the destruction of the special nerves, whilst the extensive lesion of the left corpus striatum sufficiently explained the hemiplegia on the right side of the body. Two other lesions, however, were discovered, viz., 1st, The limited disease in the right striated body; and, 2dly, The effusion of serum into the lateral ventricles and subarachnoid cavity. To the first of these lesions may probably be ascribed the convulsions which more especially attacked the left side of the body, although alone this would be insufficient to account for its paroxysmal character,—a phenomenon which I have elsewhere endeavoured to explain, can only be referred to congestions within the cranium.¹ As to the effusion of serum, I am inclined to consider it as having occurred during the last few hours of life;—1st, Because he was conscious within twelve hours of his death, and was free from delirium and stupor; 2dly, Because, after death, little im-

¹ See Articles by the writer on Apoplexy, Epilepsy, etc., in the second volume of the Library of Medicine.

bition of serum had taken place into the central white substance of the brain, and there was consequently no softening from maceration.

The histological changes described in the textures of the brain, are exactly the same as those which I connected with clinical researches into the nature of diseases of the nervous centres, in 1842-3. Since that time it has been supposed by some pathologists that the fawn-coloured softenings are more allied to fatty degenerations than to inflammations. But, notwithstanding the authority of some who have maintained this doctrine, I still believe that it is not a mere fatty metamorphosis, either of the vessels or of the cerebral substance, which occasions the change, but rather a transformation of the exudation, which has been poured forth from the former. Since those researches were published, I have had numerous opportunities also of determining the histological character of indurations of the brain, which have always been seen by me to depend on a firm and finely coagulated exudation, thrown out between the normal elements of the cerebral texture.

Within the last two or three years, however, considerable interest has been attached to the *corpora amylacea*, or amyloid bodies, first noticed by Purkinje and Valentin, and since carefully described and figured by myself in the March No. of this Journal for 1848, p. 661.¹ It has now been ascertained, that whilst some of these bodies are essentially a mineral impregnation upon an organic basis, others possess all the reactions of starch or cellulose, and are changed into a beautiful blue on the addition of weak sulphuric acid and a watery solution of iodine. This remarkable fact was discovered by Virchow, and may be considered to be fully established by the subsequent observations of Busk and others. At the commencement of this session, my assistant, Mr Carter, showed me these bodies changed into a beautiful blue by iodine. They had been taken from an indurated tumour in the brain. In the present case the amyloid bodies, although they exactly resembled the appearance of those which are changed blue by iodine, were found to be unaffected by it. The true significance of these curious structures, in their relation to physiology, pathology, and clinical medicine, has yet to be determined.

ACUTE ARTICULAR RHEUMATISM—MULTIPLE ABSCESES IN THE JOINTS, IN THE MUSCLES, WITHIN THE CRANIUM, ETC. WHAT IS PYÆMIA ?

James Lockie, æt. 17, a rope-spinner, admitted December 1, 1854. Ten days ago, when spinning ropes in the open air, he was exposed to more than usual cold and wet. Next day rigors and other febrile symptoms appeared, followed by pain, redness and swelling of the right elbow joints. During the four following days the right wrist and ankle joints were also affected, together with both knee joints. Four days before admission the heart's action became very violent, and leeches were applied to the precordial region. The pain and swelling of the joints have continued since.

¹ See also Cancerous and Cancroid Growths, p. 76, figs. 81-84.

On admission he complained of great pain in the right wrist, ankle, and left shoulder joints, which were swollen, immoveable, doughy to the feel, tender to the touch, with the integuments over them erythematous. From the left shoulder joint, the swelling extended into the axilla and down the inside of the arm. Pulse 130, full and strong—heart's impulse violent, but no blowing murmur. The tongue coated with brown in the centre and white at the edges—no appetite—great thirst—skin hot and dry—urine turbid from excess of lithates—bowels open—no headache, and the other functions normal. *Fistula venesectio ad 3riv.* \mathcal{R} Potassae nitratis 3ss. aquae 3vj. solve.—3ss. to be taken in half a tumblerful of water every four hours—warm saturnine lotions to the inflamed joints. December 2d.—Little change, pulse 120, more soft, blood not buffed, but it was drawn from a small orifice. December 4th.—Pain in all the joints greatly diminished, the swelling, however, continues. A blister has formed over the external malleolus of right ankle—complains of soreness in the heels. Pulse 100, of good strength. No blowing murmur with the heart's sounds. Took 3j. of castor oil last night, (the bowels having been constipated), which has acted copiously. Tongue dry, and covered with a brown fur. Febrile symptoms continue, with profuse diaphoresis. On the 6th December the blister over the malleolus of right ankle burst, and gave issue to a quantity of pus. Distinct fluctuation existed over the right wrist and dorsum of the hand, which was opened by an incision, and also gave exit to considerable quantity of pus. To omit the nitrate of potash. On the 8th, complained of pain in the back of the neck, and a bed sore was seen to be forming over the sacrum. To be placed on the water bed. From this time the pulse, which ranged from 110 to 140, lost its fulness, and became much more weak; the skin assumed a dirty yellowish or tawny hue, the typhoid febrile symptoms continued, with dry tongue and sordes, and numerous abscesses formed in the joints and various parts of the body, several of which, as soon as they became soft, were opened. A very large abscess formed over the occiput, which was opened on the 18th, and another over the manubrium of the sternum, extending up the left side of the neck, which was opened on the 24th. The skin over the heels, trochanter of the right hip, and the sacrum sloughed, notwithstanding every care taken to prevent it. On the 26th, the whole of the right lower extremity was swollen, cedematous, and white, resembling in aspect phlegmasia dolens, there was laborious breathing, and great prostration. Low muttering delirium, and involuntary evacuations supervened, and he sank on the morning of the 27th. The treatment had latterly been directed by generous diet and stimuli, to support his strength, relieve pressure on depending parts, and to dressing his sores.

Sectio Cadaveris.—December 30, 1854.—Body greatly emaciated, a fistulous opening, the size of a shilling, existed immediately in front of the left sterno clavicular articulation. Other sores, varying in size from a half-an-inch to 3 inches in diameter, and laying bare the bones, existed over the right elbow, ankle, both hip joints, right knee, and sacrum.

Head.—The integument covering the occiput was separated from the skull, infiltrated with putrid pus, a great quantity of which had been evacuated by openings previously made. On removing the calvarium, an abscess, containing thick yellow pus, existed between the bone and dura mater, about the centre of the occipital bone. The bone externally was somewhat carious, but internally it was healthy. No communication could be traced between the external and internal abscesses. *Brain.*—Healthy.

Chest.—On removing the heart and aorta, a fluctuating oval swelling, about $\frac{3}{4}$ inch in its long diameter, was situated outside the aorta, about an inch from the aortic valves, which was distended with yellow purulent matter. The posterior portions of both inferior lobes of the lungs were condensed. On section they presented a reddish purple colour, the air vesicles filled with a soft sanguineous exudation and readily sinking in water. *Heart.*—Healthy.

Abdomen.—Kidneys slightly enlarged—one section presenting a whitish mottled appearance, without great atrophy of the secreting, or encroachment on the tubular substance. Other abdominal organs healthy.

Joints.—The left sterno-clavicular articulation was carious and disarticulated, with matter burrowing to considerable depths in the surrounding soft textures. The right shoulder, left elbow, right wrist, both hip joints, both knees, and both ankle joints, were filled with dirty purulent looking matter, which, in several instances, more especially in the left elbow and hip-joints, had infiltrated itself more than half way down the fore arm and thigh. The various articular cartilages presented all stages of abrasion, softening and ulceration, whilst the osseous textures below exhibited a curious and blackened necrosed condition. The base of the ulcer over the sacrum, consisted of necrosed bone and over the right elbow, right hip, and knee-joints, bone was exposed and necrosed.

The veins were carefully examined, especially in the right inguinal region, and with the sinuses at the base of the brain, were everywhere found healthy, and free from coagula; indeed, the blood was every where unusually fluid—even in the heart, presenting small, dark and soft coagula.

Microscopic Examination.—The pus consisted of molecular and granular matter with debris of disintegrated pus cells, with the exception of the abscess within the cranium, the pus of which was normal. The cartilage covering the joints was in some places healthy, but in others its cells were enlarged, filled with secondary cells, and not unfrequently with fatty granules. Around the articulations of the joints were laminae of chronic exudation, consisting of dense amorphous matter, principally composed of minute molecules. The blood was carefully examined, and every where found normal.

This was a case of what is commonly called pyæmia, and is not uncommon from the result of mechanical injuries, or suppurative diseases. I believe it to be very rare, however, following attacks of acute rheumatism, such as all the symptoms and the history of this case prove it to have been. The lad was healthy, and in pursuit of his ordinary occupation when, after exposure to cold and wet, he was seized with the usual symptoms of rheumatic fever, including violent action of the heart, and on this supervened suppuration in almost all the joints, with numerous abscesses, accompanied by a low typhoid fever, under the effects of which he sank. Dr Watson has recorded two cases singularly like it, but in them the constitutional disease was preceded by otorrhœa and abscess in the ear,¹ to which he theoretically ascribes the origin of the disease. In the present case there was no primary abscess, no evidence of a pre-existing collection of pus before the attack of rheumatism, and I think there can be little doubt that the constitutional state of the blood, whatever that is, was dependent on the abscesses, which resulted from the acute inflammation of the joints.

If by pyæmia be understood that pus, as pus, enters the blood—in other words, that the corpuscles of that fluid mingle and circulate with those of the blood—the idea is erroneous. Such mingling of pus cells produce no such effects as are witnessed in these cases. In leucocythemia we observe the blood to be loaded with corpuscles, which, in all their physical characters, are identical with those of

¹ Practice of Physic, vol. i. p. 372, 3d edition.

pus, and yet none of the supposed obstructions in the smaller vessels, and no multiple abscesses ever occur. Nor when healthy pus is injected into the veins of a living animal, do these results take place, as has been proved by numerous experimenters, and of which I satisfied myself by some careful experiments, performed with Professor Barlow of the Veterinary College of this city.¹ But if by pyæmia be understood a morbid poison generated in pus, which, on its absorption, taints the system and leads to the secondary abscesses, then the idea seems to me to be correct. I think, however, it would be better, with Boyer, Bonnet, Darcet, and Bérard, to speak rather of a purulent infection than of a purulent absorption; for direct experiments prove that putrid pus produces the effect that healthy pus does not. Besides, we know that morbid pus of various kinds when absorbed into the system, such as that of small-pox, glanders, syphilis, etc., not only induces the constitutional fever, but tends to the production of secondary collections of pus, whereby the poison is supposed to be eliminated. On the whole, a careful consideration of this difficult subject has led me to the conclusion, that such cases as the one now recorded, are not owing to simple admixture of pus with the blood, or to phlebitis, but to the presence of a peculiar poison which contaminates the system.

THE CRACKED POT SOUND (*bruit de pot fêlé*) IS NOT ALWAYS
INDICATIVE OF A CAVERN.

On examining a man called John M'Kay, on the 2d of November 1854, it was determined on percussion of the apex of the left lung anteriorly, that it was unusually resonant, and accompanied by distinct cracked pot sound, of a clanging character. On auscultation over the part, the inspiration was harsh and exaggerated, the expiration prolonged, and the vocal resonance very loud, amounting to pectoriloquy. This unusual combination of physical signs attracted my attention. The rest of the lung, as well as the condition of the opposite one, together with the symptoms, could leave little doubt that this man was in the last stage of phthisis pulmonalis. The disease followed its usual progress, both lungs being ultimately affected, and the apex of the left one always presenting on percussion the same physical characters as have just been described, without moist rattle of any kind. He died January 6th, 1855, and on the following day his body was examined. Both lungs were scattered throughout with miliary and infiltrated tubercle, and the intervening pulmonary tissue was loaded here and there with simple exudation. At the apex of the left lung was a mass of bluish-grey miliary tubercles, about the size of a hen's egg, closely condensed together, very hard to the feel, and cutting under the knife with considerable resistance. This mass was solid throughout, tolerably limited, the pulmonary tissue around being light and spongy, con-

¹ Monthly Journal, January and March, 1853, pp. 80, and 272, 273.

taining a very few small isolated tubercles. There was no cavity or softened tubercle in this left lung, although, in the opposite one, a few small anfractuous excavations existed here and there in the upper lobe.

According to Skoda,¹ "the cracked pot sound is heard in the thorax, over tolerably large and superficially situated cavities which contain air, and communicate with the bronchial tubes. When the percussion is forcible, or the thoracic walls flexible, the cavity is compressed at each stroke, and a portion of air suddenly driven out of it into the bronchial tubes; this hissing murmur, caused by the escaping air, is mixed up with the ordinary percussion sound of cavities, and this compound represents the cracked pot sound." But the observation made in the case of M'Kay, has satisfied me that occasionally distinct cracked pot sound may be elicited over condensed lung, without any cavity whatever. In referring to an excellent paper on this subject by Dr Markham,² I find that on one occasion, both he and Dr Sibson have noticed this phenomenon, over the upper portion of a lung, which was afterwards shown to be gorged with blood and serum, though still retaining some portion of air. It would seem, from what has been said by Skoda, Stokes, Walshe, as well as by Dr Markham, that a peculiar tympanitic sound may be heard over collapsed or condensed lungs, when covered or mingled with a certain amount of air. For instance, when in cases of pleuritic exudation, air is effused into the pleura, a few hours before death—when in certain cases of pneumonia there is also emphysema, etc. In the case I have alluded to, these conditions were so far fulfilled, that the tubercular mass described was surrounded by spongy lung full of air. At all events, it must be evident, that the physical conditions on which this peculiar sound depends, require more careful study, and that our ideas as to its necessary connection with a cavity, must undergo modification.

TRIAL OF PHOSPHORUS IN PARAPLEGIA.

I have given phosphorus in seven cases of paraplegia from disease of the spinal cord, in the form of phosphuretted oil (4 gr. of phosphorus dissolved in 3j of olive oil.) In none of these cases have I been able to satisfy myself that any improvement was occasioned. I commenced with three drops a day, which were afterwards cautiously increased to ten, and in one case to fifteen drops. But these large doses soon induced violent nausea and vomiting, and, after a short suspension of the remedy, I have continued it in doses of three drops.

In the case of chronic myelitis, which took ten and fifteen drops, the phosphorus was excreted by the lungs, as the breath smelt strongly of the drug, but was not phosphorescent at night—a phenomenon which has been seen by some physicians who have employed it. In

¹ Markham's translation, p. 21.

² Monthly Journal for June 1853, p. 495.

another case, that of a delicate woman, with probably congestion of the cord, a large amount of phosphate was passed in the urine, presenting, under the microscope, beautiful feathery crystals. The dose of the oil, therapeutically, ought never to exceed five drops, and even this amount cannot be administered for any length of time without deranging the stomach.

ABSENCE OF CHLORIDES IN THE URINE.

In one or two cases of pneumonia, in which the disease was progressing, traces of chlorides were seen in the urine. This was discovered, by Mr Seymour (clinical clerk), to depend on an adulteration of the nitric acid, which, for testing urine, must be pure. The nitric should be tested according to the directions of the Edinburgh Pharmacopœia for hydrochloric acid, with which it is very apt to be mingled. It is of importance that pure nitric acid be added to the urine in the first instance, otherwise the nitrate of silver is very apt to throw down phosphates, which, however, may be distinguished from chlorides by being dissolved in an excess of nitric acid, which does not affect the latter salts.

ARTICLE III.—*Strychnine Poisoning.* By HENRY LONSDALE, M.D. Edin., F.R.C.P., Physician to the Cumberland Infirmary, etc.

CASES of poisoning by strychnia are comparatively rare in England, and from the few data on record, it may be supposed that the same remark holds good on the Continent and America. The reasons for this rarity are conspicuous enough; 1st, In this country strychnine was hardly known out of the professional circle till three years ago, when the "bitter beer puff" obtained for it some notoriety. 2d, The difficulty of procuring (except from the highly careless druggist) an agent so active and deleterious as a poison, without raising suspicion in the mind of the vendor.¹ 3d, If known as a poison, the rapid and specific effects of strychnine which has no analogue in action to any known animal or vegetable poison, would be a sufficient bar to its adoption by the multitude; in other words, strychnine will hardly ever become a popular poison like arsenic, opium, etc.

The infrequency of strychnine poisoning, and some peculiar circumstances attached to the history of the following case induce me

¹ Perhaps this is saying too much for the drug salesman. Arsenic used to be obtained, and readily too, as a rat poison. The last person hung at Carlisle for arsenical poisoning of his wife, went on two different occasions and bought his pennyworth of the drug, and without a question being asked. He was a labouring man and quite unknown at the shop. During the last month I know that a stranger dressed as a countryman obtained an ounce of opium at one chemist's shop, and half an ounce at another; there was not the slightest demur on the part of the vendor.

to record it for the benefit of toxicological inquirers. The case occurred at Longtown on the 28th November 1854.

Matthew Ferguson, aged 59 years, a man of large frame and strong constitution, a weaver by trade, but fonder of angling during his earlier and middle manhood; rose at his usual hour on the morning of the 28th, and walked down to the river Esk to wash himself; seemed in his usual health, and made no complaint to his wife or sons. About eight o'clock he is seen walking home, and on reaching his own house he complains of being severely held, is violently cramped, and declares himself dying. A doctor is immediately called who prescribes a pill, but the man, after exhibiting some marked symptoms, expires within thirty or forty minutes of his arrival at home.

The little town had scarcely awoke from its slumbers when the news of Matthew Ferguson's "awfu' sudden death" traversed every nook and corner. Rumour with its thousand tongues could explain it all; "he was an old man, had long suffered from boils, piles, and other inward complaints, had often sought the aid of a county charity, and that very morning had been to the river to wash his body, the cold had struck him, and he was done for at last. Poor Matthew!"

The funeral was in due preparation, and would have been carried into effect had the circumstances occurred across the Scotch dyke, about four miles north of Longtown. But on the English side of the border¹ we require a medical certificate of the cause of death, and the doctor demurs; moreover it becomes known that the deceased had been to a druggist's shop that morning, so now there is great talk and disputation, nay, violent harangues, and "crownor's quest law" is demanded and forthwith obtained.

On the 29th, the deputy coroner and jury sat *supervisum corporis*. To the kindness of Mr Lee, the coroner, I am indebted for the following notes:—

G. F., the son of the deceased, deposed that his father had been "troubled with piles and boils, and had been affected in his testicles, and that he had not been able to work for many years," that on the morning of the 28th, his father, after being to the river to wash himself awoke him, and complained of being severely cramped in his legs and body, and got into bed beside him. Dr Jardine was sent for, who prescribed a pill. The witness was not aware of his father getting anything from a druggist, but that Dr Rome came and inquired after his father's death if he had got anything from any one.

William Armstrong of Longtown, druggist, said,—“Yesterday morning about a quarter to eight o'clock, deceased came to my shop and asked for a strong dose of purgative medicine, and added five drops of croton oil. I refused to give him this, but gave him from *half a grain to a grain* of jalapine, I mixed it in honey and he took it in my presence. *I had frequently on previous occasions administered similar doses to other parties without any prejudicial effects that I am aware of.* Jalapine is a medicine not commonly used.”

Dr James Jardine said,—“About half-past eight yesterday morning I found deceased labouring under violent spasms which almost entirely subsided for from four to seven or eight minutes. During the spasms the body was extended with the limbs somewhat separated and stiff and rigid, and there was a violent shaking of the whole body. At first the spasms were most marked down the back and legs, but in the course of ten to fifteen minutes fixed upon the thorax, also about the same time violent tetanus supervened, and the next fit carried off the patient with violent tetanus and fixation of all the muscles of respiration; all attempts to restore animation by artificial means proved abortive. The heart's action was regular, though somewhat weaker than natural; there was a dryness of the mouth; there was no vomiting or purg-

¹ The Scottish “Registration of Deaths” Bill came into operation on the 1st January 1855.

ing, nor any morbid symptoms over abdomen felt or complained of during interval of paroxysms; but during the paroxysms the abdominal muscles were rigid. Deceased was certain of death impending, and was very anxious and agitated. His intellect was perfectly clear. Pupils slightly dilated, no marked distortion of features, saving a little protrusion of the eyeballs, which, during the last paroxysm were both rather inverted, and the mouth fast locked in tetanus. The face and hands were livid, and the surface below natural temperature. Saw the deceased about an hour after death; his arms were unnaturally rigid; the fingers inflexed, but not so as to grasp the palm." Dr J. had no experience of jalapine, and did not consider the symptoms to be caused by jalap, but was of opinion that deceased died from the effects of some narcotico-acrid poison."

The coroner ordered a *post-mortem* examination and an analysis of the contents of the stomach; and the inquest stood adjourned till the 4th of December.

Dr Jardine on his further deposition, said that he, assisted by Dr Rome and Mr Graham, surgeon, had made a *post-mortem* examination about thirty-three or thirty-six hours after death, and that they found nothing wrong with the brain and medulla oblongata; that the "heart was healthy in structure, empty and unnaturally atonic." That the lungs were much congested and friable in some parts; that the alimentary canal presented nothing morbid; "the mucous membrane over internal sphincter was congested without an irritable appearance." The other abdominal organs were normal except a slight congestion of left kidney. The left testicle was slightly enlarged and indurated. The lividity of skin so marked over face and hands, at and before death had almost entirely disappeared; no emaciation, and no marks of external injury. Dr J. was not prepared to say what was the cause of death, and from the healthy external appearance of the stomach was led to believe that any internal examination of it would throw no additional light upon the case without an analysis of the contents, and this analysis he declined to undertake. He considered the congestion of the lungs to be caused by asphyxia, and that the lungs were not apoplectic.

Dr Rome, who had assisted Dr Jardine in the *post-mortem* inquiry, deponed that about nine o'clock of the morning on which Mr Ferguson died, William Armstrong, druggist, called him out of bed, and said he had given Mr F. some jalapine, and that he was very ill. Before he, the Dr, got dressed, Armstrong returned to say it was all over with deceased. On asking Armstrong what quantity of the drug he had given him, he was told as far as he recollected, "a grain and a half." Dr R. concurred in the report given by Dr Jardine, but was of opinion that the flaccidity of the heart was such as to be unable to transmit the blood with sufficient force through the lungs, and that the lungs were of an apoplectic character. If on an analysis of the contents of the stomach no narcotic poison be found he would say that deceased died of apoplexy of the lung caused by the state in which we found the heart and not from the effects of jalapine. He thought the symptoms pointed to death by narcotic poisoning.

William Armstrong, on being further examined, said he got three grains of jalapine from Mr J. Todd, assistant to Wooley, wholesale druggist, Manchester, and that he gave half a grain to a young man, upon whom it had the desired effect of purging; *he gave the same jalapine to the deceased; the grain and a half which was left he had destroyed on the day that Ferguson died*, having previously shown the paper containing it to Dr Rome; said he had no strychnine in his shop.

As the medical men did not agree as to the cause of death, the coroner adjourned the inquest till Monday the 11th December, in order that the stomach might be analysed by Dr Lonsdale of Carlisle.

On the 4th, Andrew Davidson, policeman, delivered to me a sealed bottle containing stomach, and part of duodenum, etc. On

the 9th day after death, I took from the sealed bottle the stomach, with portions of œsophagus and duodenum attached. On exposing the parts, a strong odour marking decomposition was evolved; there was dark discolouration of the splenic end, and likewise of the gullet and duodenum. On making an incision along the smaller curvature, a quantity of foetid gas escaped. The gastric contents, in quantity not exceeding an ounce of thick greyish-looking fluid, had a slight acid reaction. The analysis I adopted was that recommended by Orfila and Barruel, as given in our great authority's work (Christison on Poisons), with the addition of the bichromate of potash test. In conducting these experiments I had the kind assistance of my young and highly intelligent friend Mr R. Brown, house-surgeon of the Cumberland Infirmary.

EXPERIMENT I.—(a.) Portions of the stomach and contents were boiled in water pretty strongly acidulated with sulphuric acid; the mixture was then filtered, and to the liquid thus obtained a quantity of carbonate of lime was added, sufficient to neutralize the acid. This was evaporated to dryness and digested with rectified spirit; after which it was again filtered, to remove all insoluble matter. The result—a clear liquid was then evaporated to the consistence of syrup, which had a slightly yellowish colour, and when tasted, was distinctly and persistently bitter.

(b.) To this alcoholic extract a few drops of strong sulphuric acid was added, and then a small quantity of powdered bichromate of potash. A purplish tint was at first observed, but it very speedily changed to a beautiful light green, which remained permanent.

(c.) A very small quantity of the above extract was mixed with sulphuric acid and peroxide of lead; but there was no appreciable change in colour.

(d.) A similar quantity was tried with the bichromate of potash test, with a very slight change of colour, hardly appreciable. With a large quantity the colour was unmistakeable. (*Vide* Experiment, b.)

EXPER. II.—To a cat was given in a piece of meat two grains of strychnine. A very small quantity of that was, however, taken by the animal. About two hours afterwards the animal was *found dead*. About forty hours after death the contents of the stomach were treated in exactly a similar manner to that detailed above, and the result was *precisely* the same as regards the development of the transient purple and permanent green colours with the bichromate of potash test.

EXPER. III.—A small crystal of strychnine was dissolved in a drachm of rectified spirit; and the bitter taste was exactly of the same character as the alcoholic extract derived from Experiment I.

EXPER. IV.—To an alcoholic solution of strychnine was added sulphuric acid and bichromate of potash, with the effect of producing at first a tinge of purple, rapidly succeeded by the green, quite undistinguishable from that obtained from the two stomachs. This green colour, as in the other experiments, remained.

N.B.—When the three experiments were placed alongside of each other, it was impossible to discover the slightest difference in colour. Three gentlemen were present to test the colours.

EXPER. V.—To a crystal of strychnine was added a drop of sulphuric acid, and then bichromate of potash. A beautiful purple colour was at once developed, which in about three hours became yellowish-brown. No green visible at any stage.

EXPER. VI.—Two very small strychnine crystals were dissolved in separate portions of sulphuric acid, mixed with one-hundredth part of nitric acid. To the one bichromate of potash was added; to the other, the peroxide of lead. The former showed at once the presence of the alkaloid; the latter gave no indication of its presence; but on the addition of a little more strychnine, a beautiful violet colour was displayed.

EXPER. VII.—Strychnine, to which strong hydrochloric acid had been added twelve hours previously, was unaffected by the bichromate of potash test.

EXPER. VIII.—Jalapine was subjected to the bichromate of potash test and sulphuric acid, also the peroxide of lead; but there were no results worthy of note.

On the second adjournment of the inquest (December 11), the evidence of Dr Jardine and others was read to me; and I had no hesitation in giving my opinion that, from the symptoms observed by Dr Jardine, and the results of the chemical analysis and comparative experiments detailed above, that Matthew Ferguson died from the effects of strychnine.¹

On the second adjournment, the widow of deceased corroborated her son's testimony, and added, "Dr Jardine gave me a prescription for Mr Irving, druggist. I went to Armstrong, who said I had got to the wrong place, but he (Armstrong) did not tell me that he had given my husband anything that morning."

It is unnecessary to give minutely the summing up of the coroner. The verdict of the jury, after a few minutes deliberation, was, that the deceased had died from strychnine, administered by Armstrong in *mistake*.²

Several questions arise out of the consideration of this case. Why did the poor man, on finding himself so ill, not mention the fact of his having been to a druggist's shop for physic? It appears from his history, that he was frequently complaining, and desirous of obtaining physic, and that he was a silent man in his

¹ The case of poisoning by strychnine recorded in *Medical Times and Gazette* (December 15, 1854), is worthy of perusal in connection with this case.

² Whilst claiming, as I have done, some credit for "Crown's quest-law," in England, in clearing up many unaccountable deaths which in Scotland might escape the "precognition" of the sheriff, it is but fair to admit that this official sifts everything to the bottom, and excuses neither ignorance nor wilfulness in the wrong administration of medicine. Scotland is an example to England in this respect.

own family; now, in all probability, he did not associate his suddenly alarming symptoms with the effects of an ordinary purge, whilst the imminent danger of death would hardly leave much time for thought and statement. The young druggist's presumption in prescribing jalapine, or indeed prescribing at all (for he can lay no claim to either experience or knowledge of the action of remedies), his making no mention of his administering medicine to M. F. when M. F.'s wife rushed into the shop—his hasty appearance at Dr Rome's bedside when he hears that M. F. is seriously ill—his destruction of the remaining contents of the paper from which he took the dose—his discrepancy of statement as to the frequency of his prescribing jalapine, with the *precise fact*¹ of his having obtained only three grains, and his instancing one man to whom he had given half a grain (and this man not discoverable) are circumstances upon which no comment is necessary. Moreover, the youth's antecedents are not flattering; during his apprenticeship he mistook *Tr. Opii*. for *Tr. Rhei*, and caused the death of a child.

Jalapine is rarely prescribed in Carlisle or the neighbourhood. The statement of the druggist that he found half a grain purge a countryman, is not reconcilable with the fact, that two grains have been given repeatedly to a child, and that four grains have been taken by a young adult of my acquaintance. At the inquest I stated that jalapine and strychnine resemble each other in external appearance, so as not to be detected by the non-professional observer, for whilst the crystals of the one, and the powder of the other would appear strong characteristics, it is common enough to meet with both in a pulverised state, and closely resembling each other in colour, and these specimens in respectable chemists' shops.²

The number of deaths annually caused by the ignorance and carelessness of unqualified persons, druggists, etc., must be considerable in this country, and the sad reflection awaits us that nothing is done by our legislators to prevent these fatalities. "Mantua's law was death" to those who sold poison (did this not include the ignorant who gave it by mistake?) but that was long ago in unenlightened Italy. At present it would be considered an interference with the liberty of the subject to have any such laws on our statute books. Did such restriction exist, burial clubs would have but a limited business,—the *post obits* of

"Some old lady
Or gentleman of seventy years complete."

¹ Mr Todd did send Armstrong three grains of jalapine.

² The examination of several specimens of strychnine and jalapine led to the following results:—Of strychnine there were three specimens of white crystals, four of white powder, two greyish-white powder, and one in crystals the same colour. Of jalapine there were four specimens of nearly white powder, three of a light brown resembling Dover's powder, and one of a *jalap brown* colour. In one shop the two drugs resembled each other so closely, as not to be distinguishable.

would be a kind of "hope deferred," and nuptial ties might remain indissoluble till natural death or chancery suits settled the matter, and these are tedious and expensive modes when you want the Gordian knot cut in Alexandrian fashion. It would appear to be part of our "glorious constitution" that every one, young or old, patrician or *sansculotte* should have it in his power to imitate the purchase, and verify the last words of Romeo :—

"Oh true apothecary!
Thy drugs are quick."

ARTICLE IV.—*An Experimental Inquiry into the Existence of a Sixth Sense, here called the Sense of Force.* By RICHARD F. BATTYE, Esq., London.

I.—UPON NERVES DISTRIBUTED TO SYNOVIAL MEMBRANES, AS
BEING A SPECIAL SEAT OF THIS SENSE.

IN the spring of 1844, my attention was first directed to the subject discussed in the present paper, from having been struck with the contrasting mechanism between the hand of a young Chimpanzee in Regent's Park Zoological Gardens, and that of man.

Though the homologies of the hand in the two beings are strictly numerical, to which indeed, in the bones of the extremities homology chiefly refers, yet, so far as function is concerned, no approximate analogy can be sustained; the thumb in the Chimpanzee being merely a counterpoise to the metacarpal region; but in man the same member is a counterpoise to the fingers, as well as to the metacarpal region.

From observing this marked proportional difference in the mechanism, and consequent function of the two instruments, I was strongly impressed with the conviction, that in man there exists some fixed and definite *geometrical* relations between the thumb, fingers, and palm of the hand, not common to the Chimpanzee. Of course, previous reading from various authors had led me naturally to suppose that *some* difference existed between the two, but not of that marked character which observation supplied.

Having been some weeks previously engaged in noting down a few reflections upon the structures and mechanism of the hand and foot in man; not having then, nor for several years after, read the beautiful and philosophical remarks made by Mr F. O. Ward upon the same instruments in his masterly, but disagreeably small work, *Human Osteology*; I was, from such previous exercise, the more disposed at once to attempt the reduction of theory to practice, and forthwith, after having arrived at my apartments, I tried a variety of measurements upon the hand with a pair of compasses, which resulted in demonstrating, at least, so I thought, the correctness of my previous hypothesis.

In the latter part of this paper, one series of these experiments will be given, the same which terminated in directing me to conceive the possibility, and finally, the probability of demonstrating by experiment, or induction, a sense hitherto, so far as the literature of Physiology is concerned, undefined and indeterminate, though most certainly suspected and anticipated; since it is well known that the late Dr Thomas Brown, from theoretical grounds conceived the possibility, or rather the probability of the existence of a sixth sense; and that Sir C. Bell, whose philosophic mind was fully awake to the value of such an hypothesis, especially from such a mind as Brown's, (himself nevertheless having, independent of Brown, arrived at the same theoretical conclusion), resolved upon applying experiment to elucidate theory upon the true Baconian system of induction.

It need not be said in what a glorious result that rude, but best mode of testing theory terminated, since it ended in the discovery of that arrangement in the nervous system which has justly placed him, with Magendie, amongst the foremost of physiologists. But, whilst mention is thus made of Sir C. Bell, it ought not to be forgotten, that to his last days he imagined that he had disclosed to demonstration the missing sense, which he conceived, as residing in the nerves distributed to the muscular tissue; and hence he called it *muscular sense*.

Without, then, positively contradicting or denying such a sense in muscles, it is not, perhaps, going too far to say that, if it could have been reduced to experiments of a more definite and crucial character, it would have been received with more confidence, than hitherto has been its lot.

More recently, Professor Weber of Berlin has made a series of experiments, to determine the relative degrees of cold and heat to which various parts of the integument are susceptible, and also of *weight*. The experiments very closely accord with similar experiments made by the writer previously to his first seeing them referred to in Todd and Bowman's, and more recently in Carpenter's and Valentin's (by Brinton) Physiologies, and he can only wonder that the prosecution of those experiments did not lead the learned professor to results of a more comprehensive and decisive nature. But, as it is truly said, the "battle is not to the strong, nor the race to the swift," an inscrutable Providence lying between human perfections and accomplishments and *success*, which alone can explain, in the present case, if the writer should be correct, why the palm was not borne away by a more erudite and accomplished brother in the profession, whilst walking on the path where its branches lay scattered abroad for him to gather, and carry away.

Besides the opinions of these two worthy authors, and their disciples, little has been advanced of a precise character upon a sixth sense, as standing in relation to force, and therefore, further anatomical and medical literature need not be canvassed upon the subject.

It only remains, then, to state the extent to which this sense is adapted to determine different kinds or degrees of force; and also, in so doing, what are the mechanical arrangements requisite for the determining such distinctions in force; since, for *kinds*, the regulating agents are considered to be strictly mechanical, though applied by, and performed through, the medium of living material. Before proceeding further, I will endeavour to explain what is here implied by the terms *kind* or *degree* of force in relation to sensation or feeling; and afterwards endeavour to apply them to the sense herein maintained and discussed as demonstrating, or, at least, as favouring the position or idea of its existence, leaving to experiment the province of direct demonstration and complete induction.

In the first place, we are conscious of two different kinds of feeling in reference to force. The one is that of having power or strength to readily, or with ease, overcome the obstacle or resistance desired to be surmounted; as a child, in handling a light cane, or throwing a small marble, feels that he has power to overcome its resistance at once, and with little effort. This gives to the user the feeling or consciousness of strength or power over that object; and indicates the superiority in such instance, of the muscular force over the gravitating to the child employing it. This, therefore, might be very appropriately called the feeling, or sense of *power* or *strength*.

On the other hand, by way of contrast, suppose a child, from ten to twelve years of age, has to lift a weight of from 28 to 42 lbs., he does it with difficulty, but by putting forth all his strength, he will accomplish it; or, in place of using two hands, he has to hold with one arm extended, a weight of from 3 to 4 lbs., the latter will be almost as difficult as the former, and in both instances the force employed to overcome the resistance will be put forth under circumstances of great effort and labour; and to the party so exercised, there will be a feeling or consciousness of great force being required to overcome the resisting body, which feeling is usually expressed by stating that the body is very heavy, or of great weight. Hence, this feeling or consciousness might be called the feeling or sense of *weight*.

Again, supposing an adult person applies his muscular force, aided by the mechanical arrangements of his osseous structures, to move a portion of rock, say 10 to 20 tons in weight, or to move a wall well built, and a foot and a half wide, he would be aware that neither object moved, though his eyes were shut; and, also, he would be aware that he was putting forth all the force his muscular system was capable of applying, to move the opposing obstacle, but without effect. This consciousness, or feeling within, of applying force, and applying it to its utmost extent, though no evident result follows in the body against which it is applied, might be called the consciousness or sense of *exercising* force, without any very accurate idea of the amount of that force, from the want of a standard whereby to judge.

In the cases above cited, a consciousness of *force* has existed in

connection with muscular action. In the first case, there was great *excess* of muscular force to the resistance to be surmounted, and is popularly called *strength* or *power*. The second case presents us with an instance of *little* excess of force remaining, when the resisting body, against which it has been applied, is overcome; and in popular language, the feeling within is expressed by describing the body without, as being overcome with difficulty by the force applied, and is variably described as the resistance being heavy, or of great *weight*. Whilst, in the third case, the obstacle to be overcome by muscular force, being, by virtue of its gravity, or attractive force, considerably plus over the muscular force, remains unaffected by the external force applied to it; and in current phraseology, is laconically described by the expressions, "it is too much for you;" "push away, my boy, you'll not hurt it;" or, "better save your breath for another time;" "it's no use trying;" all which expressions are only so many different ways of stating that muscular force is exercised, without any perceptible result following in the body against which it is applied.

In the foregoing sketch, relating to the effect of muscular action upon our feelings, I do not suspect that any will question the correctness of the statements therein advanced, or be disposed to deny, under the circumstances indicated, in a healthy person, that such feelings, as are there expressed, do exist; and that, in the occurrences of every day life, incidents are happening which lead to the expression of such feelings, when the incidents giving rise to them, place them before the mind, as here pointed out.

If, then, there are such sensations, etc., as have been indicated, following certain definite forms of muscular action, and those sensations are of a different nature from any with which we are supplied by the five recognised senses, what does it signify, but that there is a further source for our correct knowledge of the external world, than is to be found in the beautiful and exquisite channels of information, given to us in the orthodox senses—seeing, hearing, smelling, tasting, and touch?

But it will be said in reply, and with truth, that our knowledge of power or force, as Hume has affirmed, arises from our experience of known or observed phenomena being followed by uniform results. This the writer does not for a moment question; but for the knowledge of such facts, we are not indebted to sight alone, or to hearing, etc., etc., for both deaf and blind persons are equally conscious of possessing force, and in their actions accommodate themselves to the indications with which such feelings supply them. Hence, such abstractions leave the matter where they found it, and, in the present paper, their consideration may be respectfully declined.

Without, then, further ratiocination, a more varied mode of illustration may tend to throw the subject out into fuller relief.

Let it be supposed that there are five balls, each two inches in diameter, perfectly smooth, covered with a thin layer of caoutchouc,

to prevent the conduction of heat, and painted of one uniform colour. The first shall be composed of cork, the second of dry oak wood, the third of silver, the fourth of lead, and the fifth of platinum; and furthermore, let each ball be *accurately* fitted into a cup or die. In this condition neither sight, hearing, smelling, tasting, nor touching, could detect the difference between one ball and another; but let the same balls be placed in the hand, and muscular force be applied to counter-balance gravitating, and immediately one mystery will be solved, namely, that although all had one colour and size, yet, that each of them was composed of different material. And why such an inward conviction? Evidently because a feeling exists that each requires a different degree of muscular force to overcome the gravitating, as compared one with the other. Or, in other words, that each had a different *weight* compared one with the other, and, therefore, that each must be composed of different material.

The inference of their being composed of different materials, of course, depends upon previous experience; but that experience rests upon a feeling, which the conscious party describes as depending upon his *acquired* or *past* knowledge of weight in bodies, in other respects outwardly very similar.

If, then, there is a feeling within which will decide so curious and certain a result, and which none of the five senses could so do, what is it but that a different and independent sense determines what they have failed to accomplish?

Having thus far pleaded for the existence of a sixth sense, sufficiently varied to claim for it the more weighty examination of results, elicited from an extensive series of experiments, I shall, for convenience sake, though the inference of there being a sixth sense be still *sub judice*, refer to it as existing and real, and shall, forthwith, enter into an inquiry as to the means used by nature to regulate and determine the different results experienced in the sensations, when muscular force is applied to counterpoise gravitating.

Though the present paper limits the inquiry, with regard to anatomical region, to the nerves distributed to synovial membranes of joints, yet no attempt is here made to demonstrate their presence by dissection. The more so, from the fact that numerous authorities, Sharpey and Quain in particular, who agree in inferring the presence of sentient nerves in bone, ligament, and synovial membrane from their sensibility to injuries and acute pain when diseased, especially synovial membrane; yet none of the authorities consulted have been able to trace the ultimate nerve fibres, or their modes of distribution, in these tissues. Nay, even in the very analogous structure of serous membrane to that of synovial, the same want of demonstration exists, though so susceptible to acute pain from the slightest amount of inflammation, or any other form of disease that may affect it. But the fact that none of these structures are subject to great pain when mechanically irritated, only shows that the stimulus applied to their structure is not adapted to excite their

sentient powers; but that sudden injury of a mechanical nature can excite their sentient powers when properly applied, is most evident from the fact, that when loose cartilage is compressed between a joint—as the knee—most excruciating pain is immediately experienced.

It therefore being assumed that synovial membranes have sentient nerves, the inquiry of the present paper is limited to their *function*, and not to their course and distribution.

Again, on the other hand, it is not deemed necessary to discuss at length the *suitability* in the anatomical seat of nerves, distributed to synovial membranes, for taking cognizance of such a stimulus as force, when applied to their periphery, since, saving the sphincters, the occipito-frontalis, and a few muscles pertaining to the face, palate, œsophagus, the eye, and the ear, with their appendages, the whole of the muscles of animal life effect and complete their leverage force through the appliance of joints; and, therefore, whatever of force is applied by muscles, must, according to its amount, pass in like degree through these fulcra of bony structures; and, if nerves capable of taking cognizance of muscular force be there distributed, they will, by such locality, be best secured from mechanical violence, and most suitably placed to fulfil the office of their own peculiar function.

Without, then, any further remarks upon locality in relation to nerves of force, let the attention be rather directed to the mechanical arrangements that determine a recognition of *kinds* or *degrees* of force, as already explained.

The kinds or degrees of force which have been referred to, are recognised, it is conceived, by the different mechanical modes in which force is conveyed to the sentient nerves of joints, by the structures with which they are connected, as bone, fibro-cartilage, and true cartilage, the mechanical structure of each having a very definite and certain effect upon the mode or way in which force is made to impress the peripheral nerves.

As bone is the chief material through which muscular and gravitating forces are made to channel ere they reach the joints, a few remarks upon its mechanical structure, not microscopical texture, may be here very appropriately introduced.

Bony structure, as is well known, is hard, inelastic, or nearly so, and, according to its mechanical arrangement, presents different facilities for resisting external injuries; for instance, the long bones have two distinct kinds of mechanical arrangements, the shaft being composed of dense and compact material, whilst the extremities, as the femur and humerus, are composed of cancellous structure, or delicate bands of osseous tissue, arranged in an irregular net-like order, not very dissimilar to the siliceous matter or spicula of the poripheræ, denuded of their soft substance, by boiling in nitric acid. But the long bones do not all present this cancellated structure in equal degrees at either extremity, as the inferior extremity of the

tibia, compared with the superior; the radius and fibula, as compared with the humerus and femur. The texture of the short bones is very variable with regard to the amount of cancellated structure; but, as a rule, it may thus be stated, that in proportion to the smallness of the bone, so does the hard compact external layer become more developed, and the cancellated interior become more limited, and less marked, and the larger the short bones are, the more of the reverse of this rule holds good.

The design of such structures or mechanism, in addition to that of resisting force by its compactness in the shaft, and warding off sudden concussions in the joints, by the expanded and porous extremities diffusing the force applied to them, has assigned to it the further important function of regulating the *direction of force*; as in the long bones, when a force is communicated at one extremity it runs along in nearly a straight line, till it reaches the opposite extremity, where, by meeting with the cancellous structure at that extremity, according to its extent and delicacy, so must the force assume some *new course*, not in relation to the course or direction in which it was first received, but in the direction which the cancellated osseous structure will permit it; for of necessity, the force will channel along its structure, it being denser in its structure than the interstices, which are occupied with some softer substance or fluid, either liquid or gaseous; and, moreover, from the very direction or interlacing of the cancellous structure, much of the force will come to composition and resolution before it reaches the surface of the joint itself.

But, besides the different structures in bone regulating the direction of force, there are two other structures of great importance in relation to the present inquiry; the first of these is *true cartilage*, a tissue both hard and elastic, and whose structure is composed of cells arranged in a columnar order, very closely resembling in their regularity certain basaltic rocks, as those of Fingal's Cave and the Giant's Causeway. These columns being arranged in a line with the long axis of the bones, will of course transmit force received by them from the articular surface of bones to the synovial membranes, in the same direction in which it is applied; since, in addition to its being regular in structure, it is also *hard* and dense to a considerable degree. Hence, where joints are only tipped with true cartilage and not with articular fibro-cartilage, as in the ankle and wrist joints—save the ulnar border of the wrist joint—the nature of the osseous structure entering into the joints need only be considered in estimating in what direction force would reach the synovial membrane, after it has left the osseous tissue to pass through the cartilage, before reaching that membrane. Since, in whatever direction it was received by the cartilaginous structure, in the same direction—or very nearly so—it will be transmitted to the synovial membrane.

The remaining structure we have to attend to is the *articular fibro-cartilage*, which is found bordering and overlapping the true

cartilage in almost all the large joints, as the shoulder, hip, and knee-joints, etc. This structure, it need scarcely be mentioned, is anything but regular in its density or depth of substance, from within outwards, and is much more yielding and soft than true cartilage, though very elastic. The effect of such a structure would be, from its very softness and irregular density from within outwards, to vary and cast into some constant and uniform direction, any force which might be received upon its surface; of course, the completeness with which such new direction of force would be effected, would depend very much upon the thickness of the cartilage against which it first impinged; but this consideration, in a practical light, is of very little importance, since, in nearly every instance, where this substance enters into the composition of joints, cancellated osseous structure is associated with it, which has as great or greater modifying effect upon the direction of force, according to its amount in any particular joint, as has the articular fibro-cartilage, and therefore their modifying effect in relation to the *direction of force* in joints may be counted as unity, both tending to accomplish one and the same end. And it must be observed, that in large joints where the fibro-cartilage is the thickest, namely, at the circumference of joints, there also, namely, towards the external surface of joints, the cancellated structure becomes more dense and less spongy, and will therefore transmit force in the direction in which it is received more correctly than towards the centre or interior of the joint; but here the fibro-cartilage is a mere lamina or entirely absent, whilst towards the circumference, to countercheck the denser cancellated structure, it affords a deeper border for force to pass through.

Granted that cartilage, fibro-cartilage, and the different textures of osseous tissue have an effect upon the course or direction of force transmitted from the shaft to the joint, and that the combination of cancellated osseous structure at the extremities of bones, and articular fibro-cartilage, in its relative densities and thickness, tend to compel force, whilst passing through their structures into one uniform and constant line or direction, it will be very naturally asked, what is that line or course which force, so circumstanced, pursues? The probabilities are, that under such circumstances, it enters the joints in a *straight line*, or thereabouts, and the result of such direction, according to its intensity, is, that to the recipient, or conscious party, a *feeling or consciousness of power or strength is experienced*. Whilst, on the other hand, when the mechanism of the tissues entering into the formation of a joint, are so constituted as to admit of force entering into their structure assuming more directions or courses than one, that in such case, if the force, from any incidental cause, or opposing obstacle, shall be deflected from the straight line, or thereabouts, when travelling along the course of the long axis of bones, to an *oblique course*, or to a course almost at right angles to

the line of the muscular force (or that nearly in a line with the long axis of bones), that then the conscious party will experience within, according to the intensity of the force, a *feeling or sense of weight*.

It is, then, upon the stability of the direction of force, in these two opposite and distinct courses, that the entire integrity, and demonstrability of the accuracy of the present hypothesis is based.

Before proceeding with any experiments, I shall at this juncture of my paper, between theory and practice, take the opportunity of pressing home the above positions of the direction of forces in the oblique, or nearly midway right angle course, and the straight line, or thereabouts, by a few illustrations and diagrams.

FIG. 1

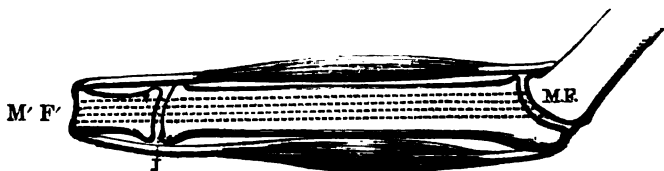


FIG. 2.

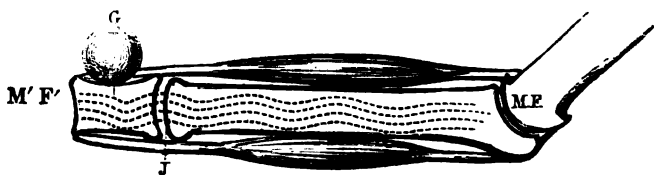
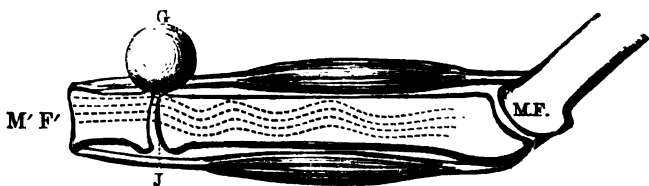


FIG. 3.



Suppose M F and M' F' represent either extremity of a rod whose continuous integrity shall be interrupted by an intersection, or joint, at the point J. Let cords extend from either extremity of the rod (above and below), endowed with contractile, or shortening powers. The cords contracting, or, in other words applying force, the residual force unexpended in the cords (*alias* muscles), will be expended in moving and regulating the position of the rod (or bone), and, in so doing, will run nearly parallel with the long axis of the rod, from the nature of the attachment of the cords; and, as a necessary consequence of the section or division of one part of the rod from the

other (as by joint), by force being applied, as here described, the opposing surfaces will be brought into closer compact and opposition ; and by being brought into opposition by a force acting, or channeling in a course parallel with the long axis, it follows that a force acting in the line of the long axis of a rod, will, whilst passing through, or across its sectional partitions, run in the same course or direction as in the remaining portions which are not intersected, which course being that of the long axis of the rod, the force passing through the intersection or joint, will be in the line of the long axis also, or, in other words, in a straight line or nearly so, which line of force is here called the *line of muscular force*.

But supposing, as in figure 2, the muscular force, at the extremity **M' F'** is interrupted at the point **G** with gravitating force, or as in figure 3, at the point **G J**, with the same force, then the muscular force being checked or interrupted at the said points, according to the amount of the gravitating force which meets the muscular at a more or less acute or obtuse angle, but seldom diverging very greatly from the right angle, and also according to the *degree of counterpoising muscular force*, a new or different line of force will result diagonal to the antagonistic forces. The force resultant upon the combined action of gravitating and muscular forces, whose direction, from the position, or rather line of direction, maintained for muscular force alone, must, in the present case, be more or less oblique or angular, in its deflections from one portion of the rod or bone to the other, and in such direction it must pass through the joint or intersection herein set forth.

If, then, the opposite states of consciousness of force, as those of *power and strength*, or of *weight*, result from the *direction* in which force is applied to the surfaces of joints, it follows, for all experiments conducted with the view of proving such a position that they should have within them, to a greater or less extent, the elements essential to the sustaining and demonstrating such a distinct feature, and, if I may so say, crucial a result. Experiment, therefore, only remains to prove and sustain the hypothesis, that, to synovial membranes sentient nerves are distributed, whose function is to take cognizance of force, as applied to joints, whether it be gravitating or muscular.

The standard from whence the experiments will be measured, will, as a rule, be based upon the supposition that muscular force, unchecked by gravitating, is accompanied by a feeling or consciousness of power or strength, which, when counterpoised by gravity, is exchanged to that of weight, and in the following experiments the circumstances and conditions under which weight can be best felt will occupy the greatest share of attention, whilst the contrasting feelings of strength or power shall not be overlooked.

Again, in attempting to repeat the experiments herein detailed it may be necessary to say a word or two. Force, like smell, taste, touch, etc., is very variable in different individuals, and unless due regard be paid to this part of the subject, no care in the conducting

of the experiments will avoid a perfect failure. The best rule to attend to in this matter is to ascertain from the party operated upon, or the operator who may try them upon himself, whether he has a good idea of the weight of bodies by taking them in the hand, and trying to weigh them; if he has a good idea, then the experiments, *cæteris paribus*, will succeed, but if he is deficient upon this point, the probabilities are, that they will fail.

Moreover, in prosecuting these experiments great care will be required in attending to the details given with each experiment, and much patience in repeating them two or three times, until some facility is acquired in readily seeing through the conditions requisite for their successful prosecution.

In order that an experiment may determine the point which it is designed to elicit, it is necessary that the object to be experimented upon be somewhat understood in those parts in which the end to be elicited is closely connected; and in the present instance an examination of a few leading points connected with the mechanism and direction of forces pertaining to the joints, may be found convenient and suitable; each joint, in its course, receiving sufficient examination to enable the experimenter to judge of the value of any result obtained from such an ordeal as that of careful experiment.

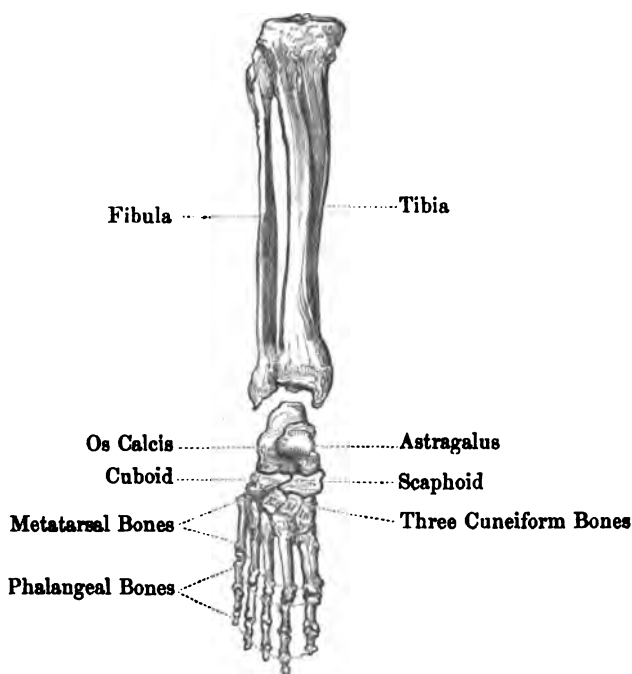
Suppose we commence with the foot.¹ So far as function is concerned, it may be said, that the foot possesses three fixed points or axes, for the weight of the body to rest upon, namely, the heel, or posterior axis; the ball of the great and second toes; and the cushion, or ball of the fourth and fifth toes, especially the fifth or little toe, the former of which might be called the *internal*, and the latter the *external* axes of the foot.

It is from this mechanical arrangement that man is enabled to stand upon one foot for an indefinite period; and to recover himself when he has in some degree lost his balance. Birds, likewise, are enabled to stand for an indefinite period upon one leg, from a similar principle of mechanism pertaining to their feet, as in most aquatic species, etc.; the tarsal terminus constituting the posterior axis, and the internal and external toes the anterior axes, regulated

¹ Before entering upon any remarks relative to the mechanism of the foot, I feel obliged to express my deep regret, that, from the nature of the subject matter of this paper, I am not capable of employing the improved *descriptive* nomenclature which its anatomical relations have naturally suggested to Mr F. O. Ward, when describing the osseous structure of the foot in his work, already referred to, on Human Osteology; but this paper was written previously to my having perused Mr W.'s work, and the nomenclature, as here supplied, being adapted to function, in relation to the sense maintained, and not to descriptive anatomy, upon mature consideration it appeared best to me to persist in what I had first written, than to alter it for courtesy's sake, and at the expense of making the subject less intelligible; while at the same time, I do not in anywise desire to claim any superiority, far less priority, in giving, in many respects, an almost identical description of the functions and motions of the foot.

by the middle toe; but where such arrangement is not made, as in the psittacidæ, or parrot family, standing is difficult, or almost impossible. So likewise the ouran-outan (*alias* orang-outang), is obliged, in progression, to preserve its balance by a stick, from the absence of the heel (the os calcis being, in that animal, very small), and also from the want of a double arrangement of axis to the anterior part of the foot, the weight being chiefly received upon the external border of the foot, or rather, modified hand; and as for standing upon this instrument for any length of time, the execution, with such a mechanism, is impossible; for, whilst two axes, or fixed

FIG. 4.



points (with which signification the word axis is here used) are sufficient to balance and rest the superincumbent weight placed upon them, with the ability of changing position by throwing the body upon one or the other entirely; as when the body rests upon the heel and great toe, by raising the heel the body falls entirely upon the ball of the great toe; yet to preserve one position for a very limited period a second axis is necessary, owing to the variety of points from which muscular force is applied during the alternate contraction and relaxing of muscular structure; and when standing

on one leg is performed with ease, and for long, as in birds, and also in man, a third, or regulating axis is indispensably necessary.

The relation which these axes have to the sense of *force* will appear evident when we come to examine their osseous connections. But it may here be remarked, that the pads or balls of the several axes, consisting for the most part, of dense cellular tissue, in which is enveloped variable quantities of adipose tissue, serve the important function of checking concussions, from the weight of the body falling suddenly upon those parts which their structure is adapted to protect; yet, as these pads have no direct connection with the osseous arrangement of the foot, further examination is unnecessary.

As the astragalus is that bone through which all weight, or force, from the body is transmitted to the foot, and also, as it is the common receiver of all force applied to the foot, as by muscles, to raise the body from the surface upon which it rests, the tibio-astragaloid articulation being the communicating medium, it may be well, at this part of the descriptive outline, to mark out the *order* in which these several axes are impressed during progression, and with it the probable amount of weight received by the articulation referred to from the successive axes.

The posterior axis, situated on the posterior border of the os calcis, receives the weight of the body first, when the foot is projected forwards and placed upon the ground, as in the act of walking; whilst the anterior external axis of the foot reaches the ground nearly simultaneously with it, so that the force of the body and consequent weight is divided between them. These two axes form the first firm resting point or platform for the body to rest upon; whilst, by a combination of movements in the lower extremity, but especially by the combined contraction of the soleus and gastrocnemius muscles, the weight of the body is thrown off the *external* axis to the *anterior internal* axis, or ball of the great toe, and the heel or posterior axis. These two axes—the posterior and anterior internal—preserve their sustained function to the body but for an exceedingly limited period, the whole weight of the body being speedily thrown upon the anterior internal axis only—the immense force applied between the tibio-astragaloid articulation and the ball of the great toe being received upon a powerful arch or bridge, whose summit is immediately over the scaphoid bone. In these successive movements, by careful examination, it will be perceived that the heel or posterior axis has a most important function to perform as a balancing or regulating axis, whilst the direction of weight from the body is transferred from the external to the internal anterior axis; or that axis which is destined, in successive movements, to receive and sustain the whole weight of the body during the period occupied by the opposite limb in wielding it off from behind forward to be planted upon a like posterior axis or heel, to undergo like successive motions, till progression ceases in rest.

From the foregoing remarks, aided by a consideration of their

osseous connection, it will be perceived that if the weight of the body be received upon the posterior axis of the foot, though the os calcis is so firmly and closely articulated with the astragalus, yet that the whole force is not transmitted from the calcis to the astragalus, and thence to all parts with which the astragalus is articulated; since part of the force is directly transmitted from the calcis to the cuboid, and to all bones articulated with it, especially to the fourth and fifth metatarsal bones. Consequently, if sentient nerves distributed to the tibio-astragaloid articulation, give consciousness of weight, the *same weight* applied to this axis will be less perfectly determined than where it is more directly transmitted, especially as it is brought early to composition by the force or weight of the body being speedily received upon the external axis, towards which point it is travelling from the heel, at the time the weight of the body is being received upon this axis—the external.¹

Again, the anterior external axis, including the heads of the fourth and fifth metatarsal bones, does not transmit force applied to it by a direct line of osseous continuity to the astragalus, but through the medium of the os calcis; by which means, much of the force applied to it is expended in the os calcis, etc., before reaching the astragalus. Hence, only a limited amount of force applied to the anterior external axis, reaches the tibio-astragaloid articulation.

Lastly, the anterior internal axis, situated between the heads of the first and second metatarsal bones, but chiefly occupying the head of the first metatarsal bone, transmits its force directly to the astragalus, through the medium of the internal and middle cuneiform bones, and the scaphoid. The articulations of the tarsal bones with each other, admit of such slight motion, that their ability to transmit force in the direct line in which it is received, is here taken as a groundwork for estimating the course and distribution of force, in the same way as a continuous solid structure, making suitable allowance for the amount of cancellous structure existing in the centres of these bones.

But before we leave this mechanical subject, it may be asked, What do I make of the third toe? Suppose we view it as a communicating and moderating medium, directing the force applied to it, according to the conditions in which it may be placed, a little more to the external axis at one time, and at another time a little more to the internal axis, of the foot; since the peculiar manner in which its osseous relations are wedged in between the cuboid and scaphoid bones allows, if the conditions are supplied, this variable way of directing or shifting force—in the same manner, but to a much more limited extent—as the middle claw of the bird is applied to equipoise and adjust the *course of force*.

Having thus far examined a few leading points in the mechanism of the foot, it may not be amiss to make a few remarks relative to the synovial membranes pertaining to this instrument.

¹ Vide Fig. iv.

In examining the synovial membranes pertaining to the foot, as well as those connected with all other parts of the body, their relative importance to the sense under consideration, must not be measured by the superficial area any particular sac may present, but by the apparent amount of sensibility upon injury or in disease. Thus, for instance, from the few scanty opportunities the writer has had of observing indications of pain, during operations, etc., upon the foot, he is disposed to think that the calcaneo-cuboid and scaphoid synovial membranes, and the synovial sac dipping between the scaphoid and cuneiform bones, possess very little sensibility, if any; but that those connected with either extremity of the metatarsal bone of the great toe, possess considerably more. Indeed, many of the smaller synovial membranes placed in the phalangeal joints, and also those in the tarsal and metatarsal joints, saving those connected with the great toe, appear, from the effects of continued pressure and friction, to be worn through, or absorbed, and where such process has not been completed, their dry and almost imperceptible structure, indicates that much of their function is obliterated. Hence, after the age of 40 years, or thereabouts, and in many much earlier, the dissection and tracing of these membranes in the foot and other parts, is impossible; and, as a consequence of this destruction of structure, we shall find, as a rule, that young men and boys, from 15 to 30 years of age, possess the power of testing the value of things by weight, better than those of more advanced years. Lastly, those joints which possess the largest amount of synovial membranes, *in conjunction with the greatest amount of freedom of motion* in the joints which they cover, will be found endowed with the highest amount of sensibility to mechanical injury, and, also, are by far the most painful when inflamed. These latter remarks apply with much force to the synovial membrane lining the tibio-astragaloid articulation; and in experiments pertaining to the foot, the testing point in relation to the sense of *force*, will have especial reference to that joint, or rather, membrane. With these remarks, we may, I think, with propriety, detail a few experiments.

A poker, or pair of tongs, especially the former, is the only instrument required; also, to prevent pain from direct contact of these instruments with the skin, and to check the sensation of excess of heat or cold, it will be necessary to have the feet covered with soft and pliable leather shoes, or easy boots.

Being seated on a chair, let the foot be slightly advanced beyond the perpendicular of the knee. Let the knee and foot be everted, or turned outwards, *as far as is found comfortable* to the party to be experimented upon, then let the poker (or tongs) be placed between the heads of the fourth and fifth metatarsal bones; with the poker thus adjusted, let the foot be gently raised and depressed from the heel, but rather quickly, but not so as to contract the muscles rigidly. In this position, by so moving, *considerable* weight will be

felt. Next, revert the experiment. Retain the poker in its original position, but *invert* the foot, and slightly depress the *external* border; then elevate and depress the foot, so adjusted, as before. In this position, *very slight* weight will be felt as compared with the foregoing.

Again, place the foot in the position *first* described—everted, and apply the poker between the heads of the first and second metatarsal bones, being the site of the anterior internal axis. Next, gently raise and depress the foot as before, in this position *slight* weight will be felt. Revert the order of experiment. *Invert* the foot, depressing its external border as before described, still retaining the instrument over the internal axis, raise and depress the foot, when the feeling of weight will be found very considerable, as compared with the same weight felt with the foot everted.

What relation, it is asked, have the preceding experiments in elucidating the hypothesis here discussed? As follows. The interior articular extremity of the *tibia* is divided by a slight ridge about its middle, the internal half of which is more *depressed* than the external, and, of course, as compared with the internal, the external half is *elevated*. By *everting* the leg and foot, the tibial articulation, in its external half, is depressed; whilst elevating the external border of the foot, and depressing the internal, the external surfaces of the tibio-astragaloid articulation are made to closely approximate each other, and the synovial membrane of the opposing surfaces, is thuswise made to approximate its own surfaces most closely. The consequences of this are, that the force, whatever it may be, passing through the joint, is uninterrupted while passing from one surface to the other; and, therefore, if *direction* of force has any thing to do with the sensation experienced in the joint, in such case, both synovial surfaces will be alike, so far, at least, as the *line of direction* is concerned. At the same time, from the nature of a shut sac, the force must be received upon one surface from behind, and on the opposite in the front; whilst, moreover, from two surfaces receiving force in the same direction, and nearly at the same instant, in the place of one surface, a larger sentient surface is exposed to the influence of its appropriate stimulus. But when the foot is inverted, and its external axis depressed, the external portion of the tibio-astragaloid articulation is widely separated from its opposing surfaces; and for a force to reach the internal portion of the articulation, in place of the external, a circuitous route must be taken, involving much expenditure of its original impetus, by the force having to cross the foot, and, in so doing, meet with friction sufficient to bring much of it to composition ere it reaches the principal sentient articulation, which will amply account for the diminished feeling of weight, when the foot is inverted, and a poker, or any other instrument, is resting over the external pedal axis.

When the weight is placed over the internal axis of the foot, by

everting it, the internal articular half of the tibia, antagonizing with the opposed astragalus, is removed considerably from the adjoining surface; hence, force or vibrations, from this axis to the astragalus, have, from this unfavourable position, their intensity greatly diminished. But by inverting the foot, the opposed surfaces are brought into close approximation, and the force travelling so directly from the internal axis to the astragalus, the feeling of weight will be slightly increased to that felt in the most favourable position on the external axis, but the difference will not be perceived unless some little practice has secured accuracy.

Of course, it need scarcely be added, that in the foregoing experiments, gravitation has been used to counterpoise muscular force; and, as a natural consequence, the direction of force would be in the diagonal of either of the forces taken separately, and would reach the joint in an *oblique* or wave-like direction.

Let me here introduce one more experiment relating to the anterior part of the foot. In this experiment, let a poker be placed over the *middle* of the first metatarsal bone, when the foot is slightly inverted, and raise and depress the foot as before. In this position, the weight experienced will be greater than in any other part over which the poker may be placed on the foot; since here we have the muscular and gravitating forces antagonizing each other in a *direct line* with *three* synovial membranes; the synovial membranes covering either extremity of the first metatarsal bone, each of considerable extent in surface, and the synovial membrane of the ankle joint. The force so applied, will, of course, impress these articular surfaces better than when applied at any angle of inclination, from that of the direct line of transmission.

Apparent exception to the Experiments already detailed.

"If a poker, tongs, or any suitable instrument, be placed upon the internal or external anterior axis of the foot (little or great toes), and the foot, with its superimposed weight, be raised upon the heel, and then be allowed to *suddenly* fall upon the ground (in the same way as conceited minions in music attempt to beat the time for their superiors) by successively raising and depressing the foot, the consciousness of great weight will be experienced in the foot, especially about that axis upon which the instrument rests."

The feeling just described, might be supposed to arise from stronger vibratory impulses being conveyed to the several synovial membranes of the foot, especially to that lining the ankle joint; and the consciousness of increased feeling in the immediate neighbourhood, might be referred to some illusion of the sense under consideration. But the writer has no such conviction about the matter; therefore, he will here simply state, that in another paper, if spared, he shall attempt to show that the sense of force, as here maintained,

has for its principal anatomical seat synovial membrane; but, from the ends its information or function are intended to secure, there are occasional departures from this general seat of distribution, the integument of the palm of the hand, the sole of the foot, and several other parts, being examples of the same.

Concerning the heel or posterior axis, it may be said that an experiment cannot be easily performed upon it, but an attempt may be made, which will elucidate the subject in a trifling degree.

Suppose, to be homely in the description, an individual, in walking across a room, has trod with his heel upon a small piece of bees' wax, and finding it difficult to scrape it off, proceeds to balance himself on the opposite foot, whilst he flexes the leg upon the thigh of the same foot, and gently extends it for the purpose of removing the wax. Now, this last motion, or extension of the foot, is not wanted; therefore, we will suppose that the individual has troubled himself in adjusting his body, as above described, for the purpose of seeing whether or not the back of his shoe has been properly cleaned. This, then, will give the exact position here wanted. If, then, an individual, with the foot and leg thus adjusted, take a poker, and place it opposite the posterior surface of the os calcis, he will experience but slight weight at first; but after it has remained over the os calcis for some little time, the feeling of weight will greatly increase. Next, alter the position, and place the poker superiorly upon the tendo achillis, immediately opposite to the tibio-astragaloid articulation, when the feeling of weight will be felt at once, and *greater* than if placed above or below this situation.

The cause of this difference in the feeling of weight in the two approximate positions is obvious; when the poker rests upon the posterior inferior border of the os calcis, the greater part of its weight or force passes along the os calcis to the cuboid and metatarsal bones of the fourth and fifth toes, but part of this force not finding a ready transit from the cuboid to the metatarsal bones will be deflected, in some measure, according to its angle of incidence, towards the articular surface of the astragalus, and there occasion an increased consciousness of weight; whilst part of the weight will travel more directly (than by a course of deflection) from the calcis to the astragalus; nevertheless, whatever course force may take from the calcis when the foot is adjusted, as has been described, it is evident that before it reaches the principal sentient articulation of the foot, namely, that of the ankle-joint, much of it will have come to composition and resolution ere it has reached that point. But the reverse of all this holds good when the weight is placed exactly opposite the ankle-joint posteriorly, where both forces are brought into collision at once, and before their resultant or oblique force has been expended by friction or composition; hence, according to the present hypothesis, the greatest weight ought to be full here, and also most speedy, which exactly accords with fact.

Here terminate what experiments I have to submit for examination on the foot. The attention will next be directed to the knee and hip-joints.

(*To be continued.*)

ARTICLE V.—*Meissner shown to have been the first who confirmed the fact that the Spermatozoon penetrates into the Interior of the Ovum of the Rabbit, the Animal in which such penetration was first observed.* By MARTIN BARRY, M.D., F.R.S.

THE Philosophical Magazine for May 1854 contains a note announcing that several German physiologists had at length seen the spermatozoa within the zona, not only in the ovum of the same class, but of the very animal in which I had witnessed it—the rabbit. For about a dozen years this fact of mine was by some neglected, by others ridiculed and set at nought. It seems to be viewed in a somewhat different way at the present time; for now we find an eminent physiologist making it known to whom is due the merit of giving to the said fact *further confirmation*. This will be seen from what follows.

On the 3d of April of the present year (1854) a declaration was published at Göttingen by R. Wagner, who says, “In the beginning (probably on the 7th or 8th) of March I had killed a female rabbit for some experiments on the heart. Dr Meissner, who was engaged in my apartment at the Physiological Institute, examined the genitals, as the ovaries presented fresh *corpora lutea*. He was so fortunate as to find in the first portion of the uterus some fecundated ovum exactly like (*i.e.*, in the same stage of development as) Figs. 31 and 32 in Bischoff’s work on the rabbit’s ovum. Even in the first ovum, as well as then in the others, M. Meissner found spermatozoa within the zona in immediate contact with the yolk. He showed me this first ovum, and asked the presence of other persons, viz., Messrs Henle, Baum, Dr Max Müller, Theodore Weber, Dr Schrader, and several students. We all convinced ourselves of this remarkable fact hitherto denied, as is known, by Bischoff, the very man who has been most, and most deeply, occupied with the mammiferous ovum. About ten days afterwards, as appears from my correspondence list of 16th March, I wrote to Professor Leuckart of Giessen, informing him of this remarkable fact observed by Meissner on the relations of the spermatozoa to the ova. I deem it a duty to declare, that it has not been agreeable to me to hear of the publication by Professor Bischoff of what was communicated in a letter to a third person, the said publication, moreover, giving an incorrect statement of the same. Should my written communication to Professor Leuckart be generally mentioned, I must at least expect, indeed require, that mention thereof be made in a different way, and in a manner showing what my letter really said; and that

neither my name nor the names of Henle or Baum be placed before that of Dr Meissner, for I expressly represented myself and the others as only witnesses verifying the important fact. As Professor Bischoff dated his new paper (Giessen) the 25th of March, my letter to Leuckart having left (Göttingen) on the 17th, *there can be no doubt of its having been through the latter that Bischoff was first prompted to institute his observations, the result of which directly contradicts what he had constantly maintained before.*"

Part Second.

REVIEWS.

A Manual of Pathological Anatomy. By C. HANDFIELD JONES, M.B., F.R.S., etc., and EDWARD H. SIEVEKING, M.D., etc. London. Small 8vo, 1854. Pp. 788.

Transactions of the Pathological Society of London. Volume Fifth. London. 8vo, 1854. Pp. 371.

Transactions of the Belfast Clinical and Pathological Society for the Session 1853-54, etc. Belfast. Small 8vo, 1854. Pp. 132.

THE advancement of pathology during the last ten years may be truly said to have been extraordinary. Indeed all that refers to those morbid processes which may be now classified under the head of growth or nutrition has undergone a complete revolution. In the pathology of the neuroses, however, no great advance has been made since the introduction of the doctrines of Reflex Action by Dr Marshall Hall, and medicine, in consequence, is in this department still too dark and unsatisfactory—still too much oppressed by non-logical distinctions and empirical rules. But in all that relates to changes in nutrition—to the circulatory, respiratory, digestive, absorbent, and secreting functions, it must be allowed that mighty beneficial changes have been accomplished. With regard to them the old nosologies have been happily exploded, never more to return. Even the most empirical practitioner has been constrained to follow in the wake of that rational system of treatment, which is dependent more upon accurate diagnosis and detection of the pathological condition of organs, and less upon the palliation of symptoms and indiscriminate faith in the action of drugs. The present state of medicine as a science, we believe to be one of rapid transition, which, before long, will bring about a new phase in its history. Its condition as an art, is that of gradual preparation for the advent of new principles. Every day exhibits the relinquishment of some old empirical notion—we no longer hear of a man cutting short inflammation with the lancet, or with calomel. The lancet is

now scarcely ever employed, and calomel is far more cautiously prescribed. The cultivation of morbid anatomy and pathology, has taught us better to discriminate between organic disease and functional derangements, and the result is a more just appreciation of spontaneous recoveries on the one hand, and of the effects of remedies on the other. Undoubtedly the next great step onwards in the improvement of medicine will be brought about, not by blind tentative efforts at curing diseases, but by laborious scientific researches, undertaken to discover their nature, and so determine the laws by which they are produced and influenced.

The Manual of Pathological Anatomy, by Drs Handfield Jones, and Sieveking, whilst it falls short in some parts of what we believe to be the actual state of our knowledge, must still on the whole be considered a great improvement upon all the British systematic works which have preceded it. It is judiciously arranged, ably compiled, and in some portions may justly claim the merit of contributing new and valuable matter to our store of scientific facts. We should have felt great pleasure in entering at some length into friendly controversy with the authors, on some points in which we differ from them, were we not frequently met with such phrases as "our limits forbid discussion," pp. 109 and 137, or "we cannot enter further into this question," p. 242—although such question is acknowledged to be at the very foundation of our pathological knowledge. Considering that this work is intended chiefly for students, we think it would have been useful had the authors indicated where the authorities on such controverted topics might have been consulted. Dr Sieveking, for instance, after alluding to the arguments of Dr Barrows, regarding the peculiarities in the circulation within the cranium, should have informed his readers how fallacious they had been proved to be by the late Dr John Reid, in an able paper which placed the views of Monro, Kelly, and Abercromby on a firmer basis than ever. We could, indeed, point to many omissions equally important, but entering as we do fully into the extreme difficulty of the authors in their attempt to reduce so extensive a subject into the compass of a manual, we would rather dwell on merits than on defects.

It is with no small satisfaction that we observe a pathological doctrine for which we have long contended, and which has constantly been brought forward in the pages of this journal, at length recognized to be correct, notwithstanding the great authorities who have hitherto opposed it. Thus Dr Handfield Jones observes—

"It really seems far too exclusive and one-sided a view to consider only the blood and the vessels as the agents concerned in hyperæmia, the common initiatory step of inflammation, and to deny to the essential elements of the part any share in the production of a state by which they are so importantly affected. We therefore recognize an increased attraction of the blood towards the part which is stimulated, as one cause of active hyperæmia, and the principal, and we regard the dilatation of the arteries as a secondary, but not unimportant.

But the influence which the tissues exert on the circulation, in virtue of their "nutritive power," we may be sure is not only an attraction which may be increased or diminished, but also an alteration which the attracted blood undergoes, and, having undergone, is either repelled or pushed on by the advancing current. We may illustrate this motive influence by the example of light bodies, when acted on by electricity. Two pith balls, one of which is in a negative, and the other in a positive state, will attract each other strongly; but as soon as they both become negative or positive, they forcibly repel each other. Some similar relation must subsist between the blood and the tissues. The arterial blood is heterogeneous to the tissue, and is attracted to it. Having become venous, it is no longer so, and it ceases to be attracted, perhaps even is repelled. Now, we may conceive the attractive force to persist, or even to be exalted, while the change impressed in nutrition may be greatly lessened. Blood will then accumulate in the part, from not having undergone that vital change which it should, and the part will be hyperæmic. This would be the case in active hyperæmia of a morbid kind—in that which forms the first stage of common inflammation, in which the vital endowments of the part are lowered, and its functional activity lessened. In healthy hyperæmia, on the other hand, the attractive and the changing influences are both increased. The blood does not accumulate, but only ministers adequately to the increased functional activity of the part. We are anxious to avoid, as far as possible, speculating beyond the limits of actual observation, but we would ask, whether some such interpretation as we have offered be not necessary to explain the different event in two cases of suckling females, one of whom has the child put to the breast early, and by the mental and psychical influence brought to bear, has the functional activity of the gland aroused, so that the hyperæmia as it arises is converted into a copious flow of healthy milk; while the other, who has the child kept from her for two days, and whose mammæ are left unaroused and unstimulated, suffers from overwhelming hyperæmia, which issues in inflammation and suppuration."—Pp. 86, 87.

Dr Jones considers hyperæmia to be of three kinds; 1st. Hyperæmia, or excess of blood in a part with diminished motion. This he calls congestion. 2d. Hyperæmia with increased motion. This he says is determination of blood. 3d. Hyperæmia in which the blood is partly increased and partly diminished. This is inflammation. The attempt to define congestion and inflammation according to the rapidity of the current of blood in a part, strikes us as being open to very grave objections. In the first place, the condition of the true capillaries seems to us to be always the same when they are relaxed and truly congested. What the author calls congestion, is that obstructed state of the veins which causes *venous* congestion and dropsy, a condition of the part more dependent on difference in the seat and cause of the morbid accumulation of blood, than of the rapidity in its current. Then the so-called phenomena of determination of blood as evinced by increased pulsation in the arteries leading to a congested or inflamed part, is certainly not a cause, but rather a result of the lesion in the extreme vessels. Lastly, that in inflammation there is at one and the same time in a part both increased and diminished motion, is a condition that has never been seen—and that its existence can in any way be proved by bringing together an observation of Mr Lawrence on a patient, and another observation by some one else, on the transparent parts of animals placed under a microscope (p. 102), is what we cannot understand.

We agree with Dr Jones in considering the neuro-pathological theory of inflammation to be untenable, neither do we attach great weight to the increased spissitude of the blood theory, supported by Mr Wharton Jones, and which is rather favoured by our author.

We are compelled to take notice of the following passage, in consequence of its containing a statement we believe—indeed we can prove to be erroneous:—"After tubercle in any quantity has softened, and a cavity been formed from which the tubercular detritus is afterwards eliminated, a cure may still take place; but it is a much rarer occurrence than in the former cases, and perhaps never attains to the complete closure and cicatrization of the cavity. This, at least, applies to the lungs; in other parts, there is no doubt that a tuberculous ulcer may heal up and cicatrize."—P. 148. Does Dr Handfield Jones seriously mean to assert that a tubercular cavity ever completely closes or cicatrizes? If so, we can assure him that in Edinburgh there is a preparation that would convince him to the contrary, and we would especially refer him to the description and figure of it in the March Number of this Journal, for 1850, p. 233. Dr Sieveking seems to hold on this subject a different opinion, as, according to him, tubercular cavities of the lung may disappear altogether, leaving a dense white fibrous tissue, ramifying irregularly in the surrounding tissue, p. 453.

Dr Handfield Jones, as is well known, has paid great attention to the morbid anatomy of the stomach and liver, and has thereby truly advanced our knowledge of their pathology. We would point especially to his chapters on these subjects as a proof of the correctness of an opinion we have long held, that such are the kind of researches which are now required, and that until every organ in the body has been similarly investigated, we cannot hope that the diseases to which they are liable, can ever be understood.

The different chapters on spinal pathology contributed by Dr Sieveking are well written and judiciously put together. They consist, for the most part, of careful compilations from authoritative works, and exhibit extensive knowledge and reading on the part of the author.

This book is copiously illustrated by woodcuts, some of which are very good, in others there has been a failure. Among the former, we would especially point out the woodcuts illustrative of the morbid changes in the heart, stomach, and liver. Figure 17 is not a good representation of a fibrous tumour, and we would advise the substitution of something better in the next edition. The section on vegetable parasites is ridiculously short, and illustrated by a something about the size of a fourpenny piece, said to represent the structure of *Favus*—partly from Lebert, too! We would again advise a reference to the pages of this Journal for July 1850, pp. 51 and 53, for something better.

Of the transactions of the Pathological Society we can speak with

unqualified approbation. The numerous careful observations, accompanied as they now are by admirable delineations of the minute structure of morbid lesions, will place such volumes as the present and the former ones, amongst the most valuable additions to our annual medical literature. Here again we recognise decided progress, as must ever be the case, when men having, in the first place, rendered themselves qualified for the task, seriously determine to investigate for themselves, with a sincere desire to discover truth. Amidst so much that is excellent we shall not venture to particularise, but shall extract, in future Nos., a few of the more remarkable contributions for our Periscope. We cannot, however, omit saying, that the editorial department of these Transactions must have cost great labour; and, if we are correct in attributing it to the zeal of Dr Richard Quain, we can only remark, that notwithstanding the great ability displayed, such is only what was to be expected from his well-known enthusiasm and successful cultivation of pathological science.

Nor must the Transactions of the Belfast Society be overlooked. Though the letterpress and plates will not vie with the brilliant production of its elder sister institution in London, every page in it exhibits a laudable ambition not to lag behind in the task of self-improvement; and the sketches appended, comparatively rough though they be, demonstrate to the sagacious inquirer, that Belfast also possesses men who cultivate pathology with earnestness, and in a right direction.

Förhandlingar vid Svenska Läkare-Sällskapets Sammankomster.
Stockholm: 1854.

Proceedings of the Swedish Medical Society. Stockholm: 1854.
Pp. 270.

Svenska Läkare-Sällskapets Nya Handlingar. Stockholm: 1854.

New Transactions of the Swedish Medical Society. Stockholm: 1854. Pp. 417.

UNLIKE the proceedings of many of our Medical Societies, the zeal in which appears to undergo many singular fluctuations, or rather revolves in something like cycles, each with its culminating point occurring at intervals, those of the Medical Society of Stockholm seem of late to have been steadily and conspicuously advancing, as well in the greater development as in the higher quality of their efforts. The report for the year 1853, which we have just had the gratification of having placed in our hands, is distinguished beyond those we have previously seen, for the copiousness, and, we think also, for the value of its details. From the manner in which these

are presented, comparatively few of them are of such a nature as to admit of being easily transferred to our own pages: but we have perused them generally with great interest, and can recommend them honestly to those of the more enthusiastic lovers of medical literature, who would not grudge the labour of acquiring an easy language, for the sake of appreciating the fresh outpourings of the Scandinavian intellect, which, in its leading characteristics, is so nearly identical with our own, and yet which presents enough of peculiarities to keep alive our curiosity, while it has abundance of more solid merits to occupy and improve the judgment.

Passing over, though with some reluctance, such interesting matter as is offered by Dr Santesson, in his description of a case of villous tumours of the bladder, corresponding in their character with the tufted cancer (*Zottenkrebs*) of Rokitsansky; and the example of displaced spleen, reported by Dr Böttiger, where the organ, with an elongated and serpentine splenic artery, was found within the true pelvis; we own to have had our attention somewhat closely attracted by the discussions on the *älta*, a popular name for a form of disease in children which the Swedish physicians seem to have some difficulty in reducing to its proper position in nosology, but which appears to be a description of periodic fever. The singularity, however, regarding it, which chiefly engaged our notice, was that, in the Swedish capital, its treatment seemed still to be admitted as the acknowledged province of a numerous sisterhood of old women, termed the *ältgummor*; the cunning women, or *skilly wives* of Stockholm. Though these coifed sages appear to assume skill only in the cure of the *älta*, they seek amends to themselves for this reserve by designating a large variety of ailments under the term, according to a most approved resource of empiricism. Their method of treatment consists in the rubbing in of a kind of ointment, which they name the *ält* salve. Upon referring to Hartman,¹ we find a formula for this salve, according to which it appears to consist of garlic, tar, ox-gall, and oil of bays: a composition which is possibly held to be assuredly efficacious because it is indubitably nasty.

We are far from alluding to these popular errors and superstitions in medicine, because we are conscious that we are free from them in our own country. On the contrary, it is a too evident truth, which we may deride or lament as the humour suits us, that there is no rank of society, and scarcely any grade of intellect, among us, which is not frequently warped by the grossest credulity into the most absurd of beliefs and practices; and that even the instances of this are occasionally so monstrous, that it is only their notoriety which could gain them credence. Philosophers and mathematicians, divines and judges, men accustomed to reason from deduction to deduction, can divest themselves at once of such trammels when they approach any of our leading quackeries; and abandon themselves with astounding

¹ *Husläkaren* (Stockholm, 1835), p. 130.

recklessness to follies which would be at once pronounced to be insane, were it not that their abettors affect a kind of consecutiveness in their ideas, and mimic vilely a few of the outward forms of science. Still, examples of this description, wherever encountered, are interesting as portions of the history of the human intelligence; and if we remark them in other nations, it is rather with sympathy than self-gratulation. If, then, the Swedish peasant is made to believe that he has swallowed a snake, and gets rid of it by drinking a weak solution of arsenic in mare's urine; or some easy victim of seduction thinks to procure abortion, by swallowing five birch leaves, on which are the red spots popularly named cuckoo's blood; or a sufferer from green-sickness seeks in the same remedy the more innocent uses of an emmenagogue; or some member of "the better classes" hangs a little bag to the right ring-finger of a boy, and the left ring-finger of a girl, as a sovereign cure for a remittent, provided always that it hang a certain number of days, and be burned afterwards; we notice these superstitions, which we have gathered at random from the volume before us, not on the score of their singularity, but because they are nearer akin to much that still prevails among ourselves than many may be inclined to imagine; or because, if they appear to differ, it is unhappily in form more than in essence.

The *wise man*, the *klok gubbe* of the Swedes, is, it seems, still not wholly extinct; though now shorn of much of that splendour of reputation which rendered one of the tribe the object of a visit of curiosity from Linnæus himself. Professor Huss, in his interesting treatise on the endemic diseases of his country, tells us, on the authority of Dr Ekman,¹ that such a "*klok gubbe*," or *skilly* old man, was especially famed at Hudiksvall for his cures of rheumatism; but as he accomplished these, in the fashion of Bouilland, by repeated bleedings, performed alternately on arm and foot, and not by nostrum or charm, he is no proper specimen of his craft. The true *wise man* gained his skill more easily, and dispensed its benefits more mildly, than after this material fashion. All that was necessary was to meet with a certain white snake, unfortunately a prodigy of exceeding rarity, which he was to seize hold of, when it would leave its skin in his hands. By merely licking this, he entered at once into enviable possession of the power of knowing the secret of all remedies, and the cure of all diseases.² The wise man of Enekualla, in West Gothland, who flourished in the last century, must have committed an unnecessary cruelty, though it was one not without precedent, when he boiled his snake, and eat it. Swen, at Bragnum, whom Linnæus visited, enjoyed his healing faculty in virtue of the snake's skin; although the famous Swedenborg, who admitted the faculty, allowed him the higher honour of deriving it from a communication with good spirits. His cures were renowned everywhere, and attracted patients from far and near; and yet, though he knew well

¹ Huss, *Om Sverges endemiska sjukdomar*, p. 18.

² Afzelius, *Swenska Folkets Sago-häfder*, Andra delen, p. 196.

beforehand that the virtue would depart from him if he took a wife, he determined to marry, and forfeited thus his high privileges and endowments.

The following case, reported by Dr S. Elmlund, of Boraas, is interesting, both on account of its nature and the advanced age of the patient; but, most of all, from the success of its simple method of cure. A man, 73 years of age, of strong and bulky frame, and regular habits, became ill at the beginning of the year from an old hernia in the right groin, an incarceration of which terminated in gangrene. A fistula now established itself, through which passed by far the principal portion of the fæces. At the close of the first month's illness, he was seen by Dr Elmlund. He had then a sallow complexion, sunk countenance, and was emaciated; his pulse 100, with febrile exacerbations, tongue foul, little sleep and appetite. On examining the opening, it was found to be circular, an inch in diameter, and with tumid, callous edges. Poultices were ordered to be applied, and laxative enemata to be administered daily. The result was that the induration of the edges gradually diminished, and the sore assumed a healthy aspect: the excretions passing in larger quantities by the natural course, while sleep and appetite improved, and the strength increased. Subsequently, merely dressings with resinous ointment were employed; and towards the end of May the cicatrix was fully consolidated, and the man restored to sound health, which still subsists.

In the volume of the New Transactions of the Swedish Medical Society, we find a valuable account of the lunatic asylums of Holland, drawn up by Dr Grähs; including a sketch of the views of the estimable Van der Kolk, regarding the varieties and the treatment of insanity: while, in a second part of the report, he appears to have succeeded admirably in familiarizing his countrymen with the most approved plans of sanitary improvement, as adopted in France and in this country. Professor M. C. Retzius presents interesting reports of the Lying-in Hospital, for the years 1850 and 1851. Dr Hamberg introduces his account of the natural productions and chemical preparations, displayed in the Great Exhibition, with so flattering a tribute to our national energies and greatness, that we may well be gratified that so judicious an observer has estimated us so favourably.

The Use of the Blowpipe, in the Qualitative and Quantitative Examination of Minerals, Ores, Furnace-Products, and other Metallic Combinations. By Professor PLATTNER, Freyberg, and Dr S. MUSPRATT, Liverpool. London, 1854.

NOTHING can more strikingly illustrate the importance of his tools to the working man of science, than the fact, that a single small instrument may become to him the means of widely extending the

field of knowledge. The Microscope is an illustration of this truth in the case of Anatomy, and the Blowpipe in that of Chemistry. The latter instrument has been employed in the economical arts since a period so far distant, that we are content to refer to the Egyptian Tombs, as containing representations of its use by workers in metals. As one of the tools of the scientific chemist, it is only about a century old, and we are indebted to the Swedes for its introduction among the apparatus of the laboratory as an instrument for gratuitously testing compounds; whilst the Germans, following in the steps of the Swedish mineralogists and chemists, have shown its importance as a means of quantitatively assaying the most precious metallic ores. Few in this country are as yet aware that the pecuniary value of metallic compounds, natural and artificial, is now determined at the chief mining establishments on the Continent with the greatest nicety, by an apparatus which, including Blowpipe, Balance, Lamp, and the accompanying needful implements, occupies no greater space than a dissecting-box, a microscope-case, or a handy book.

Professor Plattner, of the famous Freyberg Mining School, is, according to universal testimony, what one of his travelled friends styled him to us, "The greatest blowpiper of the day;" and his work, which embodies the experience of his great predecessors, in this unpoetical and unmusical branch of piping, Cronstadt, Bergman, Gahn, Saussure, Berzelius, and Herkort, besides giving the results of his own continuous researches, is a work of the greatest value to chemists. But for the practical demands of the metallurgist, the blowpipe would never have been thought of as an analytical instrument; but we need not say that it is as serviceable for purely scientific as for practical inquiries, in so far as the former fall within its range; and we have an illustration in its application to the recognition of Urinary Calculi, and in the name, "Fusible Calculi," attached to one class of them, of the many uses to which this portable but powerful little engine may be turned.

We should have been better pleased with the work before us had it more closely followed the German original, which we scarcely think has been altered for the better, where changes have been made; but, such as it is, the English version of Plattner on the Blowpipe, is a work which, we are glad to see, has reached a third edition, and can cordially recommend to all who have not access to the original.

A Discourse on Medical Botany. By EARL STANHOPE. London, 1854. 8vo. pp. 47.

THIS discourse is the substance of unpublished addresses delivered by Earl Stanhope to the Medico-Botanical Society, of which he was president. These addresses were intended to urge upon the society the extension and improvement of the materia-medica through the

means of botanical inquiry of chemical analyses, and of medical investigation and experiments. The society, he states, is "essentially a medical one, which pursues botany so far, and so far only as may be requisite for its object of examining and ascertaining the medicinal properties of plants, and which, for the same purpose, is assisted by chemists." But he adds, although botanical analogy and chemical analysis were aids and guides in these inquiries, still they were of no avail without actual experiments on the living body. One object of the author is to urge the propriety of using native plants more extensively in the *materia-medica*. He states that the Austrian Pharmacopœia contains 71 plants not in the London Pharmacopœia, many of which are indigenous in Britain. Other Continental Pharmacopœias contain 64 more of our native plants. We fear that the author lays too great stress on the efficacy of drugs, more especially when he talks of a dog cured of rabies by eating the root of *alisma plantago*, to which it was drawn by an instinctive knowledge of its medicinal efficacy! And then he tells us "that an ointment made of the leaves of the common bean, gathered when the plant is in flower, has been successful in dispelling those cancerous humours, which, if allowed to continue, might require excision;" that living on spinage for ten weeks has cured chorea; and that powdered leaves are a remedy for hydrophobia. In one part of the pamphlet the author speaks in strong terms of the effects of odours on the body and mind, and quotes some instances which certainly savour much of the marvellous and incredible. While the discourse contains some useful hints as to the extension of the Pharmacopœia, we cannot but regret that these are mixed up with many statements as to the "powers and virtues" of remedies which are more worthy of the old herbalists, than of the enlightened practitioners of the present day.

Part Third.

PERISCOPE.

PHYSIOLOGY.

KROEGER ON THE PANCREATIC JUICE.

Kroeger gives in his dissertation the results of some experiments performed with Prof. Bidder. Through a fistula in the pancreatic duct they collected the pancreatic juice during certain spaces of time at various periods of the day and under various influences. Concerning the quantitative relations, 1 kilogramme of dog secretes, on the average, within twenty-four hours, about 89.8 grammes of pancreatic juice. According to this calculation, an adult man (weighing 64 kilogrammes) would secrete, in twenty-four hours, 5.715 kilogrammes, i.e., nearly the eleventh part of the weight of the whole body. The ingestion of food exercises great influence over the secretion, the latter becoming much increased in quantity almost immediately after meals, reaching its maximum

within about half to three quarters of an hour after the meal, when it is about six or ten times larger than it had been just before the ingestion of food. Water has not the same effect: on the contrary, when taken simultaneously with solid food, it prevents the latter from causing so evident an increase. The concentration of the pancreatic juice appears frequently diminished in the same measure as the quantity is increased, but this phenomenon is not a constant one; and at all events, the absolute quantity of solid substances is greater after meals than before.

As regards the physical and chemical character of the secretion the author does in general, agree with Bidder and Schmidt; some differences, however, must be noticed. The specific gravity is, according to Kroeger, 1·01065, while Bidder and Schmidt had assumed 1·0306, and Frerichs 1·0082.¹ Further differences become apparent by comparing the following figures relating to 1000 parts of fresh juice.

	Kroeger. (Analysis by C. Schmidt.)		Frerichs.	Bidder & Schmidt.	
Water	981·52	...	986·40	...	900·76
Solid matter . .	18·48	...	13·60	...	99·24
<hr/>					
Organic substance .	12·68	...	3·50	...	90·38
Inorganic substance	5·80	...	10·12	...	8·86

Concerning the *physiological action* of the juice, its power of transforming starch into sugar is not doubted. According to the author's experiments, 1 gramme of the fresh juice transforms within half an hour, under the influence of a temperature of 35° C. 4·672 grammes of dry starch into sugar; as 1 gramme of fresh juice contains 0·014 grammes of pancreatic ferment, one gramme of this ferment would transform 333·7 grammes of dry starch. If we assume, with Frerichs, that an adult man requires daily about 490 grammes (=15 ounces nearly) of starch to compensate the daily loss of carbon, the quantity of pancreatic juice necessary for the transformation of this starch into sugar would be less than 105 grammes, while the quantity actually secreted amounts to more than 5000 grammes. Kroeger is therefore of the same opinion with Bidder and Schmidt—viz., that the pancreatic juice cannot have as its *principal* function the transformation of starch into sugar. As one of the functions, he considers the promotion of the constant interchange of fluids within the body, in the same manner as Bidder and Schmidt have made it so probable concerning the saliva, and with Grunewald and Schroeder concerning the gastric juice. It further appears to him that an intimate connection exists between the secretion of the stomach and that of the pancreas—namely, that the hydrochloric acid secreted by the former is, after having performed its part, neutralized by the soda of the latter, thus again forming the chloride of sodium previously disunited by the process of secretion. In favour of this theory Kroeger observes that the hydrochloric acid secreted by 1 kilogramme of dog through the gastric juice in twenty-four hours amounts to 0·305 grammes, while that of soda contained in the pancreatic juice of twenty-four hours is calculated at 0·237 grammes, i.e., very nearly the equivalent (0·259) corresponding to 0·305 grammes of hydrochloric acid.—*Dissert. De Succo Pancreatico Dorpat, 1854; and Brit. and For. Med. Chir. Rev.*

ZSCHOKKE ON SUDDEN DEATH AND THE MEANS FOR ASCERTAINING ITS CAUSE.

This author believes that sudden death always depends upon disease or interrupted function either of the brain or the heart; he regards death as commencing at the brain in cases of apoplexy, asphyxia, and congelation; and at the heart in cases of paralysis of that organ, and in hemorrhages. In estimating the cause of death, the author places most reliance on the appearance and amount of the blood contained in the vessels. In examining the blood,

¹ Wagner's Handwoerterbuch der Physiologie, iii. p. 844.

the extent of putrefaction must be taken into account, as it exercises a great influence on the condition of this fluid. When apoplexy has been the cause of death, the vessels of the head and lungs, as well as the right side of the heart, are distended with venous blood; and there is total or partial *ramollissement* of the brain, the same essential characters are present in death by asphyxia, and by excessive cold.

The following is a resumé of the author's observations:—

Signs of death commencing at the Brain.

(a.) Constant signs.

1st, Veins of meninges full of blood.

2d, Bloody spots in the cerebral substance.

3d, Fulness of the large veins, and of the right side of the heart. The blood is coagulated when death has been instantaneous; the contrary when death has occurred more slowly.

4th, Lungs engorged with blood; which is *dark*, when the oxygen of the air has been excluded from the organs; and *bright* when air has been breathed up to the moment of death.

(b.) Inconstant signs.

1st, Tongue protruding between the teeth. (This indicates death with symptoms of paralysis, and the external tissues of the head are engorged with blood).

2d, Frothy mucus in air passages. (A sign of convulsions).

3d, Absence of two last signs. (This indicates paralysis, with diminution of the quantity of blood, or a determination of the vital fluid towards the central organs or paralysis of the heart occurring simultaneously with death commencing at the brain).

4th, Swelling and redness of the face and eyes; lividity of the lips. (Signs of congestion).

Signs of Death commencing at the Heart.

1st, Blood uniformly distributed through all the veins of the body, without partial congestion of organs.

2d, Uniform emptiness, or fulness of both sides of the heart.

Signs "of Nervous Apoplexy"—(if there be such a disease).

1st, Uniform distribution of the blood through both the veins and the arteries.

2d, Partial fulness of the cavities of the heart.—*Zeitschrift für die Staatsarzneikunde*. 1853.

MOSLER ON THE QUANTITATIVE RELATIONS OF THE URINARY PHOSPHATES.

Mosler made a series of observations partly on himself, partly on several other healthy persons. The average quantity of *phosphoric acid* secreted within twenty-four hours by a healthy man is calculated at 3.209 grammes; the acid contained in the alkaline phosphates to that in the earthy phosphates bears the proportion of 3 : 1. The quantity of colouring matter within twenty-four hours is about eight grammes. As regards the various periods of the day the secretion of phosphoric acid was greatest in the evening, when the author was, in general, mentally engaged; after this follow in a descending series, noon, night, and morning. By intense mental occupation the quantity of phosphates excreted became regularly increased one-half, the increase relating more to the earthy than the alkaline phosphates; the quantity of colouring matter became likewise greater. By an abundance of proteinaceous food the excretion of phosphates became in a similar proportion larger. The effect of both influences combined on the excretion of phosphoric acid, urea, and chlorine, is striking in the following table under A, while the figures found in normal circumstances are placed sub B.

	Urea.	Chlorine.	Phosphoric acid.
A.	43.050 grammes.	20.720 grammes.	5.104 grammes.
B.	29.120 "	7.010 "	4.479 "

By fasting, the excretion of phosphoric acid was more diminished than that of urea, the quantity of chlorine in the urine remaining unaltered. The examination of a non-pregnant female, extending over five days, gave much lower figures for the phosphates, than of men under similar circumstances. Concerning the urine of pregnant females, the author found, as the mean figures of several examinations for women of 128 pounds' weight, after the sixth month of pregnancy: quantity of twenty four-hours — 1·488 C.C. (62 C.C. per hour); specific gravity — 1·011; reaction more frequently neutral or alkaline than acid; colouring matter 14·592 grammes (0·608 per hour); urea — 26·193 grammes (1·090 per hour); chlorine — 7·930 grammes (0·330 per hour); phosphoric acid — 2·422 grammes (0·100 per hour); sulphuric acid — 1·250 grammes (0·052 per hour).—*Dissert. Giessen*, 1853; and *Brit. and For. Med. Chir. Rev.*

HAFFTER ON THE ACTION OF THE GREATER SPLANCHNIC NERVE.

Haffter, in connection with Professor Ludwig, divided, in five cats, the nervus splanchnicus externally to the peritoneum. Concerning the *sensitive* function, it appears to contain a large amount of sensitive fibres, as the signs of pain produced by the section of the nerve are as great as those by the section of an equally thick branch of the trigeminus. The perception of *hunger* was not destroyed after the operation; as the section of the *vagus* does likewise not annihilate it (Bidder and Schmidt), Haffter concludes that it depends on the combined action of the nervus vagus and nervus splanchnicus. As regards the *motor* function, the author is led to conclude from his experiments that the nervus splanchnicus neither *excites* the motion of the intestines, nor possesses the power of *arresting* it as the *vagus* that of the heart. The *secreting* function of the intestines became only slightly altered; the secretion of the stomach, however, and of the upper part of the small intestines, seemed to be rather increased. The nutrition, as well as the secretion of the kidneys unimpaired. The colour of the liver unusually dark.—*Dissert. Zurich*, 1853; and *Brit. and For. Med. Chir. Rev.*

M. CHATIN ON IODINE IN ATMOSPHERIC AIR.

This chemist read a paper at the meeting of the *Académie des Sciences*, on the 4th December 1854, on the experiments by which he had satisfied himself of the existence of iodine in the atmosphere. He detected it by agitating distilled water with air; and he demonstrated its presence both in rain water and in hoar-frost. He particularly directed his attention to the dew, gathered from vessels which had been exposed to nocturnal irradiation. He found such dew to contain six times as much iodine as was absorbed by the same weight of water. M. Chatin's researches beautifully supplement the investigations of Regnault on the variations in the proportion of azote, and those of Boussingault on the presence of ammonia in the atmosphere.—*L'Union Médicale*, 16th Dec. 1854.

PATHOLOGY AND PRACTICE OF PHYSIC.

DR LINDSAY ON THE HISTOLOGY OF THE BLOOD IN THE INSANE.

THE following remarks are founded on the results of a microscopical examination of the blood in 236 insane patients, and in thirty-six officers and attendants in the Crichton Royal Institution and Southern Counties Asylum at Dumfries. The blood examined was, in almost all cases, that drawn from the point of some of the fingers by the prick of a needle. As a general rule, the insane are extremely bad subjects for such experiments. This applies, of course, in different degrees, to patients labouring under different forms of insanity. They are extremely sensitive, restless, and suspicious of operative interference, even of so slight a nature. Many obstinately refused to allow their fingers to be pricked. Some did so from a firm conviction that a deep-

laid conspiracy against their lives or welfare lurked under the cloak of apparently simple experiment; others simply objected to become tools of experiment or amusement; some declined on the plea that in their greatly debilitated condition they could ill afford to spare even a single drop of blood; others lacked courage to submit to the operation; some demanded full explanations of the motives which led to my making the singular request of allowing their finger to be pricked by a needle; in others this formed the key-note of their delusions, delirium, or vituperation, for days or weeks after the experiment was attempted in them. On the other hand, many, who could not appreciate the objects of experiment, submitted cheerfully, merely from a wish to please their medical attendant; others—chiefly cases of confirmed dementia or of deep lethargy—were perfectly passive, freely permitting any kind or amount of experimentation; some presented their fingers, under the impression that, from the single drop of blood, the state of the constitution, the chances of cure, and the period of their removal, could infallibly be predicted; others from curiosity to see the appearance which their own blood, or that of their companions, presented under a microscope; many, especially of the educated classes, comprehending at once the objects of the experiment, cheerfully submitted, and evinced the liveliest interest in the microscopical appearances, which, in all cases where the patient was in a condition to appreciate them, were demonstrated and explained; some carried this laudable curiosity to a great extent, begging most earnestly not only to see their own blood at different periods of the day, but that of fellow-patients and attendants, evidently strongly impressed with the belief that between their own blood and that of companions who exhibited most different traits of character or conduct, or between that of insane patients and sane attendants, there should exist a perceptible difference. On various occasions, I was obliged to demonstrate the condition of my own blood under the microscope, to satisfy the curiosity thus awakened. There was a marked difference between the two asylums in the readiness with which both patients and attendants submitted to experiment. In the Crichton Institution, a much larger proportion submitted, and with greater cheerfulness and readiness than in the Southern Counties Asylum, where a great amount of persuasion and explanation was frequently necessary. It may, at first sight, appear surprising that the experiment should have been more successful among the rich than the poor insane,—among persons of refined habits, and many of them of delicate constitutions, than among rough, hardy artizans and field labourers. This I attribute entirely to the difference in the *education* of the respective classes; to which, also, I attribute the fact that the patients in the Crichton Institution submitted more readily and cheerfully than the attendants. It is noteworthy, moreover, that, among the higher class patients, a much larger proportion of ladies than gentlemen offered themselves as the subjects of experiment. The cause of this difference appeared to be that curiosity strongly predominated in the former. They evinced great anxiety to know the difference in the condition of the blood between the sane and insane, the diseased and healthy. The classes of cases most readily experimented on were amentia, confirmed dementia, melancholia, and general paralysis; those least readily mania and monomania. In consequence of the difficulty to which I have already adverted—of prosecuting such researches among the insane—I was unable, in the majority of cases, to examine the blood of the same individual more frequently than once; and as I was obliged to do so when favourable circumstances in each individual case presented themselves, my examinations were made at irregular periods of the day. I was thus prevented from making other than a qualitative and rough examination,—from ascertaining the variations in the condition of the blood according to the period of the day (in connection with the digestion of food, etc.), sex, age, and type of disease, mental and bodily, and from accumulating similar data on which to found general deductions, which I should, under more favourable circumstances, have endeavoured to do.

The following is a *resumé* of the chief general conclusions or results drawn by Dr Lindsay from his observations :—

1. That the blood of the insane varies considerably in—*a.* colour, granularity, and dulness; *b.* density or consistence; *c.* coagulability; *d.* relative proportion of serum, fibrin, and globules; *e.* the tendency of the red discs to agglomerate; *f.* rapidity, readiness, and amount of the flow.

2. That the *red discs* vary in *a.* size, *b.* form, *c.* colour, *d.* number, *e.* tendency to agglomerate.

3. That the *white globules* vary in *a.* size, *b.* form, *c.* granularity, *d.* number, *e.* reaction of acetic acid.

4. That, in the blood of the insane, a *leucocythemic condition* frequently exists.

5. That, in many cases, this condition may be more apparent than real, and due to a *deficiency in the amount of red discs*.

6. That there is no fixed relation between the kind or intensity of the above conditions, and the various forms or phases of mental alienation.

7. That there is, however, a certain relation between these conditions and the physical complications of mental alienation.

8. That these conditions are not peculiar to the insane, but occur in the sane, under similar circumstances of physical disease.

9. That the blood is more altered in the insane than the sane, chiefly in proportion as anæmia, struma, and other physical states, are more common in them.

10. That, contrasting the condition of the blood in the rich insane, with that in the poor insane, it is deteriorated more frequently and to a much greater extent than the former.

11. That this is due, in great measure, to the essential difference in the education and habits in the respective classes; to the predominance of mental over physical culture in the higher classes; and to the predominance of physical over mental exercise in the labouring classes.

12. That, contrasting the condition of the blood in various forms of mental alienation, no alterations can be considered peculiar to, or frequent in, any one of these forms.

13. That, contrasting the blood of the insane with that of the sane, any structural alteration in either class is usually due to physical disease.

14. That the physical conditions or diseases, both in sane and insane, in which the above structural alterations most frequently occur, are debilitated states of the system and general vitiation of the blood, resulting from long-continued and exhausting diseases, *e.g.*, anæmia resulting from phthisis, menorrhagia, or intestinal diseases.—*Psychological Journal*.

DR PEACOCK ON PLASTIC BRONCHITIS.

William Chambers, aged 11, was admitted an out-patient of St Thomas' Hospital, on March 17, 1854. His mother stated that he had always been delicate, and that, when about six years of age, he had an attack of influenza, and had since that time been subject to cough and expectoration. After the attack of influenza, his mother observed that he occasionally spat up pieces of skin, which spread out when put into water. He continued to expectorate this kind of matter for five or six months. In May 1853, he had another similar illness, in which he expectorated solid matter, and continued to do so till the June following.

He has now had the peculiar expectoration since Christmas. He took cold at the end of the year, and had profuse epistaxis. The masses are generally brought up after a hard ringing cough, which comes on in paroxysms and threatens suffocation; but sometimes they are expelled with very little effort. Usually only one portion is spat up at a time, but the expectoration is sometimes repeated every hour or two hours. The paroxysms of coughing are caused by any exertion or excitement, and are relieved by the expulsion of the membranes.

His mother states that the bodies expectorated have always the form of a trunk and branches, and they are generally an inch and a half long. They have a white colour, and he has never brought up any blood with them. His voice has never been affected, but he is habitually short breathed. He was directed to take an expectorant and anodyne mixture, containing small doses of the potassio-tartrate of antimony, ipecacuanha wine, and compound tincture of camphor, with an alternative of hydrarg. c. cretâ, soda, and rhubarb.

April 3.—He has continued to expectorate the membranous bodies every

second or third day; sometimes they are brought up with a cough; at others, they rise in the throat and are expelled with a feeling of sickness, either by the mouth or through the nostrils. He has brought up a considerable mass about two hours ago, portions of which are here figured. They have the usual form; the largest piece (fig. 1) is about two inches in length, the trunk is as thick as a writing-quill, and the subdivisions decrease in size till they become very minute. His mother thinks the masses grow larger; of various portions which I have seen, all have been unmixed with other secretions, and free from any appearance of blood.

Upon the whole, his general health continues much as before, but his mother thinks that he is losing flesh. The left side of the chest is altogether somewhat less resonant than the right, and the difference is most marked at the left supra-scapular region. A slight irregular subcrepitant rhonchus is heard in every part, but is most distinct at the upper portions, and especially at the left side posteriorly. When last examined, a distinct valvular clicking sound was heard in the left supra-scapular region, but this does not now exist. He was directed to take the oleum jecoris with quinine and iron, and an anodyne and expectorant. From this time he gradually improved, and was dismissed cured July 17th.

The case which I have related will be observed to belong to the chronic form of the disease, the patient having had several attacks before that in which he fell under my notice. Like many of the similar cases, he was a very delicate person, and inherited a predisposition to pulmonary disease. The inflammatory symptoms had probably never been very severe, and had already nearly subsided. The casts appear to have been at first moulded in the smaller bronchial tubes of the upper lobe of the left lung, but, as the disease advanced, the fibrin was either effused from the original seat of disease, in larger quantity, or the disease itself extended, so that the masses expectorated had the form of the main bronchus of that lobe. When first ex-

FIG. 1.



FIG. 2.



FIG. 3.



pelled, the solid matter formed oblong or rounded bodies, about the size of a filbert, and these, when macerated for a short time in water, gradually unfolded themselves, till their peculiar branched form became apparent; but for the complete separation and exposure of the smaller divisions, much careful manipulation was required. The largest pieces expectorated had an extreme length of about three inches, and at the trunk were three to four lines in diameter. In some I was able to trace ten distinct subdivisions, and the terminal portions were most minute, as in those represented in figs. 2 and 3. Two or three masses were generally expectorated on the same day, or within two or three days, and he then usually did not bring up any more for a period of a week or ten days; but, occasionally, they were expelled more frequently, and in a day he once expectorated not less than seventeen pieces. When first observed, the membranous material was only expelled after a severe fit of coughing, but latterly they were brought up with little effort. The physical signs which were observed in the case were those of slight capillary bronchitis, with obstruction in the tubes of the upper part of the left lung, and consolidation of the corresponding pulmonary substance. There was slight sibilant rhonchus, with more or less crepitation, heard in different parts of the chest; some dulness on percussion at the upper part of the left side, especially posteriorly; and slight bronchial respiration, with mucous sounds, occasionally having the clicking or valvular character, more frequently the usual subcrepitation, in the same regions.

Dr Peacock, from extensive literary research, observes, that 1st, The *plastic bronchitis*, *bronchite pseudo-membraneuse*, or *bronchitis crouposa*, as the disease has been termed by different modern writers, is of more frequent occurrence in males than in females. 2d, While the affection is not limited to any period of life, but occurs both in the young, in persons of middle age, and in the old, it is most frequent between the ages of 20 and 50. 3d, It may attack persons who have enjoyed robust health, but generally occurs in those who have been previously suffering from some chronic pulmonary disease, or who have been exhausted by other debilitating causes. 4th, The affection is characterised by the ordinary phenomena of bronchitis, aggravated, however, by the difficulty in expectorating the solid material; and it may either be acute, rapidly terminating in recovery or in death; or chronic, the peculiar sputa continuing to be expectorated, at longer or shorter intervals, for weeks, months, or even years; or it may repeatedly occur on any exacerbation of the bronchitic symptoms. 5th, Hæmoptysis, at least to any considerable extent, is by no means a frequent or even a common symptom; but, on the contrary, the membranous material is most generally expectorated, either almost alone, or mixed with the ordinary bronchitic sputa, usually with the white, glairy, adhesive mucus which first occurs during a bronchitic attack. 6th, The membranous material is most generally of a dull white colour; more rarely of a brownish hue, or slightly discoloured by blood. Chemically and microscopically, it presents the usual characters of fibrin. In form, it may either be moulded into the shape of a bronchial tube and its ramifications, or it may constitute a thin shred of membrane. When assuming the branched or dendritic form, it may be hollow or solid, and is found to be comprised of a number of delicate laminae, arranged concentrically. The trunk of the branched cast generally has a diameter of from one line to two or three lines, or varies from the size of a small crow-quill to that of a large goose-quill. The branches subdivide till they become very minute. It would thus appear, that the casts are generally moulded in bronchial tubes of the third or fourth size; but cases are recorded in which the masses expectorated had a much larger diameter, and in which, after death, the main bronchi have been found obstructed; and others in which the false membrane extended from the smallest tubes to the trachea. 7th, The ultimate result of cases of fibrinous expectoration depends upon the circumstances under which the formation of the solid matter took place. When, as is not unfrequently the case, the membranous effusion occurs as a complication of phthisis, or of some other fatal disease, it necessarily follows the course, and may but little affect the result of that disease. When, on the contrary, it is

an idiopathic and acute affection, though the symptoms by which it is attended are ordinarily urgent and alarming, it usually soon subsides, under appropriate treatment, and the patient quickly regains his usual health. In other cases, however, the acute symptoms may subside, but the false membrane may continue to be formed, at intervals, for a long period.—*Medical Times and Gazette*.

DR PEDDIE ON THE SUPPOSED EFFECTS OF THE SUSPENSION OF ACCUSTOMED
STIMULANTS IN THE PRODUCTION OF DELIRIUM TREMENS.

In order to obtain some additional evidence on this disputed point, I submitted some queries to Drs Simson and Gibson, the medical officers of the large prison establishments of this city and of Glasgow, and to Mr Page and Dr Scott, surgeons to the county gaols of Carlisle and Dumfries; and the following information has been kindly furnished by them, as to the effect of the sudden withdrawal of all stimulants from civil and criminal prisoners known or presumed to be of intemperate habits, and the immediate substitution of prison fare, which is well known not to be of the most generous description.

As regards the prison of Carlisle, it appears that, although the annual number of commitments during the last fifteen years has been about 600; and that, although three-fourths of these are considered to have been, in one way or another, the consequence of drunkenness, Mr Page states emphatically he has never yet seen any ill "result from the sudden abstraction of stimulants from habitual drunkards, who had been drinking to excess up to the time of being placed on prison fare." Mr Page had also, during nine years' experience in connection with the Carlisle County Pauper Lunatic Asylum, observed the same impunity with which all stimulants could be at once withdrawn. (*Letters, 9th and 21st June 1854.*)

Of the gaol of Dumfries, it is stated by Dr Scott (*Letters, 12th and 21st June 1854*) that, during the last fifteen years, the number of civil and criminal prisoners have amounted to 5539; that of this number he supposes about two-thirds were committed for crimes resulting from intemperate habits; that he believes a very large number to have been habitual drunkards; and that, although all of these, of course, were deprived of their usual libations, and at once put on prison allowance, only five cases of delirium tremens are found on the register of disease, and that all of these patients but one were admitted to the prison with the disease on them; and that in regard to that one, although entered as under delirium tremens on the day after admission, there is every probability for believing that *she* had had the disease on her when admitted, although not reported to be ill. Dr Scott also notices, as an important fact, that during the time the railways were being constructed in the county of Dumfries, a very large number of navvies were committed to prison, who had led a very dissipated life for many months, and although deprived of liquor from the moment of apprehension, not a single case of delirium tremens occurred.

Then, as regards the prison of Glasgow, in which the annual commitments amount to upwards of 4000, the experience of the year 1850 is adduced by Dr Gibson (*Letter of 16th June 1854*), as affording an approximation to the facts wished to be elicited. A calculation made in that year showed that, while 4122 were imprisoned, the number of assaults, with few exceptions, committed under the influence of liquor, and "the drunk and disorderly," amounted to 1519; and of this number only three cases of delirium tremens occurred—a very small proportion indeed, especially when it is considered that the debtors, who are almost all habitual drunkards, and drinking up to the moment of incarceration, are not included in this list. Many hundreds more, therefore, may be considered to have belonged to the drunken population of the gaol. The average of the last ten years, however, is greater (5·7), there having been fifty-seven cases altogether during that period, but, after all, this is a very small proportion to the number of dissipated and drunken characters gathered together there, and at once broken off from intemperate habits. Dr Gibson, however, states that he does not altogether enter into my views as to the proximate cause of delirium tremens, al-

though he admits that "it does not so frequently occur as the advocates of the theory, which attributes it to the total withdrawal of accustomed stimuli, such as Blake and others are inclined to suppose;" and he mentions, in proof of his objection, that he had never seen it occur in less than twenty-four or beyond seventy-two hours after apprehension, which necessarily put a stop to dram-drinking. As I have already explained, however, and as the cases given at the conclusion of this paper will show, there is always, whether the individual is drinking much or little, more or less of a premonitory stage present in this affection, distinguished by digestive derangement, nervous irritability, restlessness, and sleeplessness, before much tremor is displayed, or any illusions manifested; and it is easy to suppose that these might not be brought immediately under the notice of the medical officer of a large criminal establishment, such as the Glasgow prison. But even granting that no incipient symptoms of the disease were observed, and that this proportion of the habitual drunkards were not quite on the verge of being affected with it, it is quite in accordance with the views already advanced to suppose that, when there was a certain amount of alcoholization existing, the disease might be hurried on more speedily than otherwise would have been the case in individuals of a nervous and excitable temperament, by the agitation or shock of apprehension, and the deprivation of liberty. But, further, I should suppose it a very just, nay moderate calculation, to assume that out of a population of 2000 confirmed drunkards belonging to any class of society, although enjoying unrestrained liberty and uninterrupted opportunities for indulgence to excess, at least from three to six instances of delirium tremens would annually occur.

But, in fine, on this point, the evidence communicated by Dr Simson, the medical officer to the prison board of this city (*Lett. 4th July 1854*), is sufficiently satisfactory; for while the number of civil and criminal-prisoners, committed during the last year, was 5864 (which may be assumed as a sample of the previous fourteen years, over which Dr Simson's experience extends), only four cases of delirium tremens occurred within the last eighteen months. The average number of cases during former years, Dr S. states as from 2 to 3 per annum. Dr S. considers that, at least one-half of the whole prisoners may be assumed as dissipated characters, and that at the very lowest computation, 500 must have been regular systematic drunkards, from whom all drink was suddenly abstracted; and he goes on to state as his decided opinion, that "the sudden taking away of spirits, etc., does not produce delirium tremens. In every case, the prisoner had symptoms of the disease on him when admitted—that is, they were all restless, irritable, etc.; and I have no doubt, but that in many instances the crimes committed were the effects of this disease. I do not remember a single case of delirium tremens occurring when the prisoner was quite well when received into prison. There is not the least doubt that a peculiarity of constitution predisposes to delirium tremens," etc.

Here, then, it has been shown, that hundreds of individuals among the public at large, and of the criminals committed to our gaols, leave off or are suddenly deprived of the stimulants to which they had been previously addicted, without being seized with delirium tremens, or anything approaching to it. On the other hand, also, it is unquestionable that numerous instances of the disease do occur in which there has been no suspension either voluntarily or by compulsion of the amount of liquor consumed, nay, even an increased excess in drinking up to the very moment of seizure. The assumption, consequently, that this disease is produced invariably, or chiefly, or even occasionally, by the diminution or abstraction of an accustomed stimulus, is not supported by facts. Any cases, therefore, noticed as occurring under these circumstances, are simply of an exceptional character, but which, in my apprehension, fall quite short of proof from the considerations already so fully explained.—*Peddie on Delirium Tremens.*

DR STOKES ON AUSCULTATION OF THE HEART.

If you take works upon disease of the heart, you find that it is assumed by

almost every writer, that the first sound of the heart and the second sound of the heart are to be easily distinguished from each other. There are some persons who, if you were to say to them, in any given case, "I think that I have had considerable difficulty in saying which was the first and which the second sound of the heart," would set you down as very deficient indeed, as one that had not been properly taught, and did not know his business. But the fact is, gentlemen, that there are many cases in which at first it is very difficult indeed to say which is the first and which the second sound of the heart. There are cases in which the most experienced man will require repeated observation before he can make up his mind on the point.

It has happened to me over and over again, that after I thought I had made up my mind by examining at one part of the heart, when I changed the stethoscope an inch or two I was again thrown into doubt.

I mention this to show you how diffident we should be in our opinions upon these subjects, how slow we should be to condemn men because they do not come up to the mark laid down in books. The truth, in fact is, that they go beyond it—that they are wiser than the authors of such books.

Can we distinguish by acoustic signs alone, the inorganic from the organic murmur? The answer to that question is simply,—that in the present state of our knowledge, there are many cases in which we cannot do so; that there is no special acoustic character by which you can distinguish one of these phenomena from the other. This looks like a depreciating statement, as far as diagnosis is concerned; but the cause of diagnosis would be much more injured by attributing to it powers which it does not possess, than by confessing its deficiencies. The diagnosis, in the case in question, is to be drawn from other circumstances.

The great mistake, gentlemen, that was made,—I am happy to say that it is now going out very fast,—in connection with auscultation generally, was this, it was supposed that every disease had its special acoustic sign, and consequently the attention of students and physicians was directed to the study of those signs in a purely mechanical point of view, merely to the observation of their acoustic characters.

There can be no doubt that it is of the greatest possible importance to study carefully every thing connected with a diseased organ, both its physical and its vital phenomena; but what you have to learn specially is this, not so much how to detect the sign or how to recognize it, as to know how to reason upon a particular sign when you have discovered it.

Bear this in mind always, that there is no pathognomic physical sign of any disease whatsoever. This cannot be too strongly stated; and I believe that we might go further, and say, that there is no combination of mere physical signs which, excluding the history and vital symptoms, can be justly considered to be pathognomic; at all events, if there be such a combination, it is one of extreme rarity indeed. We hear of certain murmurs being pathognomic signs of this and that disease of the heart,—of friction sounds being pathognomic of pleurisy—of crepitating râles being pathognomic of pneumonia—of amphoric sounds being pathognomic of effusion into the pleura. All this is wrong; it is based upon error; and you must expunge it altogether from your minds, if you wish to be accomplished physicians, investigators of truth, and faithful observers of disease as it is found at the bedside.—*Medical Times*.

[There is one observation by Dr Stokes with which we would beg leave to differ, viz., that, when he tells students that they have not so much to learn how to detect and recognise signs, as how to reason upon them. While granting the full importance of reasoning correctly, the great difficulty we have experienced in teaching clinically, is undoubtedly causing the students accurately to make out the signs and symptoms. We need scarcely say that unless the facts are ascertained with exactitude in the first instance, all subsequent reasoning must be erroneous.]

CASE OF ARRESTED PULMONARY TUBERCULOSIS, BY DR STOKES.

Some years ago I saw a gentleman, who came to town labouring under all the symptoms of well-marked phthisis. The disease had been of some months' standing, and the patient was a perfect picture of consumption. He had a rapid pulse, hectic, sweating, purulent expectoration, and all the usual physical signs of tubercular deposit, and of a cavity under the right clavicle. I may also state, that the history of the disease was in accordance, in all particulars, with this opinion. I saw this patient in consultation with a gentleman of the highest station in the profession, and we both agreed that there was nothing to be done. This opinion was communicated to the patient's friends, and he was advised to return to the country. In about eighteen months afterwards a tall and healthy-looking man, weighing at least twelve stone, entered my study, with a very comical expression of countenance:—"You don't know me, doctor," he said; I apologized, pleading an inaptitude that belongs to me for recollecting faces. "I am," he said, "the person whom you and Dr — sent home to die last year. I am quite well, and I thought I would come and show myself to you." I examined him with great interest, and found every sign of disease had disappeared, except that there was a slight flattening under the clavicle. "Tell me," said I, "what you have been doing?" "Oh!" he replied, "I found out from the mistress what your opinion was, and I thought as I was to die I might as well enjoy myself while I lasted, and so I just went back to my old ways." "What was your system of living?" said I. "Nothing particular," he said, "I just took whatever was going." "Did you take wine?" "Not a drop," he replied; "but I had my glass of punch, as usual." "Did you ever take more than one tumbler?" "Indeed I often did." "How many? Three or four?" "Aye, and more than that, I seldom went to bed under seven!" "What was your exercise?" "Shooting," he said, "every day that I could go out." "And what kind of shooting?" "Oh, I would not give a farthing for any shooting but the one!" "What is that?" "Duck shooting." "But you must have often wetted your feet?" "I was not very particular about the feet," says he, "for I had to stand up to my hips in the Shannon for four and five hours of a winter's day, following the birds."

So, gentlemen, this patient spent his day standing in the river, and went to bed after drinking seven tumblers of punch every night; and if ever a man recovered from phthisis he had done so when I saw him on that occasion. Suppose, now, that he had been confined to an equable temperature, and a regulated diet, and had been treated in all respects *secundum artem*, what would have been the result? Any of you can answer the question. In point of fact, this very treatment had been adopted during the first three months of his illness, and his recovery may be fairly attributed not so much to the duck-shooting and whiskey-punch, but to the tonic and undepressing treatment which he adopted for himself, and which his system so much required to enable him to throw off the disease.—*Medical Times*.

PROFESSOR FORGET ON THE NON-IDENTITY OF TYPHOUS AND TYPHOID FEVER.

Although typhous and typhoid fevers resemble each other very much in their external appearances, Professor Forget of Strasburg believes that they are clinically and essentially different in their nature. They are anatomically different; they are not equally frequent in their occurrence; and typhoid fever may arise from many causes, quite irrespective of infection. In follicular enteritis the typhoid condition is not constantly present, and it is usually secondary; while in typhus, the prostration and stupor, attending its invasion, indicate a powerful intoxicating principle, acting primarily as a poison, which is not usual at the commencement of the typhoid state. Gastro-intestinal symptoms are more constant, and primitive in typhoid than in typhus fever; for in it they are occasionally found existing alone; and we very rarely see cases of follicular enteritis which do not present the rough

(*saburral*), punctulated, and rosy tongue; also *gargouillement* and pain in the right iliac region, and either diarrhoea or constipation. Though other symptoms may coexist with them, they are never wholly obscured. In typhus again, especially at its outset, the tongue is frequently moist, smooth, and white; meteorism and abdominal pain are absent, and there is no derangement of the process of defecation. If we except the rosy, lenticular spots, peculiar to typhoid fever, and which constitute, according to M. Forget, its most constant differential sign, there are no symptoms in general connected with either malady which are not purely accessory and inconstant. Typhoid fever is usually slow, and gradual in its course, and in simple uncomplicated cases its evolution is in exact ratio to the development of the intestinal lesion. In typhus, on the other hand, the disease is very violent at first, and afterwards becomes suddenly or alternately better or worse, and often in those cases which appear the most unfavourable, it advances rapidly and spontaneously to a favourable termination. Ataxia characterises it more than it does typhoid fever.

As regards the treatment of the two diseases, Professor Forget says that while, in typhoid fever, this must always be adapted to the peculiarities of each particular case, special attention must be paid to the intestinal ulcerations which are present during its course, and that all those medicines should be avoided which are calculated to aggravate these lesions of the alimentary canal. There is no need for this caution in cases of pure typhus.—*Gaz. Méd. de Paris*, Nos. 42-48. 1854.

DR PARKES ON THE DIAGNOSIS OF TYPHOUS AND TYPHOID FEVERS.

You are brought to see this young woman, we will say, for the first time; the specific rose-spots are gone; she is labouring to all intents and purposes under severe bronchitic and chest symptoms (a chemist, or practitioner with a druggist's shop, has prescribed, and given cough mixtures, perhaps, without seeing her); you find her respiration 30 in a minute, cough incessant, with some expectoration, nervous symptoms also well-marked; vertigo complained of, torpor, the eyes closed; she is delirious at night; she has also diarrhoea, pain over the abdomen, pulse quick, tongue furrowed and somewhat coated. Suppose, I say, you were called to such a patient, and moreover she is unable to give any account of the previous illness, how are you to make the diagnosis? There are only two ways—one the positive method, the other the method as it is called by "exclusion." The first is obvious enough, and will of course be more valuable to the practised eye of the experienced physician, who seizes the nature of the case at the first glance by a sort of intuitive knowledge of what typhoid really is. Now the method of diagnosis by exclusion—the plan of logic-writers, *per viam exclusionis*, in this and other diseases, is one, though not without disadvantages, one yet of no mean importance. The first question you resolve in your mind will be—Is she or he, as the case may be, labouring under any of the idiopathic fevers? any of the exanthemata? No. Is it typhus? You make the same answer, as the eruption in *ty-phus* is as different from *ty-phoid* as scarlatina from measles. The eruption is absent in patients under 22 or 21 (this patient's age is about this). Is it relapsing fever, so common some years, as 1828-29? No. You ask yourself, then, is it typhoid? Yes. Nervous symptoms are marked, chest symptoms and diarrhoea also; the latter loose, granular, yellow, so peculiar to typhoid. You have soreness of the right iliac fossa; but then you say we have no rose-spots, and then you remember in at least 20 per cent. these rose-spots are not found. You must weigh and balance all these circumstances in your mind.—*Medical Times and Gazette*.

[In Edinburgh it has been found impossible to make out the distinctions between typhus and typhoid eruptions, here so positively laid down in accordance with the opinions of Dr Jenner.]

DR SYME'S CASE OF TETANUS SUCCESSFULLY TREATED BY CHLOROFORM.

On the 18th of August 1853, I was called to visit a young gentleman, aged twelve years, who was suffering much from paronychia of his left thumb

which I treated by incision. I did not see him again until the 25th of August, on which day I found him labouring under well-marked symptoms of tetanus; the muscles of the face, neck, and abdomen were permanently rigid, and the other well-known symptoms of tetanus were present. The treatment usually adopted in tetanus was immediately put in force: thus calomel and opium, etc., were perseveringly administered up to the 30th of August, but without any marked benefit, the disease still increasing, the spasmodic seizures becoming more frequent and violent; the muscles of the jaws were so rigid, the teeth were so completely closed, and the difficulty of swallowing so great, that not even fluid nourishment could be given to the patient. In consultation with Mr Cusack, it was determined to try the effects of chloroform inhalation, which I administered with much difficulty, owing to the violent resistance which the patient made; he inhaled a considerable quantity, and remained under its full influence for upwards of twenty minutes; after the anæsthetic effects of the drug had passed away, the boy was able to swallow a glass of wine without much difficulty, and from that moment he gradually but steadily improved, and on the 19th of October he was perfectly well. The paronychia was slow in healing, the nail was not detached until the patient had nearly recovered from the tetanic symptoms.

I am fully aware that, in other hands, tetanus has been successfully treated by chloroform; thus two such cases have been lately detailed in the *Dublin Medical Press*. Every additional case is, however, in my mind, well worthy of being recorded, as it is only in this way that the power of chloroform, in certain cases of tetanus, can be made known to the profession at large.—*Dublin Hospital Gazette*.

THERAPEUTICAL NOTES.

I. NEW REMEDY FOR PRURITUS VULVAE.—Dr Scholz recommends an Indian plant, the *Caladium seguinum*, which is used by the natives of India as an anaphrodisiac, for the treatment of those cases of this most distressing malady, which are due rather to a hyperæsthetic condition of the parts, than to any lesions of the mucous membrane of the vulva. The plant belongs to the *Aroideæ*. Scholz has used it with great success in two cases which had previously resisted all remedies; and he administered it in the form of an alcoholic tincture, in doses of six drops.—*Arch.-Gen. de Méd.*, Sept. 1854.

II. THE ACTION OF COPAIVA BALSAM.—Dr Roquette of Nantes, believes that copaiva only acts as a curative agent, in affections of the urino-genital organs, by means of the peculiar qualities which it imparts to the urine, and that it is useless in inflammations of those portions of the genital mucous membrane which do not come in contact with the urine during its passage. He considers it, therefore, to be of no utility in balanitis, vaginitis, uterine catarrh, epididymitis, blenorrhagic ophthalmia, etc.; unless, in these affections, the patients do not object to take copaiva, in order to have copaiviferous urine to use as an injection, which would be no advantage, seeing they can be cured by other means more prompt and simple, and less disagreeable. As it has been satisfactorily proved, that when copaiva is tolerated, it is eliminated from the system almost exclusively by the kidneys, and that consequently it is present in large quantity in the urine,—Dr Roquette considers, that when we administer this remedy, we should endeavour to prevent diarrhœa, vomiting, nausea, etc., as much as possible, in order that the remedy may solely leave the economy through the kidneys.—*L'Union Méd.*, 14th and 18th Dec. 1854.

III. ANAPHRODISIAC PROPERTIES OF BROMIDE OF POTASSIUM.—Thielmann recommends this remedy as an excellent anaphrodisiac in satyriasis, in the frequent and painful erections during gonorrhœa, in spermatorrhœa, and in nymphomania. He administers it in doses of from 2 to 3 grs. every two or three hours; and, at the sametime, enjoins a mixed vegetable and milk diet, and forbids all acid substances.—*Med. Zeit. Russl.*, 1, 1854.

IV. HYDROCHLORATE OF AMMONIA IN CHRONIC BRONCHITIS.—Dr Delvaux alleges that, in cases of chronic bronchitis, he has found the greatest benefit from the use of muriate of ammonia, in doses of from 15 gra. to 2 scruples, in the 24 hours. Before commencing its administration he orders a purgative, and during its use he enjoins a strict regimen. He has found that its use soon renders the cough less troublesome, the dyspnoea less urgent, and the appetite better. It causes an increased flow of urine, and also an augmentation of the cutaneous transpiration.—*Presse Méd. Belge*; and *Revue Méd. Chirurg. de Paris*, July 1854.

V. SULPHATE OF STRYCHNIA IN CHOREA.—Professor Trousseau has proposed for the treatment of chorea, a mixture containing 5 centigrammes of sulphate of strychnia to 100 grammes of syrup, so that a dessert spoonful (or 10 grammes) of the syrup, contains half a centigramme of strychnia. A spoonful of the syrup is given every hour and a half till rigidity (*roider*) occurs. When sufficient has been taken to produce this effect, the rigidity occurs 10 minutes or so after the last spoonful. After this effect has been produced, the medicine is discontinued for the day, and no more is administered till the next day, when the treatment is recommenced till rigidity is again produced, and so on till a cure is accomplished.

M. Moynier has published a paper giving the results of treatment by this remedy in 43 cases,—of which 32 were girls, and 11 boys. The average date at which a cure was accomplished was the 33d day in the case of girls, and 74 days in that of boys.—*Archiv. Gén. de Méd.*, July 1854.

VI. CHLOROFORM AND ETHER COMBINED AS AN ANAESTHETIC.—M. Robert, in his report on chloroform to the *Société de Chirurgie*, recommended a mixture of equal parts of chloroform and ether as the best anaesthetic agent. M. Cellaries has subsequently published (*Gaz. Méd. de Montpellier*, 1853), the results of experiments made by him with the compound upon rabbits, etc.; and he reports very favourably as to its efficiency as an anaesthetic. The two fluids combine together, without the occurrence of any precipitate, and the odour of the compound is said to be far from disagreeable.—*Gaz. Méd. de Paris*, No. 96, 1854.

VII. ARSENIATE OF IRON IN HERPETIC AND SQUAMOUS ERUPTIONS.—On the 17th July 1854, Mr Duchesne Duparc read a memoir on this subject to the Academy of Sciences in Paris. The following is a brief *résumé* of his researches:—1st, Arseniate of iron, in common with other preparations of arsenic, possesses great efficacy in the treatment and cure of herpetic and squamous skin diseases. 2d, Its great advantage is, that it may be administered in sufficient doses without occasioning any of the prejudicial physiological effects which are often induced by other preparations of arsenic. 3d, Whether we give this remedy singly, or combined with other substances, we should always administer it in gradually increased doses, beginning with $\frac{1}{10}$ th, $\frac{1}{6}$ th, or $\frac{1}{4}$ th of a grain, according to the age, constitution, and, above all, the condition of the digestive functions of the patient. 4th, M. Duparc asserts confidently, from long and extensive experience, that a daily dose of $\frac{1}{4}$ th of a grain of the remedy, continued uninterruptedly for the requisite time, will cure any herpetic or furfuraceous affection in the adult, however old or obstinate it may be. 5th, No absolute rule can be given as to the *duration* of the antiherpetic treatment by this preparation, inasmuch as this must necessarily vary according to the patient's age and constitution; the severity of the malady; and, above all, the extent to which the drug is tolerated by the stomach. 6th, Tropical remedies of acknowledged utility, are not counter-indicated during the use of the arseniate of iron; but, on the contrary, the internal or external use of certain thermal sulphureous mineral waters, has been found to be a powerful adjuvant to the treatment.—*Gaz. de Hôpitaux*, 25th July 1854.

VIII. BENZIN OR BENZOLE AS A REMEDY FOR ANIMAL PARASITES.—Our readers are probably aware that benzin or benzole is a clear, colourless fluid,

possessed of a pungent ethereal odour, which is produced by the decomposition of benzoic acid, or other organic substances, at a light temperature. It was long ago ascertained by Milne Edwards, that its vapour was very fatal to insects. This property has led M. Reynal, of the veterinary school at Alfort, to employ it for the treatment of pedicular maladies among animals. He has found that it destroys the parasites in these diseases, more surely, and with more safety to the animal, than tobacco-juice, mercurial ointment, or any other of the many remedies used. It destroys the epizoa without at all injuring the skin.

It is proposed to use this fluid in the parasitical diseases of the human skin, especially in pityriasis, or morbus pedicularis, and in scabies, etc.—*Bulletin Gén. de Therapeutique*, 30th July 1854.

IX. LAXATIVE DRAUGHT.—The following is a formula for a laxative preparation, very much used in Germany, especially in the Grand-Duchy of Luxemburg, which is said to have the advantage, over the simple infusions of senna, of purging moderately without occasioning griping pains.—*R. Fol. sennæ. spiritus vini extractorum. ʒiv; Fol. sambuci ʒiiss; Seminis Faeniculi et sem. anisi. vulgaris. āā ʒx:—concisa et contusa misceantur: in dispensatione adde:—potassæ tartratis ʒvij.*

The draught is administered in the form of a tea-like infusion.—*Gaz. des Hôpitaux*, 17th August 1854.

X. VINEGAR IN SCABIES.—Professor Le Cœur of Caen, recommends, for the cure of itch, forcible frictions of the parts affected with a hard sponge, soaked in good vinegar, performed thrice daily, so as to penetrate the skin, and rupture the vesicles. He has tried this treatment, with the most complete success, in ten cases, the average length of the treatment being less than five days. He thinks this treatment preferable to all others on account of its speedy action—its inexpensive nature—its freedom from unpleasant odours, and its easy application. He suggests that similar results might probably be obtained by frictions with the mineral acids dissolved in water.—*Gaz. des Hôp.*, 26th Sept. 1854.

XI. SULPHATE OF QUININE IN PHTHISIS.—Dr Muntendam, a Dutch physician, has just published a paper to show that, from his experience in twenty-two cases, sulphate of quinine, given along with acetate of morphia, or even alone, is capable of prolonging the life of the patient in many cases of phthisis, and that it may even effect a cure in those cases in which a tubercular deposition has just commenced, especially in married women and children. He alleges that, when given continuously in small doses, it does not cause dyspnoea, diarrhoea, headache, or any disagreeable effects; and he believes that in very many, but not in all cases of phthisis, it should be ranked as one of the best remedies for the disease.—*Nederlandsch Lancet*, and *Gaz. des Hôpit.*, 21st Dec. 1854.

XII. BORAX INJECTIONS IN INFANTILE DIARRHŒA.—M. Bouchut considers infantile diarrhœa to be of two kinds; the one, symptomatic of lesions of the intestinal mucous membrane; the other, idiopathic, a nervous or catarrhal flux from the great intestine, which may occasion death, without leaving any material morbid appearances. In the latter variety, M. Bouchut recommends clysters containing the bi-borate of soda, as peculiarly efficacious. This remedy proves as beneficial as it does in aphthæ of the mouth, it acts as a weak astringent on the intestinal mucous membrane, and as an alkali in neutralising the acid secretions poured out by it, which lead secondarily to ulceration of the bowels, and especially of the anus.

He uses clysters containing from 10 to 20 grammes of the salt, in 125 grammes of water.

In cases of infantile diarrhœa symptomatic of intestinal ulcerations, M. Bouchut has found benefit from the use of borax, administered internally in the dose of 2 grammes, in 80 grammes of a mucilaginous emulsion.—*Gaz. des Hôpitaux*, 16th Sept. 1854.

XIII.—OXIDE OF COPPER OINTMENT AS A DISCUTIENT.—Professor Hoppe, of Basle, attaches very great importance to an ointment made with the black oxide of copper, as a discutient application. He uses it in the following cases:—

1st. In *opacities of the cornea* (half a grain to ʒj of axunge, increased to 5-10 gra.), a small quantity introduced into the eye twice or thrice daily, causes them to disappear.

2d. In *diseases of the eyes*, especially in inflammation of the lachrymal gland, of the cellular tissue of the orbit, and of the palpebral mucous membrane, he says, it produces excellent effects when it is rubbed on the temples instead of *Ung. Hydrarg.* He used, in such cases, an ointment made with ʒj to ʒij of oxide, added to twice its weight of axunge, and combined with narcotics.

3d. In *tumefaction of the external meatus of the ear*, occasioned by chronic inflammation of its cellular tissue.

4th. In *induration of the salivary glands*, which in a few days yields to its influence.

5th. In *enlargements and indurations of the glands of the neck*, it is peculiarly serviceable, causing lymphatic glands which have become sarcomatous, to soften and disappear better than any other remedy. He says, he has seen obstinate, hard, and voluminous swellings yield to a single application.

6th. In *goitres* of the ordinary kind it is very useful, although it is inefficacious in the hard, fibroid, sarcomatous tumours of the neck.

7th. *Engorgements of the mammary glands*, whether recent, or of old standing, are speedily discussed by it.

In many other maladies, as orchitis, glandular swellings of the groin and axilla, hypertrophies of the liver, spleen, etc., Professor Hoppe believes it to be efficacious. Its employment produces no deleterious effects on the economy, but it may easily produce papular eruptions, which may go on to ulceration. Its employment should be suspended whenever the skin becomes irritated, and an ointment of zinc may then be substituted. Hoppe uses it in proportions, varying from 15 to 30 grains to the ounce, of axunge. As contact with the ointment is rather unpleasant, the parts to which it is applied should be kept covered by lint and a bandage.—*Deutsche Klinik.* 1854.

SURGERY.

DISCUSSION ON THE CURABILITY OF CANCER, AND ITS DIAGNOSIS BY MEANS OF THE MICROSCOPE, AT THE FRENCH ACADEMY OF MEDICINE.

(Continued from page 69.)

M. FERRUS (author of the article "Cancer" in the *Dictionnaire de Médecine*) shortly addressed the Academy. In two cases of cancer only, out of fifty which he had observed, had he found evidence of hereditary taint to be wanting. The extent of the cancerous cachexia was not always in proportion to the severity of the local manifestations of the disease; thus, a formidable cachexia might exist with but a trifling cancer, and there was often very little cachexia present in women, in whom the whole uterus and vagina were disorganised by the malady. The influence of the nervous system had been alluded to as often causing cancer by mental perturbation. He might state, as *apropos* to the question, that among the fifty cases he had alluded to, the temperament was observed to be lymphatic in two-thirds of the whole; it was bilious and sanguine in a smaller number; and it was nervous in a still smaller proportion.

Dec. 5, 1854.—M. BOUILLAUD next spoke, and entered at some length into a eulogium of the Parisian school of medicine. He paid a very high tribute to the speech of M. Velpeau, and expressed his firm adherence to all the views of that surgeon as to the diagnosis and curability of cancer. "The micrographers," said he, "who believe that the cancer-cell is pathognomonic of cancer, have met a terrible adversary in M. Velpeau. His second speech was a *cellulicidal* discourse (*discours cellulicide*)." He enjoined upon the microscopists the

necessity of redoubled efforts, and of firm unanimity among themselves. He stated very truly :—"I do not doubt that the microscope always gives the same results ; I affirm that it never deceives, but I hold that the microscopists are not infallible. Let us not lay upon the instrument the errors and imperfections of the many who use it. It is no easy task to observe, discriminate, and interpret well. It is a very difficult thing to manage the microscope properly. I have assisted M. Broca in microscopical observations, and all those who were then engaged with him saw the same objects, and made the same designs of them. Errors and discrepancies can only arise from one source, viz., from observing different objects, and believing them to belong to the same substance. But I cannot admit that experienced microscopists can ever be at variance about the result of such simple observations."

He believed that the microscope had rendered very important services to science, and to the study of Cancer, because it had been able to establish a satisfactory classification of tumours into epithelial, fibro-plastic, and cancerous. He again maintained that the microscope should not be made responsible for the errors of the microscopists. When it happened that these observers were disagreed among themselves about an object, their difference of opinion was generally attributable either to the inexperience of some of them, or to the circumstance of their not having all examined the same substance. He had no doubt that, instead of morbid tissues, they had often unwittingly examined those which were normal, sometimes cellular, and at other times muscular, vascular, or simply hypertrophied tissues, and chance might have made it occasionally happen that in tumours of a complex nature, one of the elements had been examined by one histologist, a second element by another, and so on.

He concluded by proposing that the Academy should appoint a committee who should investigate the subject of cancerous tumours, either by themselves, or aided by experienced microscopists, as often as they might consider this assistance necessary.

M. ROSTAN observed that the Academy had, in some measure, anticipated M. Bouillaud's suggestions ; and that the question under debate was to be given out to competition for one of the Academy's prizes.

Jan. 2, 1855.—M. ROBERT expressed his dissent from what had been stated by M. Delafond, that the cancer-cell was not characteristic or specific in its appearance and shape. M. Delafond had asserted that the form of this cell was not constant in its form : that it was spherical or ovoid when it was not compressed by the surrounding tissues, and that it was elongated or fusiform when it existed under opposite conditions. These plausible statements were not borne out by facts, for he had often seen ovoid and spherical cells in the densest schirrus, as well as elongated cells in softened encephaloma ; and moreover, fusiform bodies were common in subcutaneous fibro-plastic tumours, which were quite exempt from compression. M. Delafond had stated that the volume of the cells varied according to the phase of their development, but this was quite unproven. M. Robert exhibited a series of drawings illustrative of the appearance of the cancerous, fibro-plastic, and epithelial elements, and also of pus globules, and he showed that, although occasionally they might present somewhat abnormal aspects, there could be no difficulty, in the great majority of instances, in recognising them, and distinguishing them from one another.

Another objection raised against the specific nature of the cancer-cell was, that in several of the normal tissues of the body we found cells which presented a very great analogy, if not a perfect similitude, to those of cancer. This view originated with Virchow, and for a time was popular in Germany. According to that pathologist, the pavement epithelium of the mucous membrane of the ureters, the bladder, the lungs, and the conjunctivæ, resembled very much the cells of cancer ; and scarcely a year ago M. Michel, of Strasbourg, had traced the same analogy in the cells of the medulla of the bones of young children. M. Robert had examined all these structures, assisted by able microscopists, in order to ascertain the truth of the statements ; and after

repeatedly examining and comparing together the tissues mentioned above and the elements of cancerous growths, he never experienced any difficulty in distinguishing the different structures. It might be asked, Why have distinguished histologists in other countries arrived at different results? Might it not be due to differences in the power of the microscopes used? A microscope of high powers revealed structural appearances which were invisible to any one using an instrument of lower powers. Cancerous elements appeared very differently when viewed under powers of 300 and 500 diameters. He thought, therefore, that the objections raised against the specific character of the cancer element were more specious than real, and he maintained that an experienced eye could always distinguish them from other structures, whether normal or pathological.

M. Velpeau had principally contested the specific nature of the cancer elements in a pathological and clinical point of view; and he alleged that he had found cancer-cells in non-cancerous tumours, which were extirpated without the occurrence of any relapse. But M. Robert held that, if M. Velpeau admitted the curability of true cancer by operation, the non-recurrence of the tumours in question did not prove them to have been non-malignant. M. Velpeau said he did not found his diagnosis on the non-recurrence of tumours, but on the general appearance of the patient. But among all the admitted clinical signs of cancer, there was not a single pathognomonic unmistakable symptom, when the cachexia was absent, and generalisation of the disease was not yet manifested. Modern histology was daily showing us that the symptoms reputed to be characteristic of cancer could not be relied upon as such, since they were being constantly met with in morbid productions of a totally different nature. For example, it had been held, from time immemorial, that retraction of the nipple, occurring in tumours of the breast, was a pathognomonic sign of schirrus. But M. Robin had lately described a form of mammary hypertrophy, in which this retraction of the nipple was very well marked, and which consisted simply in a hypertrophy of the glandular *cule de sac*, and in atrophy of the excretory ducts, which give to the breast the aspect of schirrus. Two cases of this, which had recently been witnessed within a very short time, led him to believe that this affection was by no means rare.

Again, when a mammary tumour was extensively ulcerated, when its edges were fungous, sprouting (*vegetans*), and its base was greyish, sanious, discharging foetid pus in great abundance; and when this was accompanied by hemorrhage and exhaustion, we said that we had to deal with a serious case of cancer. But, to M. Robert's own knowledge, a combination of all these alarming symptoms had occurred in two cases of simple hypertrophic tumours. In one of these cases he himself had operated with complete success, and the patient had for five years enjoyed perfect health. The other case was similarly successful in the hands of a celebrated surgeon in Paris.

But M. Velpeau had further asserted that the specific cancerous elements had been found absent in truly cancerous tumours. He had searched M. Velpeau's book in vain to find proofs of this.

M. Velpeau had also stated that tumours which at first contained no cancer-cells, had been known to recur, having these cells in the secondary tumours; and he had cited several cases of this nature; but in all these cases the *primary* tumours had not been microscopically examined, and therefore there was no proof of their non-cancerous nature.

As to the benignity or malignity of the tumours, he thought that it was better to direct our attention to their prognosis and manner of evolution than to their anatomical structure. *All tumours were relatively benign or malignant.* A true schirrus, which had existed eight or ten years, without implicating the glands, or causing pain or cachexia, and which, after its removal, did not recur, or did so only after the lapse of a few years, was extremely benign when compared to an encephaloma, which ran through all its phases, and killed the patient in less than a year. The same was true of epithelial, fibro-

plastic, and other tumours not so well known, which recurred after ablation. A *noli me tangere* of the cheek, or ala nasi, was a hundred times more benign than an epithelioma of the under lip, of the rectum, or of the cervix uteri. And farther, such a schirrus as he had described was much more malignant than a simple adenoid tumour, of large size, which was ulcerated, and discharging fetid pus profusely. If, therefore, in the majority of cases, structural characters were constant and easily demonstrated, nothing could be more variable and uncertain than the prognosis and diagnosis we might base simply upon the evolution of morbid products.

We had then two problems to solve; that of structure, and that of prognosis. The solutions of these were not antagonistic, and we ought not to seek only to solve one of the questions. But the science of the present must precede that of the future, and anatomical diagnosis must precede prognosis.

This view appeared to him to be a scientific and practical one. What had been done respectively by the anatomical and clinical men, at the bedsides of the patients? The former had divided tumours into cancerous, epithelial, fibro-plastic, vascular, glandular, etc. By long and patient study they had become acquainted with the delicate characters of the tumours, which were long passed by unobserved; and they had tried to discover in what particular lesions particular elements were found, and thus they had frequently been successful in establishing a correct diagnosis. They knew that one tumour might remain for a long time unremoved, without serious injury to the system; that another could not heal spontaneously; and that the former was more benign than the latter, etc. They thus formed a mental estimate of the patients' history, weighed their good and bad chances, and finally were able to say in what cases it was right to operate, and in what other cases it was prudent to leave the disease undisturbed.

But what had the purely clinical men done? Was their judgment, based on traditions of doubtful value and on pure empiricism, better than the more complete and exact views of the anatomical school? When one of them was called to treat a tumour, if he found that the patient had a bad aspect, and a sinister physiognomy, he removed the growth, like an anatomist. But while the latter, in such circumstances, strove first to confirm, by means of the scalpel and the microscope, his first impressions as to the nature of the disease, in order to strengthen his practice if he was in the right, and to modify his first opinions if he had been deceived, the former, limiting the circle of his researches, made an uncertain diagnosis and a doubtful prognosis.

It was more philosophical and conformable to science to base our classifications of tumours on their anatomical composition and structure. If the microscope had dispelled many illusions as to the curability of true cancer, it had also revealed to us the possibility of curing diseases which often, but for it, were allowed to proceed to a fatal termination. The microscope could not, any more than pathological anatomy in general, decide emphatically in every case, the benignity or malignity of a morbid production, but it enabled us, better than mere clinical observation, to foresee the result of treatment. In certain cases it gave us caution, and in others boldness. By means of the microscope we had made great advances in the study of all accidental growths; and if we became hereafter perfectly acquainted with the obscure question under consideration, much of the praise would be due to those who had brought so much ardour and laudable perseverance to these truly difficult researches.

Jan. 8, 1855.—M. DELAFOND discussed at great length the various cell-theories, and quoted the opinions of a great many authors in support of the doctrine of cellular unity. Besides the authority of the German microscopists, Virchow, Schwann, Förster, Müller, he cited passages from the works of MM. Lebert and Broca themselves, which were favourable to his opinion. He terminated his address by enunciating the following conclusions, which he thought were proven:—

1st. Independently of *normal* cells, there exist in the organism *pathological* cells, which cannot be distinguished by fixed and constant characters.

2d. In the origin of their formation, the three cancer-cells—(the cancer-cell properly so-called, the epithelial, and the fibro-plastic cells), present no marked differences among one another.

3d. If, under certain circumstances, these cells, especially fibro-plastic cells, present differences of form and dimensions, these differences are only due to the varying densities of the tissues in which they are developed.

4th. The chemical characters, by means of which it has been endeavoured to distinguish cancer-cells, are not sufficiently well-marked to constitute a trustworthy distinction.

5th. The effects of chemical reagents are equally inadequate to afford a sufficient ground of distinction.

6th. Numerous facts have proved that true cancers do not always contain the cells called cancer-cells, while these cells have been found in tumours and tissues which were not cancerous.

7th. Still more numerous facts prove that epithelial tumours, which are not considered to be true cancers, are capable of recurrence not only at the same place, but at a distance from it.

M. VELPEAU.—I have just published a work in which I affirm that cancer is sometimes curable, and that it can be very often diagnosed, without the aid of the microscope. M. Robert having disputed these propositions, I have tried to defend them. *Firstly*, I have endeavoured to prove that cancer can be diagnosed independently of the aid of the microscope; but I have not meant by this to say that the microscope was useless, and that the works of microscopists were of no value, as has been imputed to me. *Secondly*, I have discussed the alleged existence of cancers having no cancer-cells. And *Thirdly*, the question whether or not cancer was curable. The first point has never been seriously contested, and the curability of cancer in certain cases does not appear to me to have been refuted. As regards the question of the specific nature of the cancer-cell, I am not competent to solve it in a microscopical point of view. I have only attacked it clinically. The recurrence of tumours containing no cancer-cells has convinced me that the cell is not the pathognomonic sign of cancer. M. Robert has contested my facts, and asked how I knew that it was a cancer by resting my opinion on its recurrence. Let me, once for all, answer the question.

Its non-recurrence is to me a proof that a tumour is not cancerous only when this fact is found existing in conjunction with other circumstances possessing an equal negative value. I have always founded my diagnosis on the combination of these negative characters. I admit that I may have been sometimes deceived, but how can I have been constantly deceived throughout the numerous series of cases? M. Robert has spoken of the retraction of the nipple not being a pathognomonic sign of cancer, but on what grounds does he allege that M. Robin's two cases of mammary tumours were not cancers? You answer,—On the absence of cancer-cells. But I knew both cases, one of which was my own, and in it the malady recurred two months and a half after the operation. How can this be said not to have been cancer, or how can confidence be placed on the absence of the cell as a diagnostic mark? No one has pretended that retraction of the nipple *alone* constituted a specific mark of cancer; for how can we diagnose a malady by a single character?

After a few remarks on fibro-plastic tumours, M. Velpeau postponed the conclusion of his address till the next meeting.

CASES OF HYPERTROPHIED FINGERS AND TOES.

Mr ADAMS, surgeon to the Richmond Hospital, Dublin, at a meeting of the Pathological Society, April 8, 1854, brought forward the case of a girl, *æt.* twelve years, who had been born with the index and middle fingers of her left hand in a state of hypertrophy. The subject of this observation was a native of the Isle

of Achill, and was placed under the care of Mr Adams, in the Richmond Hospital, by his relative, Dr George Montgomery. At the period of her admission into hospital (viz., April 7, 1853), the enlargement from congenital hypertrophy of the middle finger had proceeded to such a degree, and had reached such magnitude, that Mr Adams could not meet any parallel to it among the recorded cases of this lesion. The cast of the hand (represented by the wood-cut) will at once give an idea of the nature of the deformity.

The girl was healthy and intelligent, and, with the exception of the left hand, was in other respects well formed. On looking at the cast of the forearm and hand, we notice that the hypertrophied middle finger is of an enormous bulk and length, compared with that of the opposite hand; for example, while the middle finger of the right or normal hand of this little girl measured three inches and a half, the length of the hypertrophied middle finger of the left hand amounted to eight inches; its circumference, at its largest part gave the same measurement. The nail of this middle finger was very broad and strong, fully an inch square, and constituted a very remarkable feature in this extraordinary case. The direction of this finger was altered, and much adducted towards the ulnar side; the index finger was also hypertrophied, but by no means to the same degree as the middle finger—it was drawn towards the radial side, and at the same time incurvated in that direction, so as to be greatly divaricated at its extremity from the middle finger.



The index was four inches and a half long, and its circumference, at its greatest part, amounted also to four inches. On the dorsum of the metacarpus, as it were continuous with the root of the hypertrophied middle finger there existed a tumour apparently consisting of fat, the size of half an egg; above this tumour, and corresponding to the back of the carpus, another distinct swelling was perceptible, which was nearly of the same size and form; a third tumour, also apparently of a fatty nature, but not circumscribed, existed on the upper part of the forearm (see woodcut). The skin covering the hypertrophied fingers was of a deep pink colour; its temperature was natural, as was also the sense of feeling. She had the power of moving the joints, but imperfectly, and she noticed that their movements were becoming each day more difficult from the increasing growth of the middle finger. As the girl grew older the feeling of personal deformity became a frequent source of discomfort and unhappiness to her.

As to the history of the origin of this abnormal condition of the hand, it was ascertained from Dr Montgomery that the child was born with the index and middle

fingers three times their natural size. The child's father stated, that when she was born her middle finger was just the size of his own little finger. Its growth at first was only in proportion with the child's growth; a year or two since it had commenced perceptibly to increase, and continued to grow more rapidly than the rest of the hand, until it attained its present excessive size.

It was a matter for deliberation, whether anything should be done further than to cut off the principal offending parts, namely, the hypertrophied fingers, which it was thought could be readily accomplished without danger to life or interfering with the functions of the wrist joint, so important to be preserved in a state of perfect integrity. At the same time, it was to be borne in mind, that such an operation would not remove the whole of the abnormal parts, and the patient would thus be liable to some recurrence of the evils she laboured under. To remove all the fatty mass which lay on the dorsum of the wrist and forearm, together with the entire of the two metacarpal bones, might be feasible, but, on due deliberation, Mr Adams did not think that in such a case, where the life of the girl was not in danger from the deformity, it would be justifiable either to have recourse to such an extensive dissection, or to encounter the danger which might be anticipated to arise from constitutional irritation following the mere dissection of the fatty masses. Moreover, the attempt to remove the entire of the hypertrophied metacarpal bones would probably involve the opening into some of the carpo-metacarpal joints, and thus indirectly lead to a communication between the external air and the interior of the wrist joint, likely to be followed with the worst consequence.

Of the two evils, therefore, it was decided that the lesser appeared to be leaving the three fatty diffused swellings on the dorsum of the hand and forearm, together with some portions of the two hypertrophied metacarpal bones, rather than encounter the risk of exciting constitutional irritation, and more particularly of exciting inflammation of the wrist joint. Mr Adams hence determined not to interfere with those tumours, but to remove the two hypertrophied fingers, by sawing through their metacarpal bones as high up as he conveniently could.

Operation.—An incision was made on the radial side and dorsum of the metacarpal region down to the bone, extending upwards towards the annular ligament of the wrist joint; a similar incision was made to the ulnar side of the hypertrophied middle finger, to meet the former at an acute angle, salient upwards; each metacarpal bone was cut through obliquely with a small saw, in the direction of the two incisions (see lines marked in woodcut), and the hypertrophied fingers removed: the greatly divaricated fingers were then approximated as much as possible, and the integument which had covered the radial side of the index finger was applied to the cut surface on the radial side of the ring finger and its metacarpal bone; one suture was applied at the lower angle of the wound. There was very little hæmorrhage during the operation; and as, of course, chloroform was used, there was no pain experienced.

Mr Adams also laid before the meeting the amputated part, comprising the lower extremities of the two corresponding metacarpal bones, which were somewhat hypertrophied. A longitudinal section of the middle finger being made, displayed the hypertrophied head of the metacarpal bone, and the three phalanges, of a length greater than they measure in the skeleton of any giant preserved in any museum.

The epiphyses were not yet ossified, the cartilages and synovial apparatus were perfect, the bones were firm, and of a normal structure; but the section of the finger everywhere exhibited a vast predominance of adipose structure, which constituted, with the elongated and hypertrophied bones, the bulk of the deformed mass. The extensor and flexor tendons were thrown to the radial and ulnar sides of the index and middle fingers, thereby accounting for the deviations from the right line which the fingers had undergone. The wound healed rapidly, and the patient was discharged from hospital on July 23.

Mr Adams wishing to learn how this operation had succeeded, before bring-

ing the case forward, wrote to Dr George Montgomery, and learned from him, by a letter dated the 14th March 1854, that this little girl had excellent use of her left hand; she could earn her bread, sew, and do crotchet work, and pass unobserved, which she could not previously. Her father told him she could make as good use of this hand as any other little girl in the village could make of theirs. She expressed herself much gratified by having been relieved of a source of "discomfort and reproach." *Observations.*—The cases of congenital hypertrophy of the fingers, as yet published, are not very numerous. Dr John Reid has given the particulars of a case of this kind, in which the radial artery of the affected side seemed to be double the size of the opposite and healthy one.—*Monthly Journal*, 1843, p. 198. Surgeon Robert Power, of this city, some years ago, adduced a case in the *Dublin Journal*, of congenital malformation of the middle finger, in a child aged five years; in this case, if we are to judge from the engraving, the fatty tissue was very great, increasing the breadth of the finger more in proportion than the length; and in this case also, there was apparently a fatty tumour connected with the base of the first phalanx. Mr T. B. Curling has introduced into the *Medico-Chirurgical Transactions* a very interesting account of "a case of congenital hypertrophy of the fingers in a girl, with a notice of some similar cases."

In conclusion, Mr Adams said, that he thought the case worthy of notice on the following grounds:—First, that the middle finger in this example of abnormal hand had attained a size unequalled in any as yet published. Secondly, as it is the only case in which the anatomy of the affected fingers has been displayed; and thirdly, he might add, the only case yet recorded in which any feasible remedial measures had either been proposed or adopted.

DR BENJAMIN SIMPSON of the Hon. E.I.C.S., now at Lahore, records the following interesting case:—"I met with the subject of the following sketch, in marching through the upper provinces of India, where I have seen numerous cases of a similar nature, though by no means so exaggerated in form. This is also the experience of many medical men of long standing in the service, to whom I have showed the case. I made the present sketch in my tent, when I first saw him, and at the same time, took accurately the measurement of the feet, which were as follows:—



Right Foot.—Circumference of the foot, 9 inches; length of big toe, $1\frac{1}{2}$ inches; circumference of leg near ankle, $8\frac{1}{2}$ inches. *Left Foot.*—Circumference of foot, $15\frac{1}{2}$ inches; length of big toe, $4\frac{1}{2}$ inches; circumference of leg near ankle, $7\frac{1}{2}$ inches; circumference of big toe, $7\frac{1}{2}$ inches; circumference of second and third toe together, 8 inches; circumference of fourth and fifth toes, $1\frac{1}{2}$ inches; length of second and third toes (united) $3\frac{1}{2}$ inches.

The patient could give no account of his case further than that the swelling had existed from his childhood, gradually increasing, and was free from all pain, though a source of much inconvenience

from its excessive size and weight. On looking at the measurements, the

following may be observed :—The circumference of the big toe is exactly the same as that of the leg on that side, which is slightly atrophied. This latter may be accounted for by the fact that he was in the habit of merely dragging the limb after him, the muscles never being called into play on the diseased side.

The second and third toes form but one heap, and equal in size the sound leg. The bones, however, are distinct, and can be felt moving one on the other ; the fourth and fifth toes (of which a small portion only is visible in the sketch) are of the normal size, measuring together $1\frac{1}{4}$ inch ; the nails are all perfect, but proportional to the size of the toe in each case. I omitted to mention that the patient was one of the class called in India “grass cutters,” who supply the camps with provender for the cattle. This man was in the habit of walking fifteen miles per diem, on an average, carrying for half the distance a large bundle of grass on his head.—*Dublin Hosp. Gazette*.

ON CYSTIC TUMOURS, BY VERNUEIL, VELPEAU, MALGAIGNE, ROBIN, ETC.

Three modes of origin are recognised for the development of cystic tumours—(1) by the enlargement of spaces in the areolar or other tissues of the body ; (2) by the dilation of natural ducts, sacculi, or bloodvessels ; (3) by the growth of newly-formed elementary structures, having the characters of cells or nuclei. Of the first variety, specimens are familiar to all in the bursæ mucosæ, which form wherever two hard moveable substances come into contact in the body. Of the second, we have several additional illustrations in the recent records of surgery. Of the third, to which Rokitsky attaches no small importance, no considerable additional confirmatory evidence has been received. The hydatid testicle of Sir A. Cooper is shown by Mr Curling to consist in dilatation of the seminal tubes.¹ Sir B. Brodie's statement of the origin of cystic disease of the mammary gland in dilatation of the lactiferous tubes has been confirmed by frequent dissections. Mr Quekett's injections have satisfactorily proved that cystic degeneration of the kidneys commences in the Malpighian capsules, or the tubuli uriniferi. Mr Coote has traced the origin of certain cystic tumours to dilated bloodvessels, and especially veins. Dr Verneuil² has contributed the account of cyst formation in consequence of dilatation of the ducts of the sweat-glands. Cysts about the joints have been shown, not only by most English writers on surgery, but also by the French—namely Velpeau, Malgaigne, Foucher, Bauchet, etc., to be derived either from some synovial bursa or sheath, connected with the tendons, or from some process of the synovial membrane. In skin, in mucous membrane, in gland-structures, in bloodvessels, and about joints, cystic tumours may be referred to the dilatation and enlargement of pre-existing tubes. In the ‘Gazette Medicale de Paris’ (Nos. 22 and 23, 1854), M. Robin classifies erectile tumours, according to the experience of many English surgeons, into—(1) tumours formed by the dilatation of capillaries, nævi materni ; (2) tumours formed by the dilatation of veins ; (3) tumours formed by the dilatation of arteries ; (4) tumours formed by the extravasation of blood, around which a wall forms ; or aneurisms by erosion. Rokitsky objects to the term “erectile tumours,” as universal in such cases, assigning a different mode of origin, and substituting the name “cavernous tumours,” as more accurately expressing their character. He states,³ that the stroma of erectile tumours is formed of a network of fibres, analogous to those of cellular tissue. The thickness of the septa is very variable ; from the thicker ones that radiate others, more delicate, which incompletely circumscribe irregular spaces, communicating one with another. In these varieties we find blood, either liquid or coagulated, or concretions resembling phleboliths. The seat of these tumours is variable ; but they are most commonly met with in the liver. Next in order of frequency, Rokitsky puts the subcutaneous cellular tissue, the skin ; the face, the trunk,

¹ Med. Chir. Trans., xxxvi.

² Gazette de Paris, 53, 1853.

³ Zeitschrift der K. K. Gesellschaft der Aerzte zu Wien, von Hebra, 1854.

the limbs; the cranial bones, the dura mater, and the pia mater. The primitive element of development in these tumours consists, according to the Viennese professor in trabeculae (minute septa), sometimes transparent and smooth, sometimes slightly striated, in oblong nuclei, and in fusiform caudate cells. From these trabeculae extend processes which bound irregular spaces, in which blood-vessels become developed. He affirms that the independence of these tumours from surrounding vessels at the commencement of their formation is illustrated by their development in the liver, where it will be found that the blood does not enter them, except by the perforation of their areoles. We cannot say that in our opinion these views, however elaborately worked out and cleverly expressed, are yet substantiated. On the contrary, the immediate dependence of such tumours upon bloodvessels seems to receive, from dissection and examinations, yet further corroboration.

The dependence of cystic degeneration of the kidney upon dilatation of the Malpighian capsules and uriniferous tubules has received confirmation from an observation of MM. Guilleton and L. Ollier, entitled, "Upon the Abnormal Development of the two Kidneys, in a Fœtus, offering an Obstacle to Accouchement."¹ The patient, æt. 33, the mother of four children, had reached the full term of her fifth pregnancy without the occurrence of any circumstance worthy of note, except that the abdomen was much more prominent than usual. July 1. She felt something slip into the vagina, while making a violent effort at stool; it proved to be a foot-presentation. Manual efforts to extract the child were unavailing, and the patient was conveyed to the Charité Hospital, where delivery took place, by the contractions of the uterus, and the patient recovered, although the case was complicated with an attack of metro-peritonitis.

The abdomen of the fœtus, of great size, seemed, when opened, to be entirely occupied by two enormous tumours, extending from the sides to the mesial line. They were the kidneys enormously enlarged, and containing a multitude of cysts. Microscopical examination showed that the tumours contained, as principal elements, uriniferous tubules and closed vesicles; and the authors consider, after noting the relations of the vesicles with the vascular element of the organ, and the disposition in the middle of the cortical substance, that the hydatiform element belonged to the glomerules of Malpighi. Thus the morbid change consisted in hypertrophy, of hydatiform character, of the glandular substance, without the formation of any new product.—*Brit. and. For. Med. Chir. Rev.*

MR JOSEPH B. BROWN'S CASE OF EPILEPSY, TREATED BY LIGATION OF THE COMMON CAROTID ARTERY.

"June 2, 1848.—Miss Elizabeth W., æt. 22, has been subject to attacks of epilepsy since her 17th year. Some four years before this time, she first felt a strange sensation in her right forearm; it gradually passed above the elbow, and now appears at the intersection of the omohyoideus by the trapezius, and precedes the convulsions as a true *aura*, being described by the patient as a stream of warm fluid issuing from this spot. Menstruation was normally established before the disease became confirmed. The patient is now constantly afflicted with convulsions, which vary in character and intensity; those occurring in the night being of the most violent convulsive character, while those during the day are generally less aggravated. Sometimes a week or two intervenes without any attack; but she states that she has had as many as twenty-four of the fits in the twenty-four hours. Her face bears constant marks of the violent injuries she receives from falls against articles of furniture, when prostrated, as she often is, without warning, while engaged at her usual avocations, or walking across the floor. She is remarkably intelligent, and of excellent physical development.

"Her attending physician, Dr Z. Pitcher, of Detroit, despairing of affording

¹ Gazette Médicale de Lyon, Août, 31, 1853.

her any relief from the ordinary means of medical treatment, which have been properly and perseveringly tried, has consulted with me as to the propriety of attempting a cure by a surgical operation. I saw the patient to-day, and finding no sign of disease external to the brain, to which the convulsions could be referred, agreed with Dr P. as to the propriety of attempting to relieve her by applying a ligature to the common carotid artery of one side. *It was decided not to use chloroform*, as generally hazardous in operations of importance about the head, and for such a one as this, and in such a case, as particularly imprudent.

"5th. Operated to-day, assisted by my brother and Dr Alfred Brush, Dr Pitcher and some other medical gentlemen being present. The vessel upon the right side was selected, and tied *above the omohyoideus*, a slight enlargement of the thyroid gland interfering with the artery below. *No chloroform was used*. Pulsation immediately ceased in the temporal and facial arteries of the side, upon tightening the ligature, but no perceptible change in the feelings of the patient was experienced."

The above is an extract from my note-book, kept at the time. The relief afforded by the operation was immediate. The ligature came away on the twentieth day, long before which time pulsation was re-established in the right temporal. Constant service upon a remote frontier has until recently prevented me from hearing the subsequent history of the case. Dr Pitcher reports (July, 1853) that "the aura has never returned, neither have the grave and convulsive forms of the disease; but within the past year, when exercise is omitted, and any unpleasant mental disturbance takes place at the same time, very slight returns of the *petit mal* occur, but never to occasion loss of consciousness. The health of the patient is now perfect. Her temper, which had been rendered irritable by the disease of her nervous centre, is very much improved, and her appreciation of existence greatly enhanced."

For more than *three years* she was exempt from seizure of any kind.

The history and statistics of ligation of the carotid have been made the subject of a valuable essay by Dr Norris, of Philadelphia.¹ His tables show that this operation, though several times performed for cases of epilepsy, has never been successful for the cure, but all have recovered from the effects of the operation. He says that "the idea of curing epilepsy by tying the carotid seems to have been founded on false principles." It is certainly difficult to reason upon this matter, when the pathology of epilepsy, the species designated as *centric epilepsy* especially, is so imperfectly understood. Should it depend upon a functional derangement of the relation between the arterial and venous circulation within the head (as probably in the above case), a measure which would so materially diminish the force and volume of the blood sent to the brain as ligation of the carotid, would seem, if not expressly indicated, at least to demand a trial. It is true, the difficulty of diagnosing cases in which this derangement operates must ever exist; but the above case certainly shows that the most aggravated forms of the disease may arise from some such cause, and that mechanically checking the volume and force of the arterial blood poured into the brain may restore the balance and effect a cure. The operation has never proved fatal, though uniformly, till the present case, unsuccessful as a cure; but with the experience of this *one*, productive of so much benefit, restoring the patient from a state of existence too horrible to endure, to the comfort of health, it seems to me that, in proper cases, we should not be justified to abandon it entirely.—*American Journ. of Med. Sciences*.

[The great difficulty in all such cases is to determine whether the operation acts locally on the brain, or only like many other things in epilepsy, simply on the imagination of the patient.]

¹ Am. Journ. Med. Sciences, July 1847.

Part Fourth.

MEDICAL NEWS.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XII.—Professor SIMPSON, President, in the Chair.

ON RECURRENT ABORTION AND PREMATURE LABOUR. BY DR KEILLER.

Dr Keiller directed the attention of the Society to the frequent recurrence of abortion and premature labour in the same individual, and made some observations on the various predisposing and exciting causes which are supposed to operate in such cases. Dr K. mentioned several illustrative cases, and stated that he had recently attended a patient who had aborted, or given birth to still-born or putrid children, in seven out of her ten pregnancies. The following are the particulars of the case referred to.

Mrs F., æt. 32, married ten years, has had ten pregnancies, of which three terminated in miscarriages, four in premature births, and three in the delivery of full grown children, now alive, their ages being respectively nine, seven, and five years. Knows of nothing of any consequence in regard to her previous health that could account for her losing so many children. Has had, however, several attacks of jaundice, both before and after marriage.

The results of the ten pregnancies were:—

- 1st. Male, now alive.
- 2d. Miscarriage at about third month.
- 3d. Miscarriage at fifth month.
- 4th. Female, now alive.
- 5th. Miscarriage at six weeks.
- 6th. Female, now alive.
- 7th. Female, premature about seventh month, dead about ten days.
- 8th. Male, about full time, dead about ten days.
- 9th. Male, premature, about eighth month, dead about eight days.
- 10th. Male, premature, about seventh month, dead about seven days.

There was nothing peculiar in the deliveries except in the two last, the one being an arm presentation, the other requiring the placenta to be detached from the uterus, and removed piecemeal, there being considerable hemorrhage during its removal. In the former case the placenta was hypertrophied, and presented the usual characters of fatty degeneration. In the latter case, the cord, although unusually thick, was easily lacerated, the coats of its vessels being evidently diseased. The placental structure was also found to be fatty.

The causes assumed for the abortions were extra fatigue, etc.

The three living children were nursed, 11, 14, and 20 months, during which time she did not menstruate except one month, before weaning the last child.

She usually fell in the family way immediately after weaning her children, only "altering once afterwards." And with all the abortions and dead children, did not menstruate oftener than twice or thrice between each.

She felt occasional "stitches and pains in the side" when pregnant with the still-born children.

TURNING IN CHEST COMPLICATION. BY DR MOIR.

Dr Moir communicated the particulars of a case of severe chest complication, in which he succeeded in safely expediting delivery by the operation of

turning. He stated that during one of his visits to the Maternity Hospital, in which the case occurred, he found the woman sitting up in bed, the breathing so laboured as to lead to the fear of immediate suffocation; the lips livid, and the utmost distress depicted on the countenance. As a chance of possibly alleviating the patient's sufferings, he at first resolved to induce premature labour. With that view, and as a means of inducing it as early as possible, a sponge tent was introduced into the os uteri, and warm water was also injected into the vagina. He, however, did not think it advisable to allow the woman to pass through the various steps of labour, believing that she would have died under the pains, if at all severe, accordingly, so soon as the os uteri was enlarged to the size of a crown piece he proceeded to turn, and delivered the patient without much difficulty. She was at first a good deal exhausted, but ultimately did well so long as she remained under Dr Moir's care.

NOVEL VAGINAL PLUG.

Dr Keiller exhibited a *vaginal plug*, which had been used in a case of concealed delivery and suspected infanticide, with the view of obviating the discovery of any lochial indication of the recent delivery. The plug consisted of a considerable sized oblong or somewhat conical shaped bag (hurriedly constructed out of a piece of old striped petticoat, or corset cloth), and stuffed, so as, from its form and saturated appearance, to have thoroughly answered its intended purpose—that of a well-fitting and sufficiently absorbing plug. On making a section of this rude tampon of undoubted home manufacture, it was found to be ingeniously *stuffed with bran!*

Dr K. stated that it was found along with the dead body of a child, which he had examined medico-legally, at the instance of the authorities.

INCISION OF CERVIX UTERI IN CASES OF RIGIDITY OF THE OS IN LABOUR.

Dr Simpson communicated some observations on the propriety of incising the cervix uteri, in certain cases of rigidity, and referred to the lacerations which frequently take place in the os in natural labour; from the occurrence of such natural wounds, Dr S. deduced the propriety of having recourse to artificial wounds or lacerations in some cases. A conversation followed on the various modes of artificially dilating the os uteri.

EXTRA-UTERINE CONCEPTIONS.—*Dr Simpson* exhibited three specimens of extra-uterine conceptions, and made some remarks on the cases.

PREMATURE LABOUR INDUCED BY WARM WATER INJECTIONS.

Dr Thomson mentioned the particulars of a case in which he had adopted this method with success. The patient had, in her previous labours, required interference in consequence of defective brim. The child presented by the feet, and considerable extractive force was required to complete its delivery. A conversation took place on the various modes of operating with the douche, and several cases were referred to by the members present.

OVARIAN TUMOURS.

Dr Simpson stated that he had recently seen a very large ovarian tumour discharging its contents by the urinary bladder, and made some remarks on the modes of performing "paracentesis," and the rules in regard to the first operation. He had now discontinued the use of the abdominal bandage while operating, the position of the patient lying well over on her side he considered, from his experience, sufficient.

VAGINAL MELANOTIC TUMOUR.—*Dr Simpson* exhibited a melanotic tumour found in the vagina, and made some remarks as to the rarity of such a case.

CASE OF SPINA BIFIDA.

Dr Keiller stated that he had visited, along with *Dr McGregor*, an infant presenting an aggravated form of *spina bifida*. The arrest of development was situated in the lumbar region, and from the large size and appearance of the tumour at birth, the vertebral deficiency seemed unusually great. When seen by *Dr Keiller*, the tumour exhibited the character of a large vascular fungus containing fluid, its surface was very livid, its walls thin, and as if altogether devoid of cutaneous covering, the skin apparently terminating in an atrophied and irregular form, at some distance from the circumference of the tumour. The child was in every other respect healthy and well formed. The tumour became more and more discoloured, and burst about the 8th day, when, by the escape of its fluid contents, it became quite collapsed. This was not followed by any immediate bad effects on the child, it continuing, although somewhat restless, to suckle as greedily as before the rupture of the spinal cyst. The now collapsed and undefended mass, however, took on an inflammatory action, which soon gave rise to considerable constitutional irritation, under which the child sank.

No minute *post-mortem* examination of the parts involved in the congenital deficiency was obtained.

TWO HEPATA SUCCENTURIATA IN A FŒTUS. BY PROFESSOR SIMPSON.

Cases of malformation in which there exist small additional spleens, or spleen-lobes, are sometimes met with. Instances of analogous malformations of the liver are much more rare. *Dr Simpson* showed a recent specimen of this last malformation in a fœtus which he saw and dissected along with *Dr McCowan*. The child was born with a large umbilical hernia or abdominal extraversion. The umbilical cord was provided with only one artery.

The mass of the liver was of the ordinary size, but two additional lobules were projected from its anterior edge, and affixed to the inner surface of the abdominal walls. These two additional lobules were flat and button-shaped, being each about the size of a sixpence. They were connected to the anterior border of the liver by prolongations or thin bands of tissue, the thickness of whip-cord, and about an inch long.

The supernumerary lobules seemed, in fact, like two small portions of the liver which had become morbidly adherent during development to the opposite peritoneum, and then become gradually drawn out and pediculated, during the further growth of the parts. The intestines were also in several parts morbidly and firmly adherent to the abdominal peritoneum.

The case was probably an illustration of the origin or commencement of some malformations, in the results or effects of inflammation or inflammatory adhesions in the fœtus.

Dr Simpson showed the uterus of a cow—the inner surface of which was studded over with a *crystalline substance* which he had not previously observed, and the nature of which he had not as yet examined.

THE PELVIC ARTICULATIONS IN PARTURITION, BY DR MATTHEWS DUNCAN.

Dr Duncan first showed to the Society the dissected pelvis of a cow which had died on the day of delivery. This specimen had been exhibited by *Mr Barlow* to the Physiological Society, and he had pointed out the great mobility of the sacrum, the increased thickness and elongation of the sacrosciatic ligaments, the relaxation of the sacroiliac ligaments, and the formation of a large synovial cavity in the sacroiliac joint. *Mr Barlow* had shown that the change in the sacrosciatic ligaments consisted in the development of new longitudinal fibres of large size comparatively, and somewhat like the ribband-like uterine fibres described by *Kölliker* in the anatomy of the human uterus.

He then called the attention of the Society to the researches of the late

Mr Zaglas, in regard to the natural motions existing in man in the sacroiliac joints, whereby the sacrum described a rotatory motion upon the centre of the sacroiliac articulation.

Dr Duncan then stated, that in woman about the period of parturition there had been shown to occur a softening and relaxation of the softer tissues forming the pelvic articulations. Under such circumstances, the motions of the sacrum would be much more easy and extensive than at other times. Cases occasionally occurred where this relaxation proceeded to a morbid extent, but to these he would not refer at present. He stated also that obstetricians had hitherto erred in studying this relaxation chiefly in regard to the symphysis pubis. The most important point was the relaxation of the sacroiliac joints in connection with the rotatory motion of the sacrum, the nodding forwards of the promontory involving diminution of the inlet of the pelvis, and enlargement of the outlet; the nodding backwards, involving enlargement of the inlet, and diminution of the outlet; and even though this were only to a small extent, it was of the greatest importance, as a slight diminution in pelvic diameters was the cause of very serious consequences. This movement was slight at the promontory from its proximity to the centre of motion, and of course greater at the point of the coccyx, because of its greater distance from the centre of motion. This motion was strikingly analogous to that exhibited in the cow's pelvis. It would amount in the human pelvis to at least a line at the promontory, and twice or thrice that amount at the coccyx.

Dr Duncan then showed by a brief analysis of the circumstances of a female in the first and second stages of labour, how they were consentaneous with the best position of the sacrum in these two stages of labour. In the first stage, erect position, or straight recumbent position and absence of bearing down, coincided with the nodding backwards of the sacral promontory; in the second stage, limbs drawn up, body bent forwards, muscular bearing down efforts, coincided with the nodding backwards of the coccyx, and enlargement of the outlet. To this also would contribute the separation of the thighs, and the contraction of the internal femoral muscles by their influence upon the pubic articulation.

Dr Duncan concluded with some reflections upon symphysiotomy,—an operation which, he believed, had been much misconceived. The first great error in regard to it was its proposal as a substitute for Cæsarean section. In pelves requiring this operation, symphysiotomy could be of little or no value. It was in cases of slight contraction that Dr Duncan thought symphysiotomy might some day come to be of service, although now its applicability would be more limited than formerly by the introduction of artificial premature labour. He would also suggest that the operation might now be done by subcutaneous incision, and be by improvements rendered of moderate consideration in itself. Obstetricians had also entertained erroneous notions in regard to the possible amount of separation of the symphysis without laceration of the sacroiliac ligaments. Dr D. had the previous day performed with Dr Struthers an experiment upon a pelvis taken at random in the dissecting room, and found that on cutting through the symphysis, the pubic bones separated without the smallest amount of force to the extent of an inch and a half; and in parturition, the relaxation of the sacroiliac ligaments undoubtedly permitted much more extensive separation without any injury to pelvic structure. He believed also that a certain amount of separation might take place spontaneously in natural labour when the child's head was with much force propelled through the pelvic ring.

Some conversation followed, in which the President Dr Simpson, and Dr Keiller, took part.

Dr Thomson stated the particulars of an interesting case of morbid alteration of the pelvic joints in pregnancy. Other members had seen similar cases.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

December 20th.—Dr SELLER, President, in the Chair.

The President made a few introductory remarks and thanked the Society for the honour they had conferred upon him.

ON THE STATICS OF PREGNANCY. BY DR MATTHEWS DUNCAN, M.D.

Dr Duncan made observations on the three following subjects:—1st, on the position of the uterus; 2d, on the position of the child in utero; and 3d, on the position of the pregnant female. With regard to the first, he pointed out that the uterus was developed into the abdomen in the direction of the axis of the brim of the pelvis, i.e., at about an angle of 30° to the horizon, the brim of the pelvis being inclined to the horizon at an angle of above 60° . From these circumstances he showed that the brim of the pelvis was in no sense the part supporting the uterus. The uterus was supported on every side by the parts surrounding it much in the same way as a body floating and immersed is supported. The anterior abdominal flap being beneath the centre of gravity of the organ, was the part bearing the chief weight of the uterus. These circumstances afforded an explanation of pendulous belly and of the pain produced in the sides by the dragging at the insertion of the oblique muscles of the abdomen. The long axis of the uterus was inclined to the horizon in the erect position of the female at an angle of 30° , in the supine position it was more nearly vertical, being inclined at an angle of about 60° . 2. As to the position of the child *Dr D.* said that the child in utero neither stood on its head nor carried itself in any position, but floated reposing in the liquor amnii in the attitude of stablest equilibrium, viz., in an oblique direction with its head lowest. From experiments which he had made he found the fresh fœtus at the full time to float freely in a solution of salt of about its own specific gravity, in about the same position that it assumed in the pregnant female. Hydrocephalic children with large and heavy heads had been supposed to illustrate, by the frequency of their mal-presentation, the theory that the gravitation of their heads was not the cause of the ordinary position of the fœtus in utero. But their heads though heavier in air were much lighter when floating in liquor amnii from the low specific gravity of the contained fluid. Consequently their mal-presentations could be equally well accounted for on the common laws of physics. *Dr D.* showed that the statistics used by Dubois, and others, to prove the uterine positions of premature fœtuses, afforded no reliable conclusions. They showed only the positions during abortion or miscarriage, but nothing in regard to the position in utero before abortion or miscarriage. The statistics used by the same individuals, and their reasonings in regard to dead fœtuses were utterly illogical, seeing that the altered statical circumstances of such fœtuses were not known. Their frequent mal-presentations were probably the result of their altered circumstances. The uterus at the time when the child was becoming fixed in its position, was an oval cavity, with rounded glabrous walls, filled with a dense fluid, in which the fœtus floated easily, with its legs, its chief organs of locomotion, highest. In these circumstances, it was almost impossible to conceive of its assuming, much less of its maintaining, any position against the influence of gravity. 3. *Dr D.* then pointed out the condition of the erect position in the virgin; showed how they were altered in the pregnant female; and illustrated the new conditions of the erect positions in pregnancy. The centre of gravity might be retained in its usual site above the hip-joints, by the upper part of the trunk being moved backwards to counterbalance the pregnant womb. Or the centre of gravity might be moved forwards; in which case the hip-joints were moved forwards also, by the diminution of the inclination of the brim of the pelvis, as occurred in many cases of disease with anterior curvature of the spine.

Dr Simpson said that when he brought before the Society, some years ago, the subject of the influence of excito-motory action as the kind of vital mechanism, by which the foetus assumed and maintained its position, he felt himself at a loss to explain some of the residuary phenomena, he believed that gravitation might so far explain them, but as yet he was not convinced that the gravitation theory was the only key to the solution of the whole question. We had the third position of Naegele occurring 28 or 29 times in every 100 cases of head presentation and the position thus assumed, viz., the back and occiput of the child lying to the back of the uterus was the very opposite to what we would expect on the gravitation theory. It was not suddenly assumed. We had the evidence of auscultation that this position had been present for weeks before. He did not believe that the death of the child had no influence in determining its position. With living children the proportion of breech presentations was about 1 in 60, but when the child had been dead for some days before birth, the proportion rose to 1 in 6. It was difficult to avoid the conclusion that the position assumed by the child was not a physical act, but one connected with life, it was lost with the loss of life, and was hence a vital act to that extent. *Dr D.* attempted to account for this frequency of breech presentations by stating that the statics of the child had been altered by decomposition which had taken place after death. But the first change observed was an enlargement of the abdomen, which ought rather to keep the head downwards than force the pelvic extremity upwards. In fact, after death the body of the child was subject to statical laws alone, and hence its new position. As to the argument from hydrocephalic foetuses, *Dr D.* forgot that there was a marked increase in the weight of the cranial bones, and yet in such cases the breech was found presenting in the enormously increased proportion of 1 in 6. To his mind, cases where there were twins or a recurrence of mal-presentations in single births with the uterus of a more or less irregular form, virtually decided the question, for were the position of the child solely dependant on gravitation, this irregularity of form should not interfere with the physical action of gravitation, and yet preternatural position frequently recurred in such cases. The subject was one of great extent, and would require much more time than could be given to it at such a meeting.

Dr Storar maintained that if the position of the foetus was purely physical, and the result of gravitation and buoyancy combined as *Dr D.* held, then, when a woman turned round, the child ought always to turn more or less round on its longitudinal axis; and yet it did not make such rotation.

Dr Matthews Duncan, in reply, stated that his researches referred exclusively to the position of the child before labour; and all he wished to show was, that the position of equilibrium of the floating foetus was an oblique one, as in the human female. His investigations did not enable him to say whether in this oblique position, the child should float with back lowest, or one of the sides. Peculiar attitudes of the uterus or foetus, or the position of the liver, might explain the positions to which *Dr S.* had referred. These questions had to be decided, before any argument against their production by gravity could be admitted. He held that we are in entire ignorance as to the altered statical relations of dead children, and therefore, no decision could be come to whether the mal-presentations in the instance cited by *Dr S.*, depended on loss of life, and of power of motion, or on the possibly altered statics. He could furnish one fact, however, which might serve to elucidate this subject. In floating a six-month foetus, born dead, and with the skin separating, in a saline solution of similar density to itself, he had found that the head remained the highest. This foetus in labour had presented the breech. The fact was a striking one, and was worth a cart-load of arguments. As to what *Dr S.* had stated with regard to hydrocephalic foetus, he (*Dr D.*) was of opinion, that until experiments were instituted to ascertain the position of equilibrium of floating hydrocephalic children, we had no right to argue from what might possibly be the result of such experiments. He (*Dr D.*) believed that if the skull was

really found to be increased in weight, it would probably be counterbalanced by the increased density of the other parts of the child's body, the result of atrophy. Dr Simpson had alluded to cases in which mal-presentations were found to occur. Dr Duncan held that no argument could be drawn from such cases, till their whole circumstances had been investigated, and he showed that where the uterus was irregular in form, gravitation afforded a more satisfactory explanation of the position than the motions of the child. The object of his paper was not to discuss the position of the fœtus in utero, but merely its statical relations, which, in his opinion, afforded an easy solution of many questions which Dr S. had referred to reflex actions in the fœtus.

Dr Strachan had listened with great interest to the discussion, but he personally was of opinion that both gravitation and reflex action had to do with the position of the child, and that neither of these agencies could be defended as the sole producing cause.

January 3, 1855.—Dr SELLER, President, in the Chair.

ON A NEW METHOD OF INTRODUCING MEDICINES INTO THE SYSTEM, MORE ESPECIALLY APPLICABLE TO PAINFUL LOCAL NERVOUS AFFECTIONS. BY ALEXANDER WOOD, M.D.

Dr Wood had been led to introduce solutions of morphia and Batley's sedative solution into the cellular tissue, as near as possible to the affected nerve, by means of the small perforating syringe, constructed by Mr Ferguson of Giltspur Street, for injecting aneurisms with perchloride of iron. Dr Wood narrated nine cases in which he had employed this method of treatment, in all with perfect safety, in some with complete, in others with partial, success. As to the *modus operandi* of this method of treatment, he endeavoured to show, from the experiments of Muller and others, that the effect of the local application of opium to a nerve was to destroy its sensibility at the part, and that from this action of the drug the immediate cessation of the pain arose. He then pointed out the rapidity with which absorption appeared to take place from the cellular tissue, which seemed to account for the rapidity of the narcotic effect which a small dose of opium so introduced was found to produce. He also pointed out, that other medicines might be introduced in the same way.

Dr W. T. Gairdner mentioned, that a patient in his wards in the hospital, had been injected the other day in the way recommended by Dr Wood. The result was not decisive, as the complaint for which the man was under treatment, viz., lumbago, had been undergoing rapid amendment, and, indeed, the day after the operation, was nearly gone. The experiment, however, was attended with little suffering, and it was noted that some degree of giddiness was almost immediately produced.

ACCOUNT OF THE CASES TREATED IN THE CHOLERA HOSPITAL, SURGEON SQUARE, DURING THE LATE EPIDEMIC. BY DR BEGBIE, JUN.

The hospital was opened in September 1853; it was temporarily closed about the commencement of June 1854, to be again opened on the 24th of August; it was finally closed a little more than a fortnight ago. During the whole period there were admitted in all 243 patients, of which number only 45 were brought to the hospital during the earlier period, and 198 during the weeks which intervened between the end of August and commencement of December. Of the 198, no less than 168 were admitted during September and October, and 97 in September alone. Placing the numbers together, the experience of the hospital has been roughly as follows:—Total number admitted, 243; of these, males, 97; females, 145; total recoveries, 126; total deaths, 117; deaths among males, 43; deaths among females, 74; recoveries among males, 54; recoveries among females, 72. Among these cases, however, it must be remem-

bered, that not a few are included which were not genuine cholera. Of 68 cases of genuine cholera, in which the condition of the patient on admission was accurately noted, 42 were in a state of profound collapse, and 26 were affected either with the characteristic discharges, or, these having been arrested, with some other combination of symptoms distinctive of the disease. Of the 42, 28 were females, and 14 males; of the 26, 12 were females, and 14 males. Of the 42, 36 died, and only 6 recovered; 26 of the 36 surviving their admission less than twenty-four hours. Of the 26 who had not passed into collapse, 4 died and 22 recovered. Three pregnant women were admitted affected with genuine cholera—all died; two of the three miscarried. Two nursing mothers were admitted; one died, the other recovered. Of 53 cases in which the occurrence or non-occurrence of premonitory symptoms was carefully inquired into and noted, there were 32 in which there was certain evidence of their occurrence, and 21 in which no such evidence existed. In almost all the cases of recovery from severe collapse, the patients became affected with febrile symptoms; in but few cases, however, did these symptoms run so high as to deserve the name of consecutive fever. Of 13 cases in which a severe form of consecutive fever did supervene, 8 died and 5 recovered. Of these, 7 were males and 6 females. Of the 8 deaths, 4 were in complete coma; 27 deaths, 19 of females and 8 of males, took place in profound collapse.

Dr Priestley remarked on the great revulsion that had taken place in the minds of the profession as to the use of diluents, which in the first epidemic were nearly entirely proscribed. *Dr Begbie* had not mentioned whether he had employed them, and to what amount.

Dr Andrew Wood said, that the most striking fact in the paper, to his mind, was the fearful mortality among drunkards affected with cholera, and he should like to know from *Dr Begbie* if any teetotalers had come under his treatment, and with what results? Indulgence in the use of strong drinks, he was convinced, was one of the chief causes of the prevalence and mortality of cholera among our labouring population, and unless total abstinence was more widely practised than it was, he looked forward with dread to the return of the epidemic. As to the use of diluents, having been a sufferer from the disease himself, he could speak with some degree of confidence. The worst symptom to contend against he had found to be thirst, and this could only be relieved (and that most gratefully) by the sucking of ice. This his physician had only allowed in limited quantity, but finding it act so beneficially, he (*Dr Wood*) took it in pounds, and with the best results.

Dr W. Begbie knew that a few patients who had come under his care in the hospital were total abstainers; he could not, however, give the exact number of such. The impression left on his mind was, that excessive indulgence in spirituous drinks *appeared* to act as a predisposing cause to attacks of cholera, while the condition of the system, induced by habitual intemperance, was *manifestly* one which made recovery from the disease more unfrequent than any other. He had allowed diluents in all cases; ice he had generally found most grateful to the patient.

Dr Alexander Wood thought that more had been made of the liability of persons addicted to the use of spirits than was proper. They were certainly not more liable to cholera than to other diseases; and he held, that where intemperance was indulged in, provided it was unattended with physical wretchedness and want, no such marked liability existed.

Dr Simpson could not agree with *Dr Andrew Wood* as to the preventive influence of total abstinence; and begged to remind him, that the nations among which the disease had raged with the greatest virulence were practical abstainers. He referred to the Turks and the Hindoos; and, indeed, the whole Mahomedan races might be included in the same category.

Dr W. T. Gairdner was of opinion that the advocates of the preventive efficacy of total abstinence had overlooked two striking facts—viz., 1, the greater predominance of cholera in females than in males; and 2, the immense num-

ber of children who were attacked. Such evidence he felt it difficult to explain away, on the principles laid down by Dr Andrew Wood. Dr Gairdner alluded to the very ingenious theory propounded by the registrar-general, as to the influence of locality on the disease; Mr Farr asserting that in low localities there was greater prevalence and mortality. Now, Dr Gairdner doubted whether this was owing so much to the relative altitude of the localities as to the poor circumstances of the inhabitants, who usually crowded together on level ground, along the banks of rivers, and the lines of greatest intercourse. In his opinion, Edinburgh, from its peculiar construction, with our huge back-bone of houses running from east to west, with the diverging closes and their rapid declivities, should be the *experimentum crucis* in the question. He had made inquiries among the poor-law officers, as to the relative prevalence of cholera in high and low localities in the town; and, as far as the data went, they did not entirely support Mr Farr's theory. Lastly, Dr Gairdner noticed the presence of bile in the stools, and the good indications that were usually drawn from its reappearance. He had always maintained, as the result of clinical and *post-mortem* observation, that in cholera there was no want of bile in the liver and intestines; and he believed that where, from the appearance of the stools, it had been supposed to be absent, it was really present, though, from its diffusion in the intestinal discharges, the peculiar colour was lost. Any theory of the disease, and any mode of treatment founded on this presumed absence of the bile, must, in his opinion, be erroneous.

SIR GEORGE BALLINGALL ON THE NECESSITY OF IMPROVING THE CONDITION OF ARMY SURGEONS.

SIR GEORGE BALLINGALL has written an excellent letter to the Secretary-of-war, from which we give some abstracts:—

"An extensive reduction of the medical staff, as well as the other departments of the army, is a measure which the public would no doubt most willingly contemplate. The great and praiseworthy anxiety which at present prevails in the public mind for the relief of our wounded soldiers and seamen, and the natural desire that those brave men who have so freely shed their blood in the service of the country, should meet with the most efficient medical treatment, seems to render this an opportune moment for entering upon the whole question; and I would desire to consider it in the following points of view,—the description of young men whom it is desirable to attract to the service—the objections, real or imaginary, which some of the most considerate and eligible students make to it—and the means of obviating these.

"It is certainly not a wise policy to encourage young men to enter the army who do not intend to make a profession of it, but only look to spending a few years in a red coat, in the society of gentlemen, and then retiring into private life, not only without loss of time, but claiming credit for experience in their profession. That views of this nature are occasionally entertained, both by young men themselves and by their parents, I have reason to know, and have taken every opportunity of discouraging them. It is not for such men that I am going to plead. But there are others who have come forward on the present emergency in the most commendable spirit, prepared to devote themselves to the most perilous duties of the service, and ready to take their places in the field and in the trenches. These young men have made themselves the children of the country, and I am well convinced that the public will be well disposed to treat them liberally.

"I come now, sir, to the object which I more particularly proposed to myself in this letter, and which falls more especially within your province as the finance minister of the army—the half-pay and retiring allowance to medical

officers. I have long considered the want of an adequate retiring allowance after a reasonable length of service, and before the infirmities of age have crept on, as not only a hardship on individuals, but what is of more importance, one of the most serious drawbacks on the efficiency of the medical department of Her Majesty's service. This drawback becomes more remarkable when contrasted with the liberal provision upon which the medical officers of the Honourable East India Company are enabled to retire after a service comparatively short, and to this I would briefly advert. In either case, the medical officer must have attained the age of 21 before he can enter the service, and the Company's surgeon may retire after seventeen years' service in India, upon the full pay of a Captain, which, with an allowance from the medical funds at the several presidencies (to which his more liberal pay has enabled him to contribute) makes up a retirement of some L.500 a year, at a period when he has not necessarily attained more than 39 years of age. What, again, is the case of the Queen's surgeon? He cannot retire until after twenty-five years' service, on full-pay—the greater part of this time spent, perhaps, in the most unhealthy parts of India, or even in some worse climates—upon fifteen shillings a day, and when he must necessarily be at least 46 years of age, seven years older than the Company's surgeon. Be it observed also, that the former service is continuous, not necessarily interrupted by anything except bad health, or the will of the individual in taking advantage of an optional furlough, while the time of a surgeon in the Queen's service may be broken in upon by alternate periods of full and of half pay, dependent, not upon the will of the individual, but upon the exigencies of the service. I am aware that the Company's surgeons have their grievances, and complain of something very like an *ex post facto law*, depriving them of the relative retirements, compared with their military comrades which they expected on entering the service; and this leads me to say a word on the comparative position, generally, of military and medical officers, and this in a spirit of equity, most assuredly not in a spirit of detraction.

"The comparative exposure of military and Medical Officers when in actual contact with the enemy, has not, I think, been much dwelt upon of late, particularly since the liberal sentiments towards the latter expressed in the House of Commons, by Sir Howard Douglas, Sir De Lacy Evans, Colonel Boldero, and others, and particularly since the publication of an admirable pamphlet by Mr Martin, on the 'Claims of Medical Officers to Military Honours,'—claims not impaired by the conduct of Mr Wilson, Mr Thomson, and others in the present campaign. But there are two points in which I think their comparative position has scarcely yet been done justice to—the excess of work to which Medical Officers are subjected in both extremes to an army, whether of labour or of rest. If the active operations of a campaign are suspended by sickness, upon whom does the increase of duty fall? Who were the hardest worked men at Devna and at Varna? Again, all honour to their military comrades! But I would ask, who have worked harder or more continuously than the Medical Officers in the Crimea, and in the hospital at Scutari? While I readily admit that the military officer has the greatest risk and the hardest work in the day of battle, I would respectfully ask, who has the hardest work on the day following, and for many days after? The other point in which Medical Officers are sometimes looked upon as having inferior claims, is in comparison with those officers who have paid money for their commissions. Considering the expensive and protracted education which he must necessarily go through, an Assistant-Surgeon may now be said to purchase his commission at a much higher rate than an ensign, and with this material disadvantage, that he necessarily purchases it from six to ten years later in life; the purchase, moreover, as involved in the expense of education, is imperative on all Medical Officers; and while their military comrades are permitted to sell, even in some cases where the commission has not been purchased, the Medical Officer is in no case allowed to do so. Looking again to the comparative rates of retirement, we see provision made for the retirement of military officers on

full-pay, after periods of service not very protracted, while no such thing is known in the medical department as a retirement on full-pay after any length of service.

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"In a period short of ten years service I do not consider that a young man has lost much ground if he desires to enter into private practice, and is not perhaps entitled to any permanent provision, unless in the exceptional cases of severe wounds or permanent disabilities contracted on service. But such a man who has come forward to serve the public with the intention and desire of devoting himself to the service for life; he has gone to an extra expense in his education and outfit; and, if reduced, care should be taken that he does not suffer a pecuniary loss at a time when his prospects of advancement have been cut short, and when he must necessarily be condemned to a period of inaction before he can establish himself in private practice. Would it then be too much to expect that a young man in this position should have the expense of his outfit and extra education repaid to him on a liberal scale, and a gratuity of one or more years' full-pay, according to the length of time he may have served? After ten years' employment in the service, a man, if he has made good use of his time, becomes a valuable servant to the public. He has lost ground in the race with his contemporaries, some of whom have established themselves in the very locality where, of all others, he had the best chance of success; he has necessarily attained 31, it may be 36 years of age, and he has become accustomed to habits of deference to his professional authority and obedience to his prescriptions, not conducive to his advancement in private life,—as witness the few instances of success amongst the medical officers of the army and navy who were discharged at the end of the last war. It is for the public interest that a man of this standing should be encouraged to remain in the service by the prospect of speedy promotion, or, if reduced for the public convenience, he should have the option, if he so chooses, of remaining permanently on the half-pay of his rank. A period being thus fixed for retirement in the junior rank, I would propose that when an officer has gained one step in advance—when he has been promoted to the rank of surgeon—if reduced for the public interest, and again called upon to serve, he should, unless disabled by wounds or infirmities, have the alternative presented to him of either resuming his duties, or reverting to the half-pay of an assistant-surgeon, and so on throughout each superior rank—the principle being this, that whatever length of service, or whatever degree of merit entitles an officer to a step of promotion, the same should entitle him to the *permanent* half-pay of the rank from which he was promoted. This would seem an arrangement more equitable than the commuted allowance—a measure, somewhat of an arbitrary character—where the allowance is calculated on principles, perhaps very intelligible to the actuary of an insurance office, but not, I believe, generally appreciated by the profession. The question as to the light in which the half-pay is to be looked upon—*questio valde vexata*—would thus be put upon a footing equally obvious and indisputable. The half-pay of each superior rank would fall to be looked upon as a retaining fee, the half-pay of the rank immediately below as a reward for past services. The army and navy surgeon would thus see distinctly, from the moment of entering the service, what the public had a right to demand of him, and what the extent of the sacrifice he must make if not prepared to obey the public call. When an officer, recently promoted, if in receipt of the half-pay of a rank in which he has done little service, it seems only reasonable that the public should have the right to call upon him to do more duty in that capacity, provided always that the call should not be postponed until the individual is so far advanced in life as to render it impossible for him to complete the periods of 25 or 30 years' actual service specified for retirement. If the national exigencies do not require a man, who has always been ready to move, to resume his duties at the active and useful period of his life, it would seem harsh, not to say unjust, to call

upon him at an advanced age, when physical infirmities would necessarily preclude him from serving for the length of time entitling him to a permanent retirement. I have confined myself almost exclusively to a consideration of the position of assistant and regimental surgeons, and this with the view of illustrating a principle, which, *mutatis mutandis*, may easily be applied to all other ranks—that of placing the half-pay after prolonged services, so nearly on a par with the full-pay, that there may be a comfortable retirement for men advanced in life. The objects which I have more immediately had in view, are the liberal treatment of those young surgeons who have come forward on the present emergency, and who cannot expect to be retained in the service after the termination of the war—an encouragement to hale young surgeons on half-pay to resume their duties in the service, by the prospect of a considerable sacrifice on the one hand, and of a more liberal retirement on the other—the discouragement of superannuated surgeons from remaining in the service, by giving them a maximum of retirement after a period of life when their energies begin to fail.

“The advantages which I should expect from the proposed plan are the more speedy reduction of what has, not very graciously, been termed a dead-weight—the sequel of every war—and, above all, the greater efficiency of the medical department, by substituting young men in the vigour of life for those who are past their work.”

LATE PROCEEDINGS OF THE EDINBURGH COLLEGE OF PHYSICIANS.

WE should have been satisfied if the few remarks on the late election at the Edinburgh College of Physicians, made in our last number, had been permitted to pass without comment. But the parties concerned have been so foolish as to imitate the usual conduct of sly confederates, who on the exposure of their successful schemes, bluster, talk big, and speak of personalities and insults. Through the anonymous correspondent of a London journal, we are told that our observations have caused great dissatisfaction. The authority, it is true, is not much to be depended on, but if the statement be correct, we are delighted to hear it. The junto must necessarily be dissatisfied on seeing their secret plots laid open to the professional public, and honest men must regret seeing a Royal College, which, perhaps, they have hitherto respected, enslaved by a party of its younger fellows, organized at the supper-table of its Secretary. As to personalities, we have employed none, unless the truthful description of facts be such. For Dr Begbie personally, we have very great regard, and regret that he should have been elected by such equivocal means. He is a most respectable medical practitioner: who has ever said anything to the contrary? But there are other gentlemen, his seniors on the roll of the college, of whom, without saying anything derogatory of Dr Begbie, we can declare, that their respectability is not inferior to his. The question we wish to have answered is this, Why the Edinburgh College of Physicians has found it necessary to elect an elderly Fellow of the College of Surgeons over the heads of so many of those who have supported and performed their duty to the Institution all their professional lives? When the principles or reasons which have led to this act are put forth, we shall be prepared to meet them. Let it be shown that Dr Begbie's talents, discoveries, writings, standing as a physician, or any other qualities, are SUPERIOR to those of all the gentlemen who have been passed over, and we shall be among the first to applaud his election. But, while we acknowledge that gentleman's merits, we maintain that they are not pre-eminent over those of his seniors, and do not justify his election to their prejudice. They are, in fact, the parties to whom gross personality is offered, when a body of practitioners, constituting the majority of a Royal College, publicly declare them unqualified to fill a chair, in order to flatter or benefit a personal friend. It

is at least satisfactory to know that all the physicians of distinction in the college condemned the proceedings of the *junto*, refused to vote with them, and although they supported Dr Begbie as a member of council, they associated with him senior Fellows, all of whom were as well qualified for the office as himself, and whom it was the object of the *junto* to exclude, as they succeeded in doing. Would the College of Physicians of London or Dublin place an old established general practitioner over the heads of eighteen of his senior Fellows, simply because he had lately transformed himself into a consulting physician, and possessed friends who arranged with the junior Fellows (including his own son) so to constitute the council that he might be president? We cannot believe that such respectable bodies would have departed so much from propriety, or have shocked professional feeling by such extraordinary proceedings.

As to how far it is correct or even decent for the secretary of a Royal College of Physicians, constantly to make motions and amendments—as a member of council to put himself on committees and examining boards, and then to organize, at his own supper-table, a party of the juniors in order to carry out his measures?—how it happens that the anonymous correspondent of the *Medical Times* gets possession, and forwards to that journal, private documents of the College, containing its unanimous resolutions?—and by what artful and petty tricks such “unanimous resolutions” are arrived at?—these are questions into which we cannot now enter. We shall, at present, only express the conviction, that such proceedings are most derogatory to any public body; and that if the three members of the reform committee (there are only three, all of whom were active in securing Dr Begbie's election), hope thereby to carry out their measure of medical reform, they will certainly be mistaken. We are unwilling to prolong this disagreeable subject, but if our statements are called in question, we shall be prepared to substantiate them by names and documents, that must force conviction of their truth on the minds of the most sceptical. There is nothing, indeed, we desire more than to appeal from the *junto* to the profession at large, with a view of determining whether we or it have been guilty of what is called “personality.”

WHO OUGHT TO BE MEDICAL ADVISERS IN LEGAL CASES?

It is not our intention to enter into the particulars of the recent trial, “*Glover versus Syme*.” Neither shall we endeavour to answer the nice question raised by the lawyers, whether in the case of a party having a compound fracture and a lacerated wound, it be competent for a police surgeon in reporting on his capability of emitting a declaration, to declare on soul and conscience that he has examined the patient, when he has seen nothing below the splints and bandages. But we feel bound to say that the principle for which Mr Syme contended is perfectly sound; and that it is most injurious to the cause of justice and to the honour of the profession, to permit the practice which has so long been followed in Edinburgh, of excluding those best qualified to give evidence, in order to enhance the emoluments of public functionaries, or favour the friends of sheriffs or procurators-fiscal. Every one must allow that nothing can be more intolerable to a medical practitioner, than to find that a professional brother, armed with a warrant, has without your permission forced his way to your patient, examined his case, reported his opinion thereon, and received a fee from the public purse for doing what you would have done much better. Not unfrequently where two witnesses have been required, the family practitioners of the legal functionaries have been sent, and the medical man in charge of the case entirely overlooked. We are aware that it has been stated, and with some truth, that many legal investigations have failed in consequence of the incompetency of certain medical men to draw up reports with sufficient care and exactitude. We believe that such a statement may not be altogether unfounded; but this would always be obviated by causing such reports to be drawn up by the public medical officer in conjunction with the practitioner in

attendance. We need scarcely say that the charge of ignorance against a surgeon of the Royal Infirmary, who has the care of clinical wards, is preposterous, and hence the peculiar absurdity of sending a stranger into those wards, to obtain the information which ought to have been obtained primarily from the highest authority. In cases of suspected homicide, the law requires a report from two medical men as to the cause of death. For this purpose a post-mortem examination is made, and a fee given to both the practitioners who append their names to the report. That a considerable knowledge of morbid anatomy is here often necessary, we need scarcely say, and yet it is consistent with our knowledge, that the family medical attendants of certain legal functionaries have been sent into the Royal Infirmary with a warrant for this purpose, to the exclusion of the pathologist of the institution. Now, when it is considered that this latter official probably opens and reports on more bodies in one twelvemonth than the said practitioners may have done in their whole lives, the injustice to him and the folly with regard to obtaining correct evidence must be apparent? In every case, therefore, the medical practitioner in attendance, should be one of the parties consulted, and the police surgeon or the pathologist of the Infirmary, the other, according to circumstances. Considering also the peculiar skill which the latter official necessarily possesses on account of his office, it would be only a right thing to make him, as is the case in Vienna and other large continental cities, the consultant in the examination of dead bodies which are to be the subject of judicial investigation.

But after all, this would only be pursuing what appears to us the best course in a public point of view. How can Mr Syme hope to overcome what is obviously wrong when the private interest of individuals or of institutions are bound up in their retention? The evil he has endeavoured, through his college and single-handed, to overthrow, is only the result of that degraded condition which the profession occupies in this country, and how can we expect the state, or the law officers, to respect us, when we do not respect ourselves? The medical colleges which ought to be capable of supporting the dignity and honour of the profession, are in no way regarded by the state or public bodies, because they are themselves governed by petty influences, which fail to command for them the respect of honourable minds. Jealousies, divisions, junto's reign in our councils, and the result is universal misrule of the medical profession, in its relations to the state, to social policy, sanitary arrangements, the public services even in time of war, and the private arrangements of our civil institutions. All is job and clique, from the appointment of an army medical director and president of a college, down to a surgeon's dresser, or the nurse of a ward.

VARIETIES.

THE BOARD OF HEALTH.—The structure of the old board of health was doubtless one cause of its success, and the truly British propensity to job, which burst out with the utmost intensity in the endeavours of the central board to force their inspectors into business, rendered success impossible. We have now a minister of health, Sir Benjamin Hall, and the result of his policy is, to be guided by the majority of the rate-payers in a parish. The richer rate-payers are naturally the friends of cleanliness and health, but this levelling principle places them on an absolute equality with the poorest, most ignorant, and most uncleanly householder in the parish. This may be a consistent democratic principle, but it is, we fear, a fatal blow at the diffusion of sound sanitary influence throughout the country. The interests of cleanliness are handed over to the sons of dirt, and the care of health to men who habitually wallow amid the reeking elements of pestilence. If the Board of Health be a good thing, let its jurisdiction be general; if a bad thing, let it be abolished altogether. But do not let us have this ridiculous compromise between the beadle and the minister; do not let us first invest a public servant with power

and responsibility, and then make him impotent to do good for fear he may be inclined to do evil. In most local matters the Legislature may be content to stand neuter, but in matters of health it does not even pretend to be impartial; it is decidedly for the clean and against the dirty party. In some matters impartiality is a virtue—in this it is a crime.—*Times*.

THE USE OF THE MICROSCOPE IN THE DIAGNOSIS OF CANCER.—The microscope alone—that is, independently of all other kind of observation—can seldom determine in the living subject the presence or absence of cancer. At the same time, the author feels himself bound emphatically to declare, that he thinks it capable of being as serviceable to the surgeon in cases of morbid growth, as the stethoscope is to the physician in cases of diseased heart or lungs. Neither instrument is infallible; both require to be studied in an especial manner; both demand long practical experience, and judicious reasoning power; and both require to be conjoined with all the aids to be derived from other modes of observation. With the stethoscope, it is not that the crepitating rale in pneumonia, or the mucous rale in bronchitis, differ from similar rales which accompany tubercular disease, but that these signs, *conjoined with other symptoms*, clearly establish the diagnosis. So, likewise, it is not the recognition, by means of the microscope, of certain cells and fibres, which will enable us to assert with certainty the existence of cancer; but that their detection in particular places, and accompanying peculiar forms of growth, permits us to do so. In proportion as our knowledge of morbid anatomy advances, instrumental assistance becomes the more valuable for the purposes of diagnosis; and it is now manifest that, to this end, a microscope is as necessary to assist our sense of sight, as is a stethoscope to assist our sense of hearing, or a probe to assist our sense of touch.—*Professor Bennett on Cancerous and Canceroid Growths*.

CHLOROFORM.—The most noticeable new facts of late have been some novel views of Dr Murphy as to the action of chloroform, and the excellent observations of Mr Syme of Edinburgh on the same subject. Mr Syme, who has been treated very harshly by a portion of the medical press (almost a sign in these times there is something good in him), entertains an opinion that we need not watch the pulse during the action of chloroform, but watch the *respiration*. Murphy, from an opposite point of view, comes to the same conclusion; and looks upon anæsthetics (copying an idea of Dr Bence Jones) as active in proportion to their amount of carbon. The Germans, fond of a little bit of mysticism, say if charcoal be ever made soluble, it will be the most powerful poison and anæsthetic in existence. These points are a great relief to the eternal fine writing on the subject by men who know nothing whatever about the thing. The *Lancet* has done infinite good by bringing out these views of Mr Syme, and breaking through old routine and prejudices. If we are ever to improve, and hand on the flickering lamp of our little brilliance in London, it is by hearing what every man has to say, whether Irish or Scotch.—*Dublin Med. Press*.

THE USE OF SPIRITS IS NOT THE CAUSE, BUT AN EFFECT OF POVERTY.—It is an exception from the rule when a well-fed man becomes a spirit-drinker. On the other hand, when the labourer earns by his work less than is required to provide the amount of food which is indispensable in order to restore fully his working power, an unyielding, inexorable law or necessity compels him to have recourse to spirits. He must work, but in consequence of insufficient food, a certain portion of his working power is daily wanting. Spirits, by their action on the nerves, enable him to make up the deficient power *at the expense of his body*, to consume to-day that quantity which ought naturally to have been employed a day later. He draws, so to speak, a bill on his health, which must be always renewed, because, for want of means, he cannot take it up; he consumes his capital instead of his interest; and the result is the inevitable bankruptcy of his body.—*Liebig's Letters on Chemistry*.

EXTRACT FROM AN OFFICIAL LETTER.—January 5, 1855.—Sir,—“I am di-

rected by the commander in chief to acknowledge the receipt of your letter of the 10th of April last, which has been referred, in the regular course, to the field train department of the Ordnance-office, and by them to the medical department of the Ordnance and of the War-office, and to the Commissariats department. I have now to request that you will, in the first instance, address your proposition to her Majesty's Secretary of State for the War department, in order that he may move their lordships, who will consider the expediency of placing at the disposal of the proper authority the funds necessary to enable that authority to act upon your suggestion, should that authority, on mature consideration, be of opinion that the interest of the service will be advanced by so doing."—*Punch*.

SURGEONS FOR THE ARMY.—Several medical students are allowed to present themselves for examination at the Edinburgh College of Surgeons, who intend to join the army in the Crimea, although they have only completed two months of the last and most important years of their medical studies. We have already noticed that a similar relaxation of the minimum curriculum has been allowed by the London College. Whether this step is a beneficial or hurtful one, has yet to be determined. But as every account shows, that what is required, is more medical than surgical knowledge, and as this is exactly what will be most deficient in the education of such candidates, how the poor soldiers individually, and the army generally, are to be advantaged, we are at a loss to conceive.

STATISTICS OF SYPHILIS AND PROSTITUTION.—Dr Holland, of Cork, is occupied in obtaining from public Hospitals, Infirmarys, Medical Societies, sanitary officers, etc., a return of the statistics and status of syphilis and prostitution in Great Britain and Ireland. We sincerely trust that his self-imposed task may lead to an improvement in the sanitary condition of the people, and hope our readers will co-operate with him in his arduous and useful labours, by returning—carefully filled up—any schedules they may have received from him.

PUBLICATIONS RECEIVED.

On the mode of communication of Cholera. By John Snow, M.D., etc. Second Edition. London. 8vo. 1855. Pp. 162.

A Manual of Elementary Chemistry; being a Practical Class-Book. By Robert Mortimer Glover, M.D., F.R.S.E., etc. Illustrated. London. Small 8vo. 1855. Pp. 321.

The Retrospect of Medicine; being a Half-Yearly Journal, etc. Edited by W. Braithwaite. Vol. xxx. London. Small 8vo. 1855. Pp. 448.

Chloroform; its Properties and Safety in Childbirth. By Edward William Murphy, A.M., M.D., etc. Folscap 8vo. 1855. Pp. 72.

Transactions of the Belfast Clinical and Pathological Society, for the Session 1853-54. Belfast. Small 8vo. 1854. Pp. 132.

Letter to the Right Honourable the Secretary-at-War on the Medical Department of the Army. By Sir George Ballingall. 8vo. Pp. 16.

On the Chemical Equivalents of certain bodies, and on the Relations between Oxygen and Azote. By Professor Low. As read to the Royal Society of Edinburgh, Dec. 4, 1854. Royal 8vo. Pp. 20.

The Micrographic Dictionary. Part vi. Case of Mixed Aneurism of Aortic Arch, simulating Aneurism of left Carotid, with Remarks. By James G. Wilson, M.D., etc., Glasgow. 8vo. 1855. Pp. 15.

Remarks on the Examining Medical Board for Indian Appointments, etc. From James Bird, M.D., F.R.C.S., etc. London. 1855. 8vo. Pp. 15.

We shall give the list of exchanges in our next, which we request our Continental friends to consider carefully. This list is so lengthy, whilst the irregularities of many arrivals are so great, that we shall be compelled to make a great reduction.

The papers of Drs Markham, Peacock, Reeves, and others, in our next.

We have mislaid the letter of *Medicus*, but will answer his queries in our next.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Properties of the Ordeal-Bean of Old Calabar, Western Africa.* By ROBERT CHRISTISON, M.D., V.P.R.S.E., Professor of Materia Medica in the University of Edinburgh, etc.

(Read before the Royal Society of Edinburgh, Feb. 5, 1855.)

VARIOUS travellers, and other authors on the manners and customs of the negro tribes of Western Africa, make mention of the ordeal by poison as a mode of trial in that part of the African continent, when a suspicion arises of the commission of one of the more heinous offences. Of these none seems more frequent than the offence of effecting death or other injuries by means of witchcraft;—a crime so held in abhorrence, that the accused will often himself demand the ordeal, rather than lie under suspicion. We have no right, however, to express any astonishment at this folly of the benighted pagan African, when we reflect how short a time has gone by since witchcraft was generally believed in throughout civilised Christian Europe; and when the only way of meeting a charge, no less easy to make than difficult to repel, was by undergoing an ordeal of some kind quite as preposterous as that by swallowing a deadly poison.

The ordeal-poison of the native tribes on the River Gambia appears to be the bark of a leguminous tree, which has been described and figured under the name of *Fillæa suaveolens* by MM. Guillemin and Perottet, in their *Flore de Sénégambie* (1830–33, p. 242, tab. 55). This tree is considered by Dr Hooker and Mr Bentham in their *Flora Nigritiana* (1849, p. 424) to be synonymous with the *Erythrophleum Guineëns* of Mr George Don (*Gard. Dict.*, ii. 424), the bark of which yields, by infusion, the Red-water, or ordeal-poison of our negro colonists around Sierra Leone. And this again has been thought identical, or nearly so, by Mr Brown, with the *Casa*, or *Cassa*, whose bark was found by Captain Tuckey to be used as an ordeal-poison by the natives on the banks of the Congo River (*Tuckey's Narrative*, Appendix v., p. 467). It is possible that more than one species of the same genus may be used for the same purpose; but, judging from the bark of the Gambia and Sierra Leone

plants in my possession, it is improbable that *Fillæa suaveolens* and *Erythrophleum Guineëse* are the same species.

I have not yet met with a good account of the effects of the red-water tree, or cassa tree; and they certainly have not been hitherto examined scientifically, although the subject cannot fail to repay inquiry. The red-water bark would seem, from the statements of Dr Winterbottom in his "Account of the Native Africans in the neighbourhood of Sierra Leone" (1803, p. 130), to possess the property of causing, in various circumstances, vomiting, purging, paralysis of the limbs, and death. Judging from the quantity which he says is required for the ordeal, it cannot be a very subtle poison. But the bark presented to me by Dr W. F. Daniell, of the Army Medical Service, as the bark of *Fillæa suaveolens* must be energetic; for when a grain or two is tasted, it causes slowly an intense numbness and tingling of the part of the tongue to which it is confined. That which I have received from him as the bark of *Erythrophleum Guineëse* has on the contrary a purely astringent taste, without bitterness or subsequent numbness or acrimony. Its texture is also full of a red concrete resiniform matter, probably a kind of kino; which is entirely wanting in the other. Hence, these barks cannot be produced by the same species; so that if there be no mistake about the barks, the two plants must be different species.

According to Dr Winterbottom, when a culprit is to undergo the red-water ordeal, proclamation is made, and the whole proceedings take place in public before a great concourse of people, among whom the women are conspicuous by their number and their finery. The bark is publicly pounded, and half a pint of powder is switched in water till it froths like soap. After certain ceremonial observances, the culprit drinks repeatedly, and as quickly as possible, a calabashful of the poison, amounting to eight ounces. Sometimes he dies after drinking the fourth calabashful; sometimes he will take twelve before any effect results. If he is seized with violent pains in the bowels, without vomiting or purging, he is declared guilty, and if he recovers he is sold for a slave. Should he vomit and sustain no other injury, he is pronounced innocent. But if the poison cause purging within twenty-four hours, or if he lose the use of his arms and legs, and so cannot run away when liberated, the red-water is said "to be spoiled;" and in that case, too, he is sold into slavery, or a relative for him, if he be himself too old.

I should have gladly examined a poison possessing such properties as these. But, unluckily, the quantity with which I have been hitherto supplied is too small for an adequate experimental inquiry.

Meanwhile I have fallen in with another African ordeal-poison, of much greater energy and interest, and not hitherto mentioned by any author on poisons I have consulted, although prevalently used in a district long accessible to Europeans. The only notice of any kind that I have seen of it, is a short allusion to it by Dr Daniell, in an ethnological paper in the Edinburgh New Philosophical Jour-

nal for 1846, p. 319. From such trials as I have made, it seems one of the most singular and intense poisons yet known, and well worthy of a more complete investigation than I have been hitherto able to accomplish.

A few years ago the Rev. Mr Waddell, missionary in Old Calabar, who lately left Scotland for the third time, to return to Africa in pursuit of his Christian calling, put into my hands two seeds, which he described as the ordeal-nut of the Negro tribes of Calabar, and of whose properties and uses he gave me so singular an account, that I felt great curiosity to investigate its action and chemical constitution. Subsequently, from him and from a mercantile friend in Liverpool, who annually sends a trading vessel to the Gold Coast, and who kindly interested its captain and its surgeon in the cause, I obtained successively three small parcels of the seeds. The natives, it seems, regard them with mystery, and reluctantly part with them. It is, therefore, necessary to guard against the chance of deception, and other sources of error. But I have no doubt that I have received the genuine article. For the four several specimens are the same seed; and it eminently possesses the only indispensable property for the trial by ordeal, inasmuch as it is an unerring and terrible poison.

I owe also to Mr Waddell a collection of documents illustrating the trial by the ordeal poison as repeatedly witnessed by himself and his brother missionaries. From these documents it appears, that when a man dies a little out of the ordinary course, it is no uncommon thing to ascribe his death either to poison or to *Ifod*, in negro English, *free-mason*, that is witchcraft. Thereupon one, or more, or many, of his relatives come under suspicion; and there is no other way for the poor creatures to clear themselves, than by swallowing, generally in the fetish-house, an emulsion of this dreadful seed. The native belief is, that the innocent vomit and are safe; but that the guilty retain the poison and die. And such is the confidence in the test, and the general detestation of the crime of practising witchcraft, that, when an individual is accused, he commonly demands the ordeal, and is with difficulty denied it. Many an innocent person thus pays the penalty of his rash reliance.

On one occasion, a young woman having been accused by the king's niece of "having freemason," she at once demanded of the king that she should "chop nut," that is, eat the ordeal seed. Eyo the king refused, however. But she went to an adjoining house, took the poison there, and died in three hours. On another occasion, when a native of rank died rather suddenly, his brother, a lad of eighteen, was accused of killing him by witchcraft. The lad indignantly demanded the ordeal, and swallowed an infusion of thirty seeds. Dr Taylor, a missionary, who rushed forward to save him, was at first repulsed by those around, but, reaching him at last, found him cold, flaccid, insensible, and unable to swallow the emetic which was offered him; so that he died in half-an-hour after

taking the deadly potion. A dose of this magnitude indeed, seems usually to prove fatal within an hour. A man, who was seen to pass the mission-house to the habitation of the priests, where he was compelled to take nineteen pounded seeds in a draught, was carried back, a stiffened corpse, in an hour afterwards. The only chance of life is an early attack of vomiting. In a courageous attempt by Mr Waddell to persuade king Eyo to abandon the trial of several people who were charged with slaying a man of family by witchcraft, a woman who had taken ten seeds was seized with vomiting while Mr Waddell was engaged in an altercation with the king; upon which she was immediately claimed by the missionary as innocent by their law, and was saved. Sometimes the horrible trial is undergone on the great scale, when a chief of rank is thought to have died suspiciously. In 1834, when a noted chief man called Duke Ephraim died, all his relatives and slaves, to the number of fifty, were condemned to undergo the ordeal, and no fewer than forty of them died. Among the documents put into my hands by Mr Waddell was a journal of this transaction, kept at the mission by a native convert in his broken English, of which the following extract is a graphic specimen :—

"Old Calabar, October 14, 1834.—Ephraim Duk died in five o'clock this evening, and put him for grown next morning.

"16 October 1834. This morning all country and Calabar come, and we go for Mr Young, and stop little, not long, after that we go for Duk Palaver House, with all country and our people, about the Duk Ephraim sick, and we go in for his yard; so all our people chop nut. The name of them: Erim Cooffee Duk chop, dead. His son chop, no dead. Orrock Cooffee, and two his son, dead. Cooffee Copper, dead. Egbo Esham, dead. Egbo Young Egbo, dead. Bashie Archiebong Egbo Duk, dead. Erim Odoor, mother dead. Erim Egbo Duk Ephraim Otto, dead. Young Old Archiebong, dead. Otto Ercanam, dead. One Otto slave, dead, for street. Egbo Eshen, mother, dead to night.

"Ditto 17.—5 Duk wife chop nut this morning. All dead, etc."

Here other names follow; and so the entries go on day by day until forty of these cold-blooded murders are recorded.

I have received a few days ago some additional particulars respecting the uses of this poison from Dr Daniell, who resided for some time in Calabar, and, by his influence with Eyamba, the king of the country, was allowed to witness scenes which are usually forbidden to Europeans. This gentleman confirms the testimony of the missionaries as to the deadly nature of the poison, and says it is used not only as an ordeal-poison, but likewise often for despatching the numerous wives and slaves who are buried on the occasion of the funeral of men of consequence.

The ordeal-nut of Calabar, called Esére by the natives, is a leguminous seed or bean, about the size of our garden-bean, but thicker. According to one account I have received, it is not produced in the Calabar district, but is floated down the river from the upper coun-

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This is possible, for it floats in water; but it is not very likely. According to information communicated to me by Dr Daniell, it is stated to him by the natives to grow in marshy places near Starpah and Old-town in Calabar; and the Rev. Mr Waddell was informed that the plant is everywhere destroyed by order of the king, except where it is preserved for supplying the wants of justice; and that the only store of seeds is in the king's custody.

The seed is, I apprehend, quite unknown in Europe. Of several eminent botanists, including Mr R. Brown, to whom I have shown it, none has been able to recognise it as a known species. In order to describe it, it has been cultivated at my request by my colleagues, Professor Syme and Dr Balfour, and both have succeeded. It proves to be a perennial creeper, of the natural family, *Leguminosæ*, and closely resembling a *Dolichos*. It has a large root-stock. The fresh plant has a heavy, strong smell, after being some time cut. Though two years old, it has not yet flowered; and, like other perennial creepers, it may require to form wood for several years longer before it bears flowers. I am therefore unable to describe it farther, or to name it.

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It has a hard, brittle, ligneous tegument, rather rough, and of a brownish-crimson or pale chocolate-brown colour; but many specimens are ash-grey, apparently from slight mould. The kernels, which weigh from 36 to 50 grains, are always in good preservation, and never injured in the slightest degree by insects—a rare occurrence with tropical seeds. They are white and hard, but may be chewed; and they have the taste of the eatable leguminous seeds, without bitterness, acrimony, aroma, or any other impression on the organs of taste; in fact, they are scarcely, if at all, distinguishable in taste from a haricot-bean. This is a formidable peculiarity, were it possible for the seed to become a familiar poison in Europe. So far as I know, the property in question is peculiar to it, for all other poisonous seeds of the *Leguminosæ*, with which we are sufficiently acquainted, are bitter. The blandness of its taste is indeed so unusual a character that I was at first misled, and imagined that I had probably got a wrong and harmless seed; but I soon found that I was much mistaken.

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I began a chemical examination of it, with the hope of separating an active proximate principle, which assuredly must exist in it, and will prove to be a poison of appalling subtilty. But with my limited materials success was unattainable; for leguminous seeds are difficult to analyse; and in this instance there is the additional obstruction, that at every stage the want of any marked sensible property makes it necessary to perform a physiological experiment on one of the lower animals, otherwise we may follow a wrong direction in the search. All I can say is, that the seed, like others of its natural order, contains much inert starch and legumin, and 1·3 per-cent of fixed oil, also probably inert; that its active properties may be concentrated in an alcoholic extract, which constitutes 2·7

per-cent of the seed ; and that this extract does not yield a vegetable alkaloid by the more simple of the ordinary methods of analysis.

I shall now proceed to mention what I have observed of the effects of the ordeal-bean on the animal body. These are interesting, energetic, and in some respects peculiar, as it seems to affect directly and violently the functions of the heart, and the exercise of volition over the muscles.

When a poison impresses powerfully both the circulation and some function or functions of the nervous system, it is a matter of great nicety to eliminate the true phenomena, especially by observation, upon the lower animals alone.

We know that some poisons, such as strychnia, and the various seeds and barks which contain it, cause, by direct irritation of the spinal chord, violent tetanic spasms of the voluntary and respiratory muscles, without impairing sensation, or enfeebling the heart, or clouding the mental faculties ; and thus they occasion death by convulsive arrestment of respiration. Others, such as the urari poison, and conia, or hemlock from which it is derived, cause, by direct exhaustive action of the spinal chord, the opposite state of paralysis of the voluntary and respiratory muscles, but still without influencing the heart, or sensation, or the mental powers ; and so death arises in their instance from arrestment of respiration, by simple paralysis of the muscles which maintain it. Others again, such as atropia, or belladonna, the plant which yields it, principally assail the functions of the brain, at first combining stimulus of some with exhaustion of others in the most singular and often grotesque concatenation, but inducing at last a state of profound coma, and as the result of this a universal muscular paralysis ; and thus death ensues, equally as before, from arrestment of the breathing, not however by direct action on the origin of the nerves which govern the muscles of respiration, but indirectly, through an influence on the cerebral functions, exactly as in ordinary apoplexy. We can likewise conceive a poison to possess only a simple and direct action upon the heart, producing exhaustion of its irritability, paralysis, and consequently death, by arrestment of the circulation ; but no such poison is yet known.

These are all instances of simple action on a single vital function. But many poisons exert a more composite action. Some, such as nicotina, and its source, tobacco, produce paralysis of the heart, and also a narcotic action on the brain. Others, such as fox-glove, and in all probability its active proximate principle, digitaline, not only possess this double action on the heart and brain, but likewise powerfully irritate the kidneys. Others, such as hydrocyanic acid and picrotoxa, the active constituent of cocculus-indicus, exhaust the functions of the brain, so as to induce coma, and at the same time irritate the spinal chord, so as to excite convulsions ; and thus, here again we have death produced by arrestment of the breathing, indirectly through the brain, but concurrently with direct spinal

irritation. In others, such as aconitina, and its source, monkshood, there is a singular combination of exhaustion of the heart's irritability, and of common sensation, but without any influence on the voluntary muscles, or on the mental faculties; and death arises by arrestment of the circulation.

It is easy to see,—on considering attentively what must be the manifestations of these various actions, both simple and compound, but especially the latter,—that extreme difficulty will often occur in seizing and rightly comprehending the facts, above all when the succession of phenomena is swift, and when the subject of observation is one of the lower animals, which cannot adequately express by external signs the varying influence of agents on sensation and the other cerebral functions.

Hence it arises that many erroneous conclusions have been come to regarding the action of our most potent and interesting poisons. Take for example hemlock. This formidable poison was long supposed to cause death by coma, that is, a narcotic action on the brain. But I have shown in a paper read before this Society in 1836, that the mode of death is really by paralysis of the muscles and arrestment of respiration, through an exhaustive influence on the spinal chord. And it is easy to see where the source of error lay. For, when the muscles are paralysed, sensation and the mental faculties will seem to a common observer to be paralysed also; because the animal mechanism for producing expression is at rest.—It appears that many persons think it an easy task to investigate experimentally the physiology of poisoning. But they are assuredly mistaken. A long apprenticeship must be passed before any one can observe with accuracy the phenomena of the action of poisons.

These cautions are prefatory to the remark, that it is a matter of great nicety to apprehend the deceptively simple manifestations of the action of the ordeal-bean on the lower animals. Scarcely do signs of uneasiness appear after a fatal dose has been given, when the animal becomes in quick succession languid, prostrate, flaccid, immovable; respiration, now faint, speedily ceases; and death is complete. It may thus appear to die insensible and comatose. But that is not the case. So long as the power of expression remains, amidst the swiftly advancing languor, signs of sensation may be elicited. Or we might infer from the phenomena that it dies of paralysis of the voluntary and respiratory muscles. But this too is in all probability not the fact. For, on dissection immediately after respiration ceases, the heart is found in a state of paralysis; and it is evident that a quickly increasing paralysis of the heart not only explains the mode of death, but might likewise account for the antecedent muscular weakness and flaccidity.

These effects were well exemplified in the first experiment I tried, when twenty-one grains of fine powder, made into an emulsion with two drachms of water, were secured in a cavity in the subcutaneous cellular tissue of the flank of a rabbit. For three minutes there was

no appreciable change. But the animal then evidently became weaker, especially in the hind legs. Its feebleness quickly increased, and was attended with slight irregular twitches of the muscles of the trunk and extremities, and occasional twitching of the head backwards. But sensation remained; for the animal struggled a little when held up by the ears, and resisted attempts to shove it from behind. In four minutes, when put upon the side, it lay in that position; which the rabbit always vehemently resists so long as it is able. The trunk and extremities immediately afterwards became quite flaccid. Respiration ceased in five minutes certainly; probably indeed sooner; but the precise time could not be fixed, owing to continuance of slight muscular twitches. The chest being immediately opened, the heart was seen pulsating slowly, feebly, and inefficiently for ten minutes; and when its cavities were then perforated, the left side gave out a much brighter blood than the right, showing that the circulation, owing to paralysis of the heart, had not been maintained after respiration had ceased. The muscles of voluntary motion contracted at this time vigorously under the stimulus of galvanism, and continued to do so twenty-five minutes after death.

The same remarkable properties are possessed by the alcoholic extract of the seeds. When two grains and a third of this extract, obtained from one hundred grains of powdered seeds, were introduced into the cellular tissue of a rabbit in the same way as before, at the end of two minutes, without any previous indication, the animal suddenly became weak, fell on its side, struggled a little with its feet, and ceased to breathe in one minute more. On the chest being immediately laid open, the same phenomena were observed as in the last experiment.

It is evident that this poison is one of great intensity of action upon the lower animals; but I have not endeavoured to ascertain exactly its degree of energy. I may mention, however, that on making trial of the exhausted powder from which the extract used in the preceding experiment was prepared, although no effect could be detected in the course of an hour, in ninety minutes the animal was observed to become suddenly weak, and it died in a few minutes more exactly like the others. This result, which appeared unintelligible at first, was afterwards satisfactorily traced to the residual farina not having been carefully enough washed clear of the second spirituous decoction; so that a little of the poisonous ingredient was inadvertently allowed to remain before the farina was dried. The quantity must have been very small.

The only other fact I have to mention relative to the action of the seed on the lower animals, is one observed incidentally by Mr Macnab. As the seed vegetates, the two fleshy cotyledons or sarcolobes rise partially above ground. In this state one of the seeds growing in the Botanic Garden stove-house was attacked by two slugs, one on each cotyledon. Mr Macnab observing that one

of them had begun to swell about the head, he removed it for further observation; and in twenty-four hours it was found dead.

Having ascertained the mode of death from the action of the ordeal-bean, I did not consider it advisable to study farther the details of its action by means of experiments on animals, because I had been fully informed as to this in a more precise manner by an experiment made with the bean in my own person. I shall conclude this notice with an account of what I experienced; and I trust the details will not appear needlessly minute, as they seem to me to establish an action of a very singular kind in the case of this poison, and one of which we might discover other instances among known poisons, had we equally precise opportunities of determining the true phenomena.

Having some doubts whether I had obtained the true ordeal-poison, as it tasted so like an eatable leguminous seed, I ate one evening the eighth part of a seed, or six grains, about an hour after a very scanty supper. During an hour that I passed in bed reading, I could observe no effect whatever, and next morning I could still observe none. I am now satisfied, however, that a certain pleasant feeling of slight numbness in the limbs, like that which precedes the sleep caused by opium or morphia, and which I remarked when awake for a minute twice or thrice during the night, must have been owing to the poison.

On getting up in the morning I carefully chewed and swallowed twice as much, viz., the fourth of a seed, which originally weighed forty-eight grains. A slight giddiness, which occurred in fifteen minutes, was ascribed to the force of the imagination; and I proceeded to take a warm shower bath; which process, with the subsequent scrubbing, might take up five or six minutes more. The giddiness was then very decided, and was attended with the peculiar indescribable torpidity over the whole frame which attends the action of opium and Indian hemp in medicinal doses. Being now quite satisfied that I had got hold of a very energetic poison, I took immediate means for getting quit of it, by swallowing the shaving water I had just been using, by which the stomach was effectually emptied. Nevertheless I presently became so giddy, weak, and faint, that I was glad to lie down supine in bed. The faintness continuing great, but without any uneasy feeling, I rung for my son, told him distinctly my state, the cause, and my remedy—that I had no feeling of alarm, but that for his satisfaction he had better send for a medical friend. Dr Simpson, who was the nearest, reached me in a few minutes, within forty minutes after I ate the seed, and found me very prostrate and pale, the heart and pulse extremely feeble and tumultuously irregular; my condition altogether very like that induced by profuse flooding after delivery; but my mental faculties quite entire, and my only sensation that of extreme faintness, not, however, unpleasant. Dr Simpson judged

it right to proceed at once for Dr Douglas MacLagan as a toxicological authority, and returned with him in a very few minutes.

In his absence, feeling sick, I tried to raise myself on my elbow to vomit, but failed. I made a second more vigorous effort, but scarcely moved. At once it struck me—"This is not debility, but volition is inoperative." In a third effort I was more nearly successful; and in the fourth, a resolute exercise of the will, I did succeed. But I could not vomit. The abdominal muscles acted too feebly; nor were they much aided by a voluntary effort to make them act. I then gave up the attempt, and fell back, comforting myself with the reflection that vomiting was unnecessary, as the stomach had been thoroughly cleared. At the same time the sickness ceased, and it never returned. There were now slight twitches across the pectoral muscles. I also felt a sluggishness of articulation, and, to avoid any show of this, made a strong effort of the will to speak slowly and firmly, through fear of alarming my son, who was alone with me.

Dr MacLagan, on his arrival, thought my state very like the effects of an over dose of aconite. Like Dr Simpson, he found the pulse and action of the heart very feeble, frequent, and most irregular, the countenance very pale, the prostration great, the mental faculties unimpaired, unless perhaps it might be that I felt no alarm where my friends saw some reason for it. I had, in fact, no uneasy feeling of any kind, no pain, no numbness, no prickling, not even any sense of suffering from the great faintness of the heart's action; and as for alarm, though conscious I had got more than I had counted on, I could also calculate, that, if six grains had no effect, twelve could not be deadly, when the stomach had been so well cleared out.

Presently my limbs became chill, with a vague feeling of discomfort. But warmth to the feet relieved this, and a sinapism over the whole abdomen was peculiarly grateful when it began to act. Soon afterwards the pulse improved in volume, but not in regularity. I was now able to turn in bed; and happening to get upon the left side, my attention was, for the first time, directed to the extremely tumultuous action of the heart, which compelled me to turn again on the back, to escape the strange sensation. Two hours after the poison was swallowed, I became drowsy, and slept for two hours more; but the mind was so active all the while, that I was not conscious of having been asleep. On awaking, the tumultuous action of the heart continued. In an hour more, however, I took a cup of strong coffee; after which I speedily felt an undefinable change within me, and on examining the condition of the heart, I found it had become perfectly and permanently regular.

For the rest of the forenoon I felt too weak to care to leave my bed; and on getting up, after a tolerable dinner, I was so giddy as to be glad to betake myself to the sofa for the evening. Next morning, after a sound sleep, I was quite well.

On considering this narrative, as well as the experiments on the rabbit, it will appear evident that one principal action of this extraordinary poison, and the immediate cause of death in fatal cases, is depression, ending in paralysis, of the heart. I think it may be also safely inferred, that another action is paralysis of the voluntary muscles, attended with suspension of the influence of volition. It does not appear to me that mere faintness is adequate to account for the extreme muscular inability I experienced; neither do I conceive it possible for me to have been deceived by the strong conviction I felt of the will being inoperative in its influence over muscular motion. My failure reminded me forcibly at the moment of a phenomenon invariably remarked during the impaired acuteness of the mind which often attends the early stage of hemiplegia. When the patient is told to stretch out the palsied arm, he stretches out the other, however pointedly the physician turns his attention on the powerless limb, and even though the patient himself keeps his eye on it; thus clearly showing that the will orders, though the muscles cannot obey.

The integrity of the mental faculties, during the prostration of that cerebral function which conduces to the operation of the will or muscular action, was most remarkable. The minute details I have given are chiefly intended to illustrate this point; and I am persuaded that I have not overstrained any one article of evidence on that head.

The apparent efficiency of coffee, in removing what remained of the poisonous action after five hours' duration, is not unworthy of notice. Every physician knows that coffee is used for dispelling the after effects of various narcotic poisons; but its real utility has been doubted. In the instance of the present poison, the *post hoc* at least was both very prompt and most complete, so far as the main symptom, the irregular heart, was concerned; and I have myself no doubt of the reality of the curative action.

Whether the extraordinary power, which this poison possesses in depressing the action of the heart, may be susceptible of application in the exercise of the healing art, is a question which time and experiment will alone enable us to answer. Its mere potency is no objection, when it is considered that drugs so potent in poisonous energy as hydrocyanic acid, aconite, and digitaline, are now firmly established in medical practice as safe and efficacious remedies.

Let me advert lastly to a peculiarity in the action of the ordeal-bean which struck me forcibly while labouring under it. Philosophers have thought it not unworthy of inquiry, how in criminal executions death may be completed without physical suffering to the criminal. Governments have even consulted science on the subject. But science has not yet satisfactorily solved the question. Meanwhile, I suspect it has been accidentally solved by the negroes of Old Calabar. At least, so far as the effects of their state-poison on myself went, there was no bodily uneasiness except the single attack

of sickness—apparently the relics of the action of my peculiar emetic,—but simply a sense of sinking vitality, with clearness of mind, and without any sensation deserving in the slightest degree to be called physical distress. We know, indeed, that many forms of extreme fainting, of which this is evidently one, are attended with feelings, which, if not positively pleasurable, are certainly quite unallied to pain. Death by simple fainting, without any preparatory painful process, is evidently what a humane execution should aim at producing. And all this, I apprehend, will be effected by the Calabar Ordeal-bean.

ARTICLE II.—*Remarks on the Treatment of Disease.* By W. O. MARKHAM, M.D., Assistant Physician to St Mary's Hospital, London.—(Continued from p. 33.)

"THE nature of medical causation is such, that it takes as much time and trouble to rectify an error as to establish a truth. Thus it may require the experience of one man's life to arrive at some plausible theory, and the counter-experience of another man's life to show that it is false."—*Latham's Clinical Medicine.*

THE influence exercised over the human intellect by Bacon's master spirit is, amongst other illustrations, supposed to have been manifested, and in a striking manner, by advancements made in the art of medicine,—in a more rational application of it to the cure of disease; and at this present day, I imagine, that no body of scientific men makes greater claim for the credit of pursuing the inductive method, in their investigations, than do the practitioners of the medical art; and perhaps no body of scientific men possesses more just claims for such credit. The reason of this is plain enough. The direction in which the mind of the physician is turned by the physical and mathematical branches of study, whose cultivation is absolutely necessary, as stepping-stones to a knowledge of his profession, naturally gives to his ideas a positive and somewhat a material character; he demands to feel, to see, and to touch; and by the exercise of these faculties of the senses it is that he becomes, for the most part at least, possessed of a knowledge of his business.

He is thus, by the very nature of his previous education, by his study of chemistry, of natural philosophy, and of other kindred branches of the positive sciences, taught early to draw correct inferences respecting causes and their effects, and to trace out the proper connections in which sequences stand to antecedents; he is thereby continually warned against a fallacious acceptance of conclusions; he therein finds a safeguard against the great and very numerous temptations to the ready reception of fallacies besetting him in his investigation of those matters, which are the special objects of his professional pursuits. And such a safeguard he indeed requires, for these pursuits are constantly leading him into unknown regions of

knowledge; his inquiries have perhaps scarcely commenced ere he reaches a barrier which bars all further progress; the question he is investigating remains unsolved; the problem unsatisfied; his scientific aids and reasoning powers can help him no further; sane reason tells him he can proceed no further; here is the limit to his knowledge, fixed until such time as new lights shall shine upon and dispel the darkness which now obstructs his vision. And it is just in these very regions of unknown facts; of the nature of the connection of the physical and the vital forces, for example; of the intimate action of various matters upon the different organs and parts of the body; of the essences of disease; in a word, it is in that immense region of the unknown, which on every side at once impedes the physician's steps, and stimulates his energies for research, that the syren Imagination is ever on the alert, to delude him into brilliant paths of beautiful errors. When once the feet are engaged in those enticing paths, how difficult is a retrograde movement back into the quiet, unobtrusive walks of quiet reason! Even John Hunter had his day-dreams in the regions of fancy.

Well, the end and aim of the physician's strivings is to treat disease, to anticipate its attacks, or divert their force; towards this object all his various learning is concentrated—to this all his studies are subservient; and now, let me ask, does he in this, the prime act of his, still take for his constant guide that inductive method, that cautious analysis of events, which adjusts their relations as antecedents and as sequences, as causes and as effects, and which we have supposed hitherto to have been his guide, while he was acquiring a knowledge of all those lesser lights that were to lead him to this great purpose—the cure of disease?

I endeavoured on a former occasion to answer this question in general terms, and was compelled, in a certain sense, to answer it in the negative. I then ventured to suggest, that an unbiassed investigation of the subject forces upon us the conclusion, that our present methods of treating acute diseases were in the highest degree unsatisfactory; that the data upon which such methods of treatment were founded are uncertain; and that the results of such treatment were found to be most contradictory in the hands of different individuals.

This view I illustrated by referring to the very opposite practices followed by equally talented and honourable practitioners in their treatment of disease; to the revolutions which the opinions of observers, in different generations, as to the powers and effects of remedies, and of the same observers at different periods of their own experience, were continually undergoing.

And then I ventured to ask, specifying some particular acute inflammation, the *actual grounds* upon which the physician, who proceeds to treat such inflammation according to orthodox methods, builds his belief in the efficacy of his remedies; that he should state in distinct terms the reasons upon which rests his faith in the effi-

cacy of this or that special line of treatment; and then I could not help suggesting that he himself would be astonished at the insufficiency and barrenness of those reasons when thus reviewed.

Now, it seems pretty clear, that the grounds upon which these reasons must be based are to be sought either in the result of a man's own experience, or in the information which he has obtained through the experience of others, and tested by his own opportunities of observation.

With regard to the results of a man's own experience, I would observe, that it can rarely, if ever, be taken as decisive of the advantages or disadvantages of any particular line of treatment, even though his experience be very great; and there is no difficulty in understanding this, if we will reflect upon the endless complications which beset the subject of remedial agents and their effects, and the vast amount of disturbing causes which render satisfactory conclusions so hard to attain.

The conditions under which our remedies are necessarily administered are ever varying—so varying, that one may assume it to be a thing impossible to apply a like treatment to two exactly similar cases. We are early taught the modifications which age, sex, seasons, climates, diet, constitution, pre-existing disease, states of the mind, states of the different organs of the body, hereditary tendencies to disease, etc., etc., introduce into what are considered the ordinary and proper effects of remedies. What, again, is called the idiosyncrasy of an individual is that peculiarity of constitution which makes him to differ from another; and is there one individual living who does not differ from another in that sense which relates to the action of remedies upon the body? When we calmly take into our calculation the whole series of necessary or natural and accidental conditions of things which must in the human body inevitably act upon, and reciprocally be acted upon by, our remedial agents, before their influence can be brought to bear upon the disorder for which they were specially administered, we shall be forced to acknowledge that every living person has an idiosyncrasy as regards the administration of remedies; and that it is, in the present state of our knowledge, scarcely possible for any individual to decide as to the action of remedies by the light of his own experience alone; the action of remedies, I mean, upon those acute diseases, where the apparent effects are neither uniform nor constant; where they have, in fact, so little uniformity and constancy in their results, as to induce certain trustworthy observers to doubt their efficacy altogether.

I trust I need scarcely observe here, that my remarks as to the treatment of disease in no way whatever apply to those cases where the results of remedies are so constant and so little variable, as to leave no room for doubt in the mind of the candid inquirer as to their efficacy. I am confining my attention to those large classes of acute diseases, where treatment of the most opposite

character is declared by men, equally competent to judge concerning it, to be equally efficacious for their cure; and to other diseases, wherever difference of opinion as to the right mode of their treatment is so great, and so contradictory, as to justify an unprejudiced person in the belief, nay, as to force him to believe, that there must be some mistake in the conclusions drawn from the results of such treatment, and that the whole subject of the administration and effects of remedies is one demanding, reasonably, a reconsideration on the part of practitioners.

When we first apply ourselves to the treatment of disease, we are, in our inexperience, necessarily driven to place ourselves under the guidance of others, and the wisdom of our ancestors is there for our guidance and instruction; this experience of theirs, we are taught, is thus transmitted to us, to be tried and tested by our own daily experience, and modified or rejected accordingly as it bears that test; we are to hold firmly to that alone which stands this trial of experiment. And what, after all, is the value of the experience of individuals thus gained in this matter? what influence does it exercise over the majority of practitioners? We are, perhaps, ourselves, scarcely conscious (until we come to consider the matter a little closely), how little our own actual experience is a guide to us in directing our treatment of diseases, and how much more the experience of others influences our opinions and our actions.

We naturally enough receive, in the original instance, our ideas as to the efficacy of particular remedies from others; their teachings are our first guide; custom gives them favour in our eyes; and habit at length exerts such power over us, that the very name of a disease, when mentioned, becomes instantly associated in our minds with the name of some particular remedial agent. Every one must have felt, in his own person, the influence which the lessons of some favourite authority, learnt in early youth, have left upon his mind; in years, long after, the meaning of those lessons still lingers in his thoughts, and directs his hand in practice; and thus it happens that, more or less, and to the very last, our practice is biassed, guided, and directed, not so much by our own, as by the opinions which we have derived from others.

But it may be said in answer to this, that although men are thus governed by the opinions of others, still the results of their own practice, and their own personal experience, confirm those opinions, and so far prove their correctness; and what more forcible mode can there be of impressing belief in any conclusions on the mind, than by showing their constancy as results flowing from certain premises, as certain consequences following with regularity upon certain conditions of things. This objection seems very difficult to answer, and, undoubtedly, to the great majority of practitioners, it is unanswerable. Nevertheless, the following undoubted facts must, in a greater or less degree, demonstrate its speciousness in the present case, the facts, viz., that in the hands of different observers the same

results flow similarly and equally from the most opposite methods of treatment; that the same treatment, in different hands, is followed by different results, *longo intervallo*, or *toto celo* different; that a method of cure, lauded to the skies to-day, to-morrow deceives the hopes of him who sang its praises; that there is a fashion attached to the use of the articles of the pharmacopœia, just as there is a fashion in other more indifferent matters; that the treatment of one generation, and in the most striking particulars, is reversed by the judgment of the next generation.

As corroborative of the same position I am maintaining, I might also add this other fact, that in order to account for these diversities of opinions and these differences in results of treatment, the supposition has been made that diseases change their type, and that, therefore, a treatment which was well adapted to their cure at one epoch is useless at another; some observers, indeed, have very "firm convictions" that this typical change in diseases is true, and they almost take it for granted as an undeniable fact; but when we come to investigate the grounds upon which rest those convictions, then we find what "a baseless fabric of a dream" they form; the convictions become mere vague ideas, resting on some floating notions in the mind. How, for example, can we prove the dissimilarity between pneumonia of to-day and pneumonia of ten years ago, except by comparison of the history of the disease now with its history then? Well, I fancy he must be a bold man who would undertake that effort. Differences in type at different epochs! why! how many differences in type does not the same disease present at the same epoch? What are all those different modifications in symptoms which disease presents to us except changes in its type; changes chargeable to the influence of age, sex, climate, and the thousand varying accidents which beset the life of man! We have acute pneumonia, "*franche pneumonie*," the simple inflammation; we have typhoid pneumonia; we have tubercular pneumonia, and so on; and what different ideas as regards the prognosis and the treatment of pneumonia do not those qualifying terms excite in our minds; surely these are types different enough of the same disease, and plenty more might be added to them; how then, in the face of such facts as these, can we with any show of reason pretend to talk of pneumonia varying its type at different periods? No, I am compelled to see in this idea also further evidence of the mistaken opinions we labour under regarding the influence of remedies over certain diseases. If the remedy which was found efficacious in the cure of disease by authorities of the past generations, prove a failure in our hands, we prefer rather to indulge in the visionary belief that the disease which they treated was not the disease which, under the same title, presents itself to our notice now—we prefer this, rather than question the treatment adopted, rather than admit a doubt as to the effects ascribed to the remedies administered.

Many years ago an indelible impression respecting the difficulty

attending the treatment of disease was made upon my mind. The incident which gave rise to it seemed to me then very startling, and as it is strongly illustrative of some of the remarks I have here ventured to make, I will shortly refer to it. The disease of whose treatment I am about to speak was typhus; the treatment was carried on in three separate wards of the same hospital, on the first, second, and third stories of the hospital; three physicians of high repute had charge of the wards, each of a separate ward. Now, it must be understood that into these wards were carried indiscriminately the fever cases of a large city, just as they occurred, and just as they presented themselves to the hospital, so that here at least there can be no room for the supposition of any peculiar types of the disease; any differences in the character of the fever attributable to different accidents of climate, modes of life, and so on; the class of people were the same, subjected equally to the same conditions of existence, to the same sources of contagion, and the same disgraceful sources of pollution, which then rendered their habitations notorious. These victims of fever when brought into the hospital were thus subjected to treatment.—In one ward, port wine and other stimuli were administered with an unsparing hand, and no period of the disease was too early for their prompt administration. In another ward the disease was treated *pro re nata*, there was no prescribed formula, but as symptoms indicated, or were supposed to indicate, so the remedies were directed. In the third ward the essence of the treatment consisted in the most energetic attempt to strangulate a certain inflammation which in this disease made the strongest efforts to fix its seat on that very vital organ the brain; the most copious effusions of blood, leechings, cuppings, and blisterings, formed the staple agents by which the physician of this ward attacked his foe; stimuli were forbidden with an unswerving rigour; the treatment was a combat à l'outrance between the lancet and the fever, at least in the physician's eyes.

I did not, of course, introduce this *extreme case* here for the sake of alluding to the treatment of fever, but solely to back me in the facts I am endeavouring to establish; which treatment was right or which was wrong, or if they were all defective matters not here, but right or wrong this much is certain, that the students who year after year witnessed the treatment would in after life as practitioners, and to the end of their days, be more or less influenced in their treatment of fever by the accidental circumstance of their having studied under this or that physician; in one case they would see a disease which overwhelmed and oppressed the organs of the body, reducing their vital forces, enfeebling, and weakening, and annihilating; in the other case they would see just the reverse, an exaltation, acute inflammation of central organs and its destructive consequences.

I spoke before of how little we are really guided in the treatment of disease by the results of our own experience, and I cannot but

think that the results derived from experience had little to do with directing the treatment here. A predominant idea held possession of the physician's mind, and his treatment was guided thereby. The results were, of course, in his opinion, a justification of the treatment, but is it possible to believe that such totally opposite methods of dealing with the same disease could be followed by consequences which, in reason, equally justified these methods. I cannot but lay particular stress on this case, because I consider that if we will take the trouble to apply it to many of our own proceedings in practice, we shall find that the results of our practice do as little prove the merit of those proceedings as does the treatment here spoken of seem to do in the case of fever.

When a foregone conclusion has once taken full possession of the mind, the facility with which facts accidentally associated with such conclusion are made subservient to its illustration and proof is surprising. Unfortunately it happens that the occasion for the adoption of fallacies is very great in the conclusions which may be drawn regarding the action of remedies ; between the administration of a remedy and what we presume to be its manifestations in the body, speaking generally, all is a complete mystery—all is hidden from us ; and thus a large region is opened wherein imagination may delight herself. In this region it is that the empiric revels, delighting his facile clients with the special language of his class ; bracing up the nerves, purifying the humours, giving a tone to the system, fortifying it against outward attacks, settling on the nerves, poisoning the sources of life, and so forth ; such is the poetical language with which he charms his auditory, and effectually veils his own ignorance. Poetical figures of speech how often do they stand us also in good stead when hard pressed by our patients ! We deceive them thereby into a belief that we possess a knowledge which we do not possess, and we end by deluding ourselves into a similar belief.

Perhaps I might mention one other remarkable fact connected with the treatment of fever as above spoken of, for it is not the least interesting or curious part of the story. A distinguished physiologist was pathologist of the hospital at the period in question, and during one year he collected carefully (and not a more careful, calm, cautious, and philosophic investigator of facts, adorned the university which had the honour to call him its scholar) the records of the mortality of the fever cases which took place in the hospital referred to ; and he found after a careful comparison of the results that the mortality, other things being equal, under the several methods adopted in the treatment of the disease was very much the same.

Again, suppose a man of ordinary intelligence, not a practitioner of medicine, were to have laid before him the therapeutical history of mercury, its past and present history, the history of one of the most important items of the pharmacopœia, what would he think of the condition of medicine as a science ? I need not go back and make reference to its use, or I should say abuse, in past days ; of the

mode of its administration as so graphically detailed by the indignant pen of the late Sir Astley Cooper, or of the fearful consequences resulting from such administration, some of which remain recorded, *in corpore vili*, in certain of our hospital museums.

Let us consider, in a superficial way, the general manner of its use at the present moment; take, for instance, its administration in that disease with which its name is so intimately connected, in syphilis. What do we find? well, north of the Tweed the induction of mercurialization is anathematized in this disease; south of the Tweed its induction is, as a rule, held to be a *sine quâ non* for the cure of the same. Leading teachers of surgery have long taught in the Scotch school that mercury should never be administered in syphilis so as to produce salivation, but should be used "merely to promote the secretions, and act as an alterative in restoring a healthy state of the system." "All those severe constitutional effects which embitter the patient's existence and ultimately destroy it, are still met with in the practice of those who employ mercury profusely and indiscriminately." "This medicine produces those effects, more especially those on the bones, only in persons who are suffering from venereal ulceration of the genital organs." Thus, for years, have students in the north been taught; and I believe I am in no way exaggerating, if I say, that the rule is, amongst Scotch practitioners generally, to regard the specific effects of mercury produced in syphilitic diseases as little better than a species of slow poisoning. Cross the Tweed, and then we find, that in the very same disease, the very same remedy, in England and in France at least, is administered as the rule, and that its non-administration is the rare exception, its administration, I mean, for the special purpose of producing mercurialization. "Le mercure . . . c'est le remède le plus sûr et le plus puissant, jusqu' a ce qu'une spécifique soit trouvé," writes Ricord. Every one, now-a-days, I must suppose, admits the specific nature of the syphilitic poison; no one can believe that the disease is one thing in London and another thing in Edinburgh, that its cure requires that treatment there, which is found worse than useless here, a little remove farther towards the north pole.

But what is the case? Certain symptoms which in London are looked upon as the consequences of syphilis, are held in Edinburgh to be the results of mercurial poisoning; the very remedy which, in the hands of this man, is called a sheet anchor, is the same which, in the hands of another, is proscribed as a positive poison! When facts like these come before the unbiassed public, what are they to think of the condition of our therapeutical knowledge, surely they are justified in believing that it is impossible to reconcile such complete variance of opinion, except by supposing that there is a gross misconception somewhere involving the whole subject. This very year, I learn, that in the first medical school in this country, one of the professors, not a mesmerist nor a homœopath, be it observed,

but a most energetic and unwearied worker in the science of his profession, has actually taught his class, that from the beginning to the end of it, mercury is nothing but a downright poison. He must defend his own thesis, and he is perfectly capable to do justice to it. I only proclaim the fact. I see, however, nothing more surprising in such a conclusion, nothing more conflicting between it and generally received notions on the subject, than I do in the opposite conclusions arrived at by professional men, concerning the effects of salivation in syphilitic diseases.

It is surely not necessary for me to multiply proofs of the fact, that the opinions physicians hold regarding the nature and treatment of many diseases are very various and often very opposed to each other; the proofs of it may be readily found by a perusal of the history of such diseases. I would, however, just shortly refer to some few practical conclusions, which appear to me to result from a consideration of this subject.

One conclusion is, that more faith is placed in the power and efficacy of therapeutic agents as regulating and controlling the courses of many diseases than the reason of the case warrants us in ascribing to them, a conclusion proved by the fact of the want of uniformity, and of the actual diversity, exhibited in the treatment of the same diseases by practitioners in different countries, or at different epochs in the same country, or at the same time, and in the same place, by physicians practising on individuals subjected to precisely the same external influences. As a corollary of this, also, it may be suggested that inasmuch as the proofs of the beneficial effects of such agents are not, in such cases, deducible from the premises, it is quite possible that these agents may occasionally (if they have any effective power at all) exert a deleterious action on the system, a side of the question, which, if not altogether disregarded, can scarcely be said to have received its full share of attention. It is impossible to look upon the specific action of mercury or the system as an indifferent matter; if its induction is not the right cure for a complaint, then surely we may presume that its consequences may be injurious. If erysipelas requires for its cure in London, bark, port wine, and such like remedies, then the treatment which prescribes bleeding for the same in another city, can hardly be looked upon as innocent, and *vice versa*.

One of the most talented physicians of the day, writing a few years ago concerning the treatment of a particular acute inflammation, observed, that such was the vigorous nature of the onslaught made by the disease that we were sometimes forced to bring our patient, *by treatment*, almost to the door of death, if we would prevent the disease carrying him through that door altogether; could such dealing with his body be a matter of no importance to the person most concerned, if it happened not to be the right dealing with it? Such treatment is not only not adopted now, but it is proscribed as dangerous. If a therapeutical agent is potent enough

to produce some manifest action on the vital organism, it may be potent for evil as well as for good. That salvo to the conscience of the doubtful practitioner, "at all events it can do no harm," always sounds to me as the expression of a very poor logic; if the agent under no circumstances can do harm, it is pretty evident that neither can it, under any circumstances, be suspected of doing any good, in other words, it must be supposed to possess no physiological action whatever—to be inert.

It is very well for us to deride "the milk and water practice" of French physicians; but, after all, a consideration of this subject rather tends to foster the idea, that their "*diète*" and "*eau guimauve*," may not be farther removed from a show of reason than our polypharmaceutical system. And there is one objection thrown in the teeth of the orthodox practitioner of medicine, which appears very difficult for him to answer. How, the objector asks, can you prove that your *medicine* produces such and such results, you have never observed the progress of the disease when such medicine was not exhibited? And he adds, perhaps: I have tried the orthodox plan, and an expectant treatment likewise, and I prefer the latter; statistics seem to be in favour of my doing so. Now, if it happens that, as the results of their long experience, men with European reputations have been led to diminish or modify their faith in the efficacy of remedies over certain diseases, we surely have no right to turn round on and deride them; they are honest men; they have tried the interfering and the expectant plans, and make their choice. We ought to be cautious in this; we have little opportunity of judging in the matter; custom has decided, and habit confirms the custom, that, in the face of active diseases we must be vigorously active with our remedies. We speak continually of a *vis medicatrix*, and of guiding and directing the force, and of removing obstacles to its free operations; but have we any data to show what of the cure depends upon this *vis*, and what of it is due to the interfering hand of the physician? Have we any data by which we can prove our powers of aiding, guiding, and abetting in its operations this our great ally, this medicating *vis*?

What is the interpretation to be given of many of the successes boasted of by that outrage on science and common sense, homœopathy? What but this, that the mere cessation from long indulgence in the sweets of routine polypharmacy has given fresh life to the over-drugged body. The truth is, that from our earliest days we have been bred up in the idea, that physic and physicians are inseparable things, that drugs and diseases are necessary opponents. When Molière introduces a physician on the stage, it is generally with a chorus of apothecaries, armed with the classical syringe, in his suite. We are all trained up in exorbitant notions respecting the power of drugs over disease. But our patients, it may be replied, expect us to do something; they have no idea of being left without efforts for their restoration to health being made; they have

faith in physic and faith in its practitioners; to tell them that you see not how drugs can avail them, would be like dismissing them helpless into the jaws of the enemy; their imagination then would react banefully on their disordered condition, and thus aid the progress of the malady.

The objection is one to which I scarcely know how to reply, for as regards the *individual* it is a very strong one, but as regards *the reasonable being* I think this may be said to it: Why does the patient thus love and believe in drugs on all occasions, and their necessity? Why, but because, in all his bodily distresses, in them we have taught him to put his trust. What would a patient think of his physician if he were to dismiss him without a prescription? What physician ever attended a consultation without inditing his ordonnance? We have led the world in very many instances to place undue confidence in our remedies—undue, because it is a confidence which our own observation does not warrant, as I have endeavoured here to show, and therefore it becomes the duty of our craft to make, at all events, an effort at redeeming the public mind from the error in which it is involved.

Space forbids me doing more than briefly mentioning the two chief injuries which the polypharmaceutical system brings upon the patient. He is taught to believe, by the temporary relief which he thereby receives, that he may continue with impunity to indulge that particular course of life, for example, to which his malady may fairly be attributed, as its cause; such belief distracts his attention from that main point, which should rule all other considerations, viz., removal of the causes which excited the malady. Can any one deny that it is much more rational to tell the patient that certain infringements of the organic laws, which his account of himself enables us to point out, are the sources of his complaint; that the true remedy for the same is in his own hands; that to look for its cure by drugs is a deception, and worse than a deception, beguiling him on into a continued and still farther infringement of those laws—much more rational, than endeavouring by the aids of pharmacy to patch up, restore, and combat against evils whose causes are constantly permitted to operate with impunity? Inordinate attention to the purely therapeutical treatment of disease, distracts our attention from the proper management of it by a due consideration of diet, habits, modes of life, and so on. It is only of very late years, we have discovered that the successful management of typhus, for instance, has much more to say to the management and regulation of the diet, that is, of the right administration of articles of diet, than it has to do with the articles of the pharmacopœia. Specific remedies for its cure becomes rarer every year, and the most experienced physicians, if we may judge from their writings, are they who trouble the disease the least with the articles referred to.

Before concluding, I would desire to say, that I trust no one will suppose me silly enough, in anything I have here said, to be

sceptical as to the uses, advantages, and power over diseases which therapeutical agents possess ; or that I am desirous of running my head against the rampart of solid real facts which medicine has stored up concerning the treatment of disease, and of which she possesses such a goodly garner. Nor do I wish to interfere with experiment ; I am perfectly aware that we can only get at the facts of the case, at the meanings and true interpretation of these things by experiment ; that we must give the drug, apply the remedy, and watch the result. The method is correct enough ; I differ not from any objector as to the mode of getting at the fact, but only as to the value which is to be attached to what are called the results of the remedy. The remedy is given, a result follows, and certain conclusions are drawn as to the intimate relation between the remedy and the result, as to the one being the cause of the other. In the present state of our knowledge concerning the action of remedies, I cannot but think the inferences thus drawn are often very erroneous, I cannot think them legitimate. Why I think so, I have endeavoured here to show.

ARTICLE III.—*An Experimental Inquiry into the Existence of a Sixth Sense, here called the Sense of Force.* By RICHARD F. BATTYE, Esq., London.—(Continued from p. 140.)

THE knee-joint is by far the largest, and has synovial membrane more abundantly distributed to it than any other, joint in the body ; here, then, we should suppose that a consciousness of a feeling of weight would be above all others the most apparent ; but in this respect we shall be greatly deceived—since the mechanical relations of this joint check the manifestation of the feeling of weight in the sense of force, and supply those of strength, or resistance, only.

This peculiarity may be set forth in the following manner :—The femur is very much more constricted in the shaft than at its extremities, especially its inferior condyloid extremity ; moreover, the shaft consists of firm and dense osseous structure, the condyloid extremity, in its interior, of spongy or cancellated osseous structure. The consequence of this contrast of texture must be, that whilst the weight of the trunk is sustained by the arched shaft of the femur in a narrow space, all unexpended force which passes from thence to its inferior condyles is distributed over a much wider surface, the arrangement of the cancellated structure of which affords great facilities for the widely diffused force to come to composition, whilst channelling along rows or bands of osseous tissue placed at opposite angles to each other. Furthermore, though the osseous tissue covering the articular surface of the condyles is very hard and dense, yet, like its antagonist in the tibia, it is *exceedingly thin*, and therefore the two opposing surfaces, the femoral and tibial articulations, present no sufficient barrier for the resisting, or regulating of forces, impinging against

them. But, by their diffuse spongy tissue, a ready path is opened both for the diffusion and expenditure of force; neither is it till the force is aggregated or compressed into the much narrower and less cancellated structure of the tibio-astragaloid articulation that, when the body is raised and depressed successively upon the anterior extremities of the feet, with the knees in a semiflexed position, that we experience any sense of weight, at which time it may be distinctly felt in the ankle-joint and ball of the great toe.

But if the knee-joint present a structure, the osseous mechanism of which is peculiarly adapted to diffuse and expend force, from the very nature of such mechanism, as has already, in the introductory remarks, been referred to, it is incapable of conducting any force to the articular surface in the same direction in which it was first received, but in some course or direction which the mechanism of the structure of the osseous tissue, etc., of the joint compels it to assume, *independent of previous bias*. Therefore, it follows, that the feeling experienced in the joint itself must obey the indications of the *forced or compelled bias* which its structure is adapted to secure, and not that which external appliances appear likely to determine. (Of course the remarks relative to fibro-cartilage, as well as to the spongy tissue of the bony framework, already detailed, apply to the joint at present under consideration.)

To throw the matter into fuller relief, contrast this with the ankle-joint, and the articulations of the metatarsal bone of the great toe, unprotected with articular fibro-cartilage, with the additional advantage of the constricted and comparatively dense inferior extremity of the tibia; so constructed as to gather the widely distributed force at its superior extremity into a narrow surface inferiorly, and that too covered with simple cartilage, whose line of direction being continuous with the long axis of the bone, the mechanism of this structure is in every way adapted to perpetuate motion in the same direction in which it is first received.

The same remarks, relative to the knee-joint, apply with almost equal force to the *hip-joint*; since, though the rim of the cotyloid cavity, and the head of the femur, have not so widely diffused a cancellated structure, yet the obliquity of the surgical neck of the femur to the shaft of the bone will completely destroy the precise direction of pre-existing force channelling along the shaft. Again, the peculiar mechanical arrangement of the pelvic bones, and the variable angles at which force will meet, and, by counterpoise, be brought to equilibrium, or nullity, ere it reaches the hip-joint, greatly tend to limit force; while the residue, not so composed, will, by friction, be much checked in rapidity, and reduced in original impetus, and will reach the joint, for the most part, in one common direction according to the course any force may have to take along the pelvic bones, and not in relation to the direction in which force is first received upon any part of its firm framework. The result of this compulsion, in relation to the course or line of force,

will be, as stated in an early part of this paper, that in place of a consciousness of weight being experienced in the hip or knee-joints from any force reaching, or being applied to, their articular surfaces in an oblique direction, a feeling of *strength* or *resistance* will only be felt.

Notwithstanding the special pleading in the foregoing paragraph, it will nevertheless be asked, is it not possible to induce by experiment a consciousness of *weight* in these joints, as an antithesis to their normal feeling, and so render the hypothesis of one sense presenting the combined function of opposite states of consciousness, as that of resistance or strength, and of weight, more determinate, and so place the hypothesis upon the broad foundation of experiment and induction? To meet this very natural inquiry, I will endeavour to illustrate the hypothesis by only one experiment for either joint.

I will commence with the knee. If I extend the thigh and leg *backwards*, and place the foot, or rather the toes, upon a chair or a high stool, taking care to hold by some fixed point, lest, in such an unnatural position, I lose the balance in the opposite limb; and, if in this position I place a poker or any other suitable instrument in the popliteal region, exactly opposite the joint, I shall feel the full weight of the instrument; but if I place the same a little above or *below* this site, very little consciousness of weight is experienced.

This, then, is almost the only experiment that I have been able to devise for this joint; and, since it appears to be a pretty conclusive one, and not open, as far as I can see, to any objection based upon the conditions required for a conclusive experiment upon the present hypothesis, I need not, if I could, proceed any further with multiplying experiments upon this joint. I shall merely add, that in the foregoing trial, all the provisions made in the mechanism of the joint to throw force into one common and arbitrary course, independent of the manner or course in which it is received, are here obviated; and the diagonal of the two forces—gravitation and muscular force—will reach the sentient surfaces of the joint little expended, and at that line of obliquity which is here supposed to be well adapted to give to the nerves of the part a suitable stimulus for exciting the feeling of weight.

As regards the hip-joint, it will be perceived that in devising any experiment for it, difficulties surround us on every side from the peculiar position of the articulation; but here, as before intimated, one experiment may be set forward.

In performing an experiment upon the hip-joint care must be taken as to the position of the party to be experimented upon. First, then, let the party be seated in a chair, and leaning backwards; let the (right?) leg be semiflexed and resting upon the ball of the great toe, very much in the same position as a sociable man will take a little child and place it upon his thigh or knee, and give it—as the little creature will call it—a ride. In this position let a poker be placed

external to Poupart's ligament, and immediately below the inferior anterior spinous process. Thus adjusted, let the party gently raise and depress the limb upon the internal axis of the foot (ball of great toe), when a considerable degree of weight will be felt; place the same instrument superiorly or inferiorly to the afore described position, when scarcely any weight will be felt.

Here we have not quite so decisive an experiment—from the difficulty of performing it—as that upon the knee-joint, from the conformation of the joint itself. Yet here, as elsewhere, in that part where the two forces—muscular and gravitation—can be brought to *antagonize* with each other—from the variable angles at which they may be brought in contact with each other—we have the consciousness of weight most apparent.

In suggesting these two experiments it may be well to add a caution or two in case any attempt be made to vary them. The first caution has relation to the hip-joint, and comes within the category of an *apparent exception*. Supposing a weight, as a poker, be placed internal to Poupart's ligament at variable points, till it reaches the mesian line over the symphysis pubis; the intensity of feeling or consciousness of weight will be found to gradually increase till the mesian line is attained, where it is the greatest, and gradually diminishes as the object is removed external to that point. The intensity in the feeling of weight, in this region, is owing to the distribution of nerves of force to the integument, at least so the writer thinks, and he will, as before stated, endeavour to examine the matter in a subsequent paper.

The other caution refers to the knee-joint. Suppose an individual, after trying a few experiments, conceives that some more decisive ones might be brought to bear upon the subject, and in his diligent zeal tries to realize the weight of the trunk by raising and lowering it, by gentle flexion and extension of the lower extremities, he will find, by so doing, that a sensation of weight is experienced in the ball of the great toe and ankle-joint. But could not, he will say, a similar sensation be induced in the knee-joint? Suppose then, again, that the same individual, to limit the weight of the body to the knee-joint as much as possible, and so make the trial more conclusive, balances the body upon one leg, flexing the trunk upon the thigh, and the thigh upon the leg, and by very powerful contraction of the muscles he raises the body into the erect position. Contrary to his feeling any sense of weight in the joint, if it is more than he is equal to, the exertion may at the time, from too great pressure, produce aching or pain in the joint, but if he is quite equal to it, whilst raising the body, he will only feel a sense of resistance or strength, which, according to the conditions under which this sense responds to the stimulus of force in the knee-joint, ought to be the case. But, *as soon as* the extension ceases, a feeling of aching and weariness immediately follow, sufficient to drown all other previous feeling in the limb. Yet, as experience has

taught us that this feeling follows great muscular exertion, or slight and *long* continued, it is naturally supposed that here is the sense which supplies us with a consciousness of force. But the consciousness is rather of a negative character than positive, for, whilst action or contraction in muscles is going on, vigour and strength are experienced, but when that has ceased, *and not until then*, weariedness is experienced; and this feeling of weariedness is experienced most intensely over the region of the fascia lata; but more or less *all over the body*, especially where fascia is the most dense.

To enter into any detail upon this last mentioned feeling in the present paper would be quite out of place, but the writer may state that he has no reasons for supposing that the nerves pertaining to the sense of force have any thing to do with it, beyond their being a kind of contrast, and, if he may so speak, a counter-compeer to the same. The writer cannot well conceal his impression that it was from the feelings of weariness, as just described, that Sir C. Bell inferred there was a muscular sense.

To return, though a little out of order, for brevity's sake, we will consider the shoulder-joint. This joint presents all the peculiarities for the distribution of force, and of throwing it into one common course, which the two preceding joints have presented; but, from the manner in which it is protected by the acromion and coracoid processes, I have not been able to devise any experiment whereby the feeling of weight can be induced, the feeling of vigour or strength being alone felt.

In relation to the three joints, knee, hip, and shoulder, it is worthy of remark that we are daily in the habit of judging of the activity and vigour of the muscular system, in any given individual, by observing whether the knee and hip joints are tightly compacted together, and the shoulder-joints appear neatly and closely approximated. This arises, apparently, from the fact that every one, when about to put forth much strength, has a feeling within of these parts being so adjusted, and he naturally applies his own convictions, or feelings, in observing and measuring others; thus a popular standard for physical force has its foundation in physiological induction.

The next part to be examined is the spinal column. The transverse processes of the vertebræ are lined or tipped with synovial membrane, but, from the very limited amount of motion, especially in the dorsal region, this membrane is scarcely discernible. But in the cervical and lumbar regions the motion is not so limited, and the synovial sacs are consequently better developed. Here nervous tissue may be supposed to be but very slightly supplied, yet to the integument of these parts I imagine that the nerves of force are well developed, so that when a weight is placed on the lumbar region it is very soon felt, but not, I presume, from nerves distributed to the synovial membranes of the transverse processes—probably they may assist. The same remarks pertain to the cervical region, till we reach the atlas in its articulations with the axis and

occiput, at which parts, especially with the occiput, synovial membrane is very freely distributed, and appears to be always well lubricated with synovia.

If, then, a book, or solid piece of wood, say the size of Kidd's Bridgewater Treatise, be made to rest across the cervical region, or be balanced upon one of its angles, and, with the neck bent, be placed in succession from the seventh to the first cervical vertebra, at short points of distance from one another, it will be found that the feeling of weight attains its maximum opposite the first cervical, where weight is felt to no inconsiderable degree, when the experiment is carefully conducted.

Again, if any experiments are tried in the dorsal region, where no feeling of weight is experienced, from a book, or any other suitable object, being made to lie or rest with its long border upon their spinous processes; yet, if the book is laid *flat* upon the back, over the dorsal region, and, by being so placed, imparts part of its weight to the ribs bending outwards from either side, that weight will be distinctly *perceived*. This arises from either of the articular surfaces of the heads of the ribs, resting upon corresponding facets in the bodies of the vertebræ, being tipped with synovial membrane; and such an apparent exception only tends to confirm the hypothesis advanced.

Next in order comes the head. The experiments upon this part appear to be deserving of particular attention, and tend, as much as any hitherto advanced, to confirm the hypothesis here maintained.

EXPER. I.—If a weight, say a polished stone, or a piece of lead, 2 lbs. in weight, and tipped with leather, be placed in the mesian line of the head, at the sagittal suture, *opposite to the condyles of the occiput*, it will be found that a consciousness of weight, greater than at any other part of the head or neck, will be experienced, and also, that it will be felt more quickly than elsewhere. If the nerves distributed to synovial membranes supply us with the sense of force, or a *consciousness* of its existence and degree, then, in any given region, where such nerves are distributed, over that part where force will channel the quickest, and with the least expenditure by friction to the seat prepared for its perception, there, if the premises are correct, shall we feel it the earliest and the most intensely. Perhaps it need scarcely be observed, that the cranial bones, from their thinness and compactness, are better adapted to convey force, considering their extent of surface, than any other in the body; whilst the loss by divergence, when weight is placed in the centre, on the top of the head, is greatly compensated by this region being balanced upon equilateral condyles. These, by being supplied with sentient nerves, adapted to test force on either side of the centre of gravity of the head, act the part of a pair of scales, in guiding and directing muscular action how to apply its force in adjusting the organ placed under its protection.

EXPER. II.—Place the same weight, as was used in the first, on the frontal bone, as far anterior as can be accomplished with con-

venience, when but slight weight will be felt; but retain the weight for some time in the same position, and the feeling of weight will gradually intensify, but will never attain to the same degree of intensity as in the first experiment. The expenditure of force by friction, before reaching the condyles, sufficiently accounting for such diminution; and its more tardy recognition is equally satisfactorily accounted for, from the force, in this instance, having to travel obliquely, and also at a greater distance from the seat of perception than in the former case. Such a contrast, under circumstances so similar, can, I think, only be accounted for by an hypothesis, either similar or identical with that here maintained.

EXPER. III.—Let the same weight be placed on the upper lip, immediately below the nose, the countenance looking upwards—in this position the amount of weight experienced will be little different to that in the last experiment; but it will be *much sooner felt*, since it has a much shorter course to run along the zygomatic arch to the occipital condyles, but, from the narrowness of the bridge or arch, much of the force must be expended in other parts ere it reaches, if it ever does, the desired point; hence the weight felt, though a shorter course to run than in the second, is not increased by taking such a course. Of course, experiments can be tried at any point between the 1st and 3d experiments; but it will be observed, that the *quickness* with which weight is felt will bear a strict relation to the distance the gravitating body is placed from the occipital condyles, and also the *intensity of weight* experienced from any given body placed upon any of these parts, *ceteris paribus*.

The transit from the last experiment, where the superior maxillary bone was the resting point, to the inferior, is natural and easy. This bone, the inferior maxillary, articulates with the temporal bone in the glenoid fossa: between the head of the inferior maxillary bone and the fossa is placed an articular fibro-cartilage, on either surface of which is a synovial sac; but, as age advances, the fibro-cartilage mostly becomes absorbed towards its centre, and the synovial membranes are almost defaced at this point. The thinness and hardness of the fibro-cartilaginous disc makes this structure capable of transmitting force to the synovial membranes, without much interruption to their natural course. Moreover, if it did not so act, one membrane is always free to receive impressions from force channelling along the osseous tissue: since it is placed between two synovial membranes, both of which are placed in juxta position with the bony articular surfaces; hence any heavy body is felt when balanced on the chin, as well as elsewhere.

But it will be asked, why, if this bone has articular surfaces adapted to take cognisance of weight, do we not feel weight, of a greater or less degree, at all times when masticating food? It has been before stated, that a feeling of strength was the normal or *un-counterpoised* state of this sense, and that weight was the result of gravity *counterpoising* muscular force; now, the very end of masti-

cation implies, *a priori*, that the thing acted upon is inferior, or in subjection, to the masticating power, or else why *attempt* to subject it to such a process? And, if subject to it, it cannot be upon a parity with it, or a counterpoise, but, so far as gravity at least is concerned, it must needs be considerably plus; and hence all feeling induced by, or connected with, counterpoise, must necessarily be wanting under such circumstances, and therefore, in partaking of food, vigour and strength ought (as they are), to be experienced, and not weight and labour.

I might remark that, in experimenting upon the chin, if a weight of 3 or 4 ounces be placed upon the chin, its weight will be quickly felt, but not to the same degree when the countenance is but slightly elevated above the natural erect position; but, if it is made to look directly upwards, the same weight will be felt much more intensely. This evidently arises from the same weight channelling its force onwards to the occipital condyles, whereby, by increased sentient surfaces presenting themselves, increased feeling of weight is experienced. Such incidental peculiarities are not to be altogether despised and rejected!

I shall pass over in silence the articulations of the clavicle, not from not having experimented upon them, and that satisfactorily; but any one who has carefully examined their synovial membranes, and also the very variable degrees of mobility different individuals possess in this articulation, will agree with me, that, from ordinary grounds of doubt, in articulations so variable as are these, experiments, as a test of correctness, or contrariwise, of any assumed hypothesis, are better avoided than pursued.

The shoulder-joint has already been examined and dismissed—the next in order, therefore, will be the elbow-joint. This joint is a compound one, the articulation of the ulna with the humerus being as perfect an example of a ginglymoid joint as the skeleton supplies; whilst the articulation of the radius with the humerus more closely resembles the enarthrodial order of articulation than any of the orders of articulation usually given in anatomical works.

In attempting to perform any experiment upon this joint, it will be well to remember the distinctive kinds of articulation the radius and ulna have with the humerus. With reference to the ulna, it is difficult to conceive how gravitating force would channel, if a weight were placed over the coronoid process, and how far it would affect the sigmoid notch, much less, if it were carefully analysed, would it be easy to describe the same: neither, again, is it an easy matter to place any weight over this part. Though, then, the writer has tried several experiments upon this articulation, with apparent success, yet, from the above reasons, he deems it wise to *avoid* giving any of them in details, and will consequently direct attention to the radial articulation. This articulation being very simple, affords great facilities for retaining distinct the antagonising forces, muscular and gravitation.

One experiment will be sufficient for all practical purposes. Let the experimenter extend the arm and forearm, but not to perfect straightness or rigidity; at the same time let the hand be supinated, or in that position in which a large flat book would be balanced on the surface of the hand. In this position, place a book upon one of its angles, or any other suitable object, of 1 lb. to 2 lbs. weight, immediately over the radio-humeral articulation. This articulation in the extended arm is not just under the bend of the elbow, but about two-thirds of an inch in advance of it; and if this little point be not borne in mind an error in performing the experiment is very likely to occur. Supposing, then, the weight has been carefully adjusted, so that its gravitating force shall pass through the articulation, considerable weight will be felt; but if the same weight be placed one or two inches anterior to this point, whereby, from increased leverage, much greater weight ought to be felt, scarcely any will be felt, the same thing happens if it is placed *posterior* to this point; but this is what might be inferred as a natural consequence of diminished leverage. In the former position this objection cannot be urged. If, therefore, weight is felt more intensely over the articulation, than over that part where leverage gives such a decided advantage to the weight, it tenders to the fulcrum against which it rests, what is it but that, at the point over the articulation, the counterpoising forces can be brought into play where their resultant can best have applied, to sentient nerves there distributed, the stimulus their function naturally demands; which function, when thus stimulated, namely, by receiving the diagonal of two antagonizing forces, is manifested in the recognition of weight?

(To be continued.)

ARTICLE IV.—*Case of Pharyngocoele, with notices of similar Lesions of the Pharynx and Oesophagus.* By EVANS REEVES, M.D., London.

A TALL, somewhat emaciated man, æt. 55, by trade a shoemaker, came under my care in August 1854, suffering from dysphagia. The affection had commenced about ten years before, with a sense of uneasiness in the throat, which impelled him to constant efforts to dislodge something which seemed arrested there, but without any further effect than to bring up a little frothy phlegm. This continued for about three years. Then he began to bring up every ten or fifteen days some yellow mucus, at first about a teaspoonful each time, but the quantity increased, as also did the frequency of its occurrence. He continued in this state for about four years, when, after eating some dried currants, the uneasiness in the throat became very severe, accompanied by constant efforts to bring something up, and inability to swallow any solid food. After suffering for nearly a week relief was obtained by bringing up a number of currants, distended and mixed with mucous. From this time food was brought up, at first in small, but later in larger quantities. He was seldom free from uneasiness in the throat, except for a short time after the food had been brought up. He observed, "I can take solid food very well directly after I have got rid of the stuff, but the difficulty soon returns, and on the second or

third day I can only get liquids down, but even these, if the uneasiness is very great, sometimes return." Bougies had been frequently introduced, sometimes they descended with ease, while at others they could not be made to pass. During the last four months, from the increased difficulty, none had been passed.

On examining his neck, by pressing the finger deep under the sterno mastoid muscle, a soft swelling could be felt, extending down from the cricoid cartilage, behind the œsophagus. It was much more prominent on the right than on the left side.

By pressing on both sides of it at the same time, about three tablespoonfuls of pultaceous matters were suddenly ejected into the mouth. Immediately after this he swallowed without difficulty a piece of soft bread of the size of a large hazel-nut. His voice was hoarse; and he had cough with expectoration. The upper part of the larynx was swollen, but the pharynx presented no particular change.

A large sized bougie could be easily introduced, if its point was made to glide down the left side of the pharynx. But if attempted to be passed otherwise, it passed into a sac, and its point could be distinctly felt close to the spine, about two fingers' breadth below the cricoid cartilage. By the use of enemata of strong beef tea—the nutriment taken by the mouth being confined to milk, with cod-liver oil—occasionally mopping the larynx with a solution of nitrate of silver, he has gained flesh, and his state has become more comfortable. He is now able, by pressing on the lower part of the pharynx, on the right side, while swallowing fluids, nearly to prevent the entrance of any into the sac; and as soon as he experiences uneasiness, he can, by pressing on each side of the throat, evacuate the contents of the sac, and obtain immediate relief.

Albers (*Pathol. Anatomia*) considers dilatation of the œsophagus to be a congenital disease. I do not think this the case, for it never once fell under my observation during the two years I attended the Hôpital des Enfants; and all the cases which I have been able to collect from Continental and English sources, show that it is a disease of the prime and decline of life rather than of the earliest period.

In 21 cases the following were the ages and the duration of the disease:—Males present a greater liability to this disease than females; in 19 cases, 3 were females and 16 males. The same preponderance I have observed to exist on the part of the male sex in cancer and simple ulceration of this part of the digestive canal.

Males—aged 54, the disease commenced then; 66, begun when 18; 65, suffered several years; 60, five years before death; 69, three years before death; 77, suffered since youth; 73, suffered twenty years; 24, one year before death; 24, four years; 55, ten years (still living); 38, since boyhood, from a blow; 43, since boyhood, from a blow; 43, fifteen years; 43, ten years; 43, a few years; 43, since early youth.

Females—aged 33, ten years before death; 30, time not stated; (mother of children) time and age not stated.

Situation and Forms.—1st, In the pharynx, where it is met with under three forms: *a*, as *pharyngocele*, the mucous membrane protruding through the fibres of the inferior constrictor muscle of the pharynx; *b*, as a *pouch-like* dilatation of all the coats—the *jabot* of

Vio-d'Azyr; *c*, as a sacular dilatation. All the coats as in the last being affected.—2d, In the œsophagus, where it is met with: *a*, as a *spindliform* dilatation, which appears to be the most frequent, all the coats being affected; *b*, as a *sacciform* dilatation; *c*, as *œsophogoele*, the mucous membrane protruding between the muscular fibres of the canal.

CASE II.—A gentleman¹ had suffered for some time from uneasiness in the throat, with occasional difficulty in swallowing, which was always relieved by the introduction of a bougie. Some ineffectual attempts had now been made to pass one. At last after many attempts by giving the instrument a lateral twist so as to cause the point to glide along the side of the throat, it passed without difficulty. The instrument was introduced once a fortnight up to his death, one year from being first seen. A bag formed entirely by the mucous membrane was found projecting from the posterior and inferior part of the pharynx, and descended between the œsophagus and the spine.

CASE III.—A merchant, æt. 54² (had enjoyed good health up to this time), began to suffer from pain in the throat when swallowing, as if something had stuck there. The pain increased in severity, and at last cough and suffocation set in with regurgitation of the food taken, which he was obliged to remasticate before it would descend. Astringent gargles and other remedial measures were ordered, without benefit. An attempt was made to pass a bougie, but it met with an obstruction, which it was found impossible to surmount. His appetite continued good, and he eat largely, but most of the food taken was almost immediately rejected. After death, a pouch was found at the lower part of the pharynx, three inches and nine lines in length, and one and a half inches in diameter; it descended as low as the 14th ring of the trachea, and contained several ounces of fluid. It opened between the lower fibres of the inferior constrictor of the pharynx, the aperture being somewhat narrower than the opening of the œsophagus. The pouch had externally a cellular coat; in it, near the opening, large muscular fibres were observed; internally, it had a vascular coat, thicker than that of the œsophagus or pharynx, also an epithelial one.

CASE IV.—A man, æt. 66,³ of robust form, entered the hospital in August 1837. When 18 years of age, an enlargement was observed on the right side of the neck, about three fingers' breadth below the jaw. It gradually increased in size, and at last became as large as a pigeon's egg. He had difficulty in swallowing, and felt a constant sense of uneasiness in the upper part of the œsophagus, as if something was lodged there; it was relieved by vomiting up some alimentary matters. Once the sensation lasted three days, relief being obtained by the vomiting up of some indigestible substance taken some days before. When 40 years of age, the difficulty in swallowing greatly increased, and he was obliged to confine himself to fluids, but at last even the passage of these was attended with great difficulty. The vomiting became more frequent, but it was always followed by relief. He was now suffering under pneumonia, and was greatly emaciated. On the right side of the neck an elastic tumour existed of the size of the fist, implicating the right lobe of the thyroid gland. On the left side, another of the size of a pigeon's egg, which subsided on his vomiting up that which he had drunk. The vomiting was easily produced—by lying back and pressing on the tumor. After death, the right lobe of the thyroid gland was found to contain a cyst the

¹ Sir Charles Bell's Surgical Works.

² Kühne Rusts Mag., für die Gesammt Heilkunde, Bd. xxxix.

³ Rokitansky Oestreich Med. Jahrbucher, Bd. xxii.

size of a goose's egg. The pharynx was œdematous; and near its lower part the mucous membrane passed through the fibres of the inferior constrictor muscle, forming a pouch the size of the pharynx itself; the pharynx opened directly into it, instead of into the œsophagus. Some muscular fibres were observed in its walls. The œsophagus was greatly atrophied.

Rudolphi speaks of having met with a case of pharyngocele, but he gives no history of it during life, nor any details of the post-mortem appearances.

Littre (*Collect. Academiæ partiæ Française*) found in a body he examined the pharynx more dilated and its walls thicker than usual; in its inferior part, four cylindriciform sacs existed, opening from above.

Roennow (*Ancien Journal de Med.*, tom. lxxv.) found in a person, who during life had had particularly offensive breath, pouch-like formations containing putrid food.

Samuel Cooper (*Surgical Dictionary*) mentions an instance where a pouch was found in a person who died from dysphagia, filled with apple pips.

Mr H. C. Johnson has recorded, in vol. i. of the *Transactions of the Pathological Society*, the case of a gentleman aged 65, who had for years been unable to swallow but very little food at a time. A considerable quantity of food remained in the œsophagus after each meal, being rejected in the course of an hour or two, in the form of small pellets. He came under Mr Johnson's care for laryngitis, accompanied with dysphagia. He sank under a second attack of laryngitis, pneumonia also existing. In the upper and posterior wall of the œsophagus a pouch existed, having a small valvular opening, on the same level as the cricoid cartilage.

De Guise (*Dissert. sur l'Anévrisme*) observed in a horse the mucus membrane to protrude through the muscular fibres of the upper part of the œsophagus, forming an egg-like sac.

Pouch-like Dilatation of the Pharynx.

CASE I.—A man æt. 60,¹ unable to swallow since the day before he applied to me. Five years ago (1759), while eating cherries, one of the stones lodged in his throat, and was not got rid of until three days after, when it was discharged during a fit of coughing. A sense of soreness remained for some time, and at the end of a year he noticed that from one to two, or even more hours after a meal, a little food was returned unchanged. The quantity returned increased, and the time for its remaining down diminished. He also found that if he drank during a meal, he was in great danger of being suffocated. At last he could not take more than four table spoonfuls of food without its being returned, none entering the stomach: Efforts were made to pass a probang—quicksilver was also given him—but in vain, he died 13 days after being seen. A considerable sized muscular bag was found descending between the œsophagus and the spine—it was continuous with the pharynx, and was also of the same thickness.

CASE II.—Col. D., æt. 69,² three years before death began to suffer from

¹ Ludlow's Medical Observations and Inquiries, vol. vi.

² Mr Worthington, Medico-Chir. Trans., vol. xx.

slight dysphagia, which continued for eighteen months. In the months of January and February 1846, it became more severe, and was accompanied by emaciation. When seen in July solids required the most complete mastication, and the descent of the morsel produced a gulping noise. A fluid like the saliva was constantly secreted, to the amount of 1 or 1½ pints in the 24 hours, occasionally it was mixed with food. A probang was arrested opposite the cricoid cartilage; a slender wax bougie seemed to pass beyond the obstruction, but, from the shape it had assumed, this was doubtful. When food was swallowed, it was retained for a short time, and then, regurgitated, unchanged. At last, no food could be made to enter the stomach, and for the last three weeks of life, he was supported by nutritive injections. A pouch, in shape to that of a finger-stone, 3½ inches in length, descended from the posterior and lower part of the pharynx, down between the spine and the œsophagus. Two-thirds of it was covered by fasciculi from the inferior construction of the pharynx. The pharynx was much dilated; a free communication existed between it and the pouch. A stricture was found immediately behind the cricoid cartilage, formed by a fold of mucous membrane. A large-sized bougie could be passed.

Dr Coley observed, when this case was read before the Society, that he had known a medical man who had suffered from dysphagia for ten years; he sank from inanition and thirst, the deglutition of even fluids being impossible. Mr Cline in this case had discovered the existence of a pouch during life. It was found after death to extend from the lower part of the pharynx, and was three inches in length and one in diameter; a stricture existed at the upper part of the œsophagus. A priest, says Burserius, (*Inst. Med. Pract.*, lib. iv.) died in 1782, in a state of extreme emaciation, in consequence of dysphagia, which had existed since early youth. He could eat, but very slowly, and to facilitate the descent of the food, he was obliged to press his throat with his hand. The difficulty was at first confined to solids, but at last extended to fluids, which were rejected. A dilatation existed at the posterior part of the pharynx, which descended six or seven fingers' breadth between the spine and the œsophagus. The upper part of the œsophagus was narrowed.

Burserius was indebted for the case to Gianella of Rome, who was present at the post-mortem examination.

Marx (*Göttingen Anzeigen*, 1753) reports the case of a man æt. 73, who died after suffering for 20 years from gradually increasing dysphagia. A pouch, five inches in length and three in breadth, descended between the œsophagus and the spine.

In the next case, the pouch was unusually broad:—

CASE III.—A man, æt. 77,¹ had suffered since his youth from difficult deglutition. A year ago, in fixing some dental appliance to his upper jaw, force was used; inflammation of the whole alimentary canal was excited, and for some months he was subject to vomiting immediately after meals. This was followed by increased dysphagia, and he was obliged to confine himself entirely to fluids, which descended with difficulty, and with a kind of gurgling noise, and they were immediately afterward returned to the mouth without effort, mixed with mucus. He often had recourse to friction on the neck to assist the descent of the fluid, and to empty a pouch which he fancied existed; the little fluid which did descend, appeared to do so in a thread-like manner. No tumour could be discovered in the neck, or pain, but a sense of burning existed in the stomach, with severe thirst. No instrument was passed. He at last

¹ Dr Cassan, read before the Academie de Med., *vide* Arch. Gén de Méd., 1826.

sank from inanition. The lower half of the pharynx was dilated into a kind of pouch, formed by its posterior and lateral parts. In front and below the dilatation a narrowing, without thickening, of the œsophagus existed for eight lines of its length, it then insensibly enlarged.

Affolter, Naumann (*Handbuch der Med. Klinik*) observes, he found in a case where the commencement of the œsophagus was the seat of a stricture $1\frac{1}{2}$ inch in length, that the pharynx formed on each side a large sac-like dilatation. Dr Melville, Monro (*Morbid Anatomy of the Stomach and Gullet*) states, met with an instance of dilatation where the patient could retain a pint of fluid for ten minutes, conversing the whole of the time with his friends.

I have in my possession the pharynx taken from the body of a middle-aged, emaciated man, brought into the Clamart for the purpose of being dissected. The pouch will admit the three fore-fingers as far as their second joints. The œsophagus is the seat of a stricture, rather less than $\frac{1}{4}$ th of an inch in length. A large-sized bougie could be easily introduced. I was led to study the best mode of introducing an instrument in this case. It appears that sliding the point of the instrument down the sides of the pharynx will not in every instance succeed; but if it is passed along the posterior part of the larynx, failure is scarcely possible.

Spindliform Dilatation of the Œsophagus.

CASE I.—A man, æt. 24;¹ health good up to a year ago, when a sensation was experienced in the centre of the sternum, as if an obstacle existed to the descent of the morsel. The obstruction increased to such an extent that solids would not pass unless large quantities of fluid were taken to wash them down. Regurgitation followed, and then vomiting of all taken, whether solid or liquid. After some irregularities in diet, the affection made great progress, mucous and blood being discharged. Hectic set in, and he sank in a month. For the last three weeks of life he was supported by nourishing enemata. The œsophagus immediately below the pharynx began to dilate. This continued as low as the stomach. Its centre, the most dilated part, was 4 inches in diameter. It contained 2 pints of thick liquid, mixed with mucous and blood. The mucous membrane was ulcerated. The cardia was converted into a hard narrow ring.

CASE II.—A man, æt. 24,² had suffered for four years from vomiting, which was particularly liable to recur after eating indigestible food. His present illness arose from a beer debauch. He sank thirteen days after entering the hospital from diarrhoea. The œsophagus opened into the stomach like the os uteri. Above this, as high as the pharynx, it was largely dilated; near the centre so much so as to admit with ease the arm of a middle-sized man. Its coats were hypertrophied; and from the upper third down to within an inch of the cardia, ulcerations, varying in size from a linseed to a fourpence-piece, existed.

CASE III.—A female,³ the mother of children, became, after gastric fever, subject to dysphagia, with oppression and difficulty of breathing; and at the end of a year vomiting was added. A small quantity only of the food swallowed seemed to pass into the stomach, giving rise to convulsive efforts, which

¹ Dr Lindlaw. Caspar's Wochenschrift. 1840.

² Rokitsansky Oestreich, Med. Jahrb. Band xii.

³ Della Chiaga, Il Progresso. Napol. 1840.

were only relieved by vomiting up that which had been taken. She at last sank. The walls of the œsophagus were found hypertrophied, the muscular coat greatly developed, the mucus membrane red and thick. It was dilated to eight times its usual diameter. Its lower part was of the natural size.

CASE IV.—A man, æt. 38, strong and muscular, experienced on taking food a sensation as if it was arrested a little above the stomach, giving rise to the most painful and protracted efforts to swallow.¹ The kind of food taken made but little difference. Immediate relief always followed vomiting. After a debauch, to which he was much addicted, the vomiting occurred spontaneously, and fluids then gave rise to the painful sensations before confined to solids. A probang was passed without difficulty. The difficulty in swallowing had existed since childhood, and was attributed to a blow from a club. Of late he had suffered from difficulty of breathing, particularly on exertion. He was found dead after having eaten and drank to excess. The brain and lungs were found gorged with blood, and the œsophagus was found greatly distended with food, and largely dilated. The dilatation commenced immediately it entered the chest, gradually increasing until it reached its centre, it then diminished until it passed through the diaphragm. When distended it measured 6 inches in diameter. Its walls were thick and strong like leather. No trace of muscular tissue could be observed.

CASE V.—A blacksmith, æt. 43,² had, when a lad, received a severe blow on the sternum. It was almost immediately afterwards followed by difficulty in swallowing. For the last 20 years he has suffered severely from attacks of dysphagia, sometimes for three weeks no food entering the stomach. Occasionally he has, however, by great force, been able to propel food into the stomach. Of late he has allowed the food to remain for hours, sometimes days, to become somewhat digested before he attempted to do this. No food would pass into the stomach until the œsophagus was quite full. After death the œsophagus was found capable of containing 2 quarts of fluid; about 2 inches of it above the cardia was contracted.

Mr Mayo (*Outlines of Pathology*) found in a female, æt. 33, who died after suffering 10 years, that the œsophagus began to dilate immediately below the pharynx, at 4 inches above the cardia it was $2\frac{1}{2}$ inches broad, it then narrowed to its natural dimensions.

Sacciform Dilatation.—I opened the body of a man, Portal (*Anatomie Médicale*) observes, in whom the œsophagus formed a dilatation much larger than the stomach, which last was only the size of that of a child 2 years of age. The cardiac orifice was so narrowed that a quill would not pass.

Dumas (*Recueil Periodique*) also found the lower part of the œsophagus dilated into a kind of second stomach.

Blasius (*Adversar. Medica Obs. ix.*) found in the body of a man the lower part of the œsophagus dilated into a kind of second stomach; below this the canal was narrowed.

Dr Huss of Stockholm found in the body of a female, æt. 43, a large dilatation of the lower part of the left side of the œsophagus; the canal above this as high as the pharynx was also dilated.

Grashuis (*Acta Nat. Curios. lib. vi.*) mentions the case of a single female, æt. 31, who, after a troublesome catarrhal affection, began

¹ Dr Hannay, Edin. Med. and Surg. Jour., vol. xl.

² Dr Purton, Medical and Physical Journal, 1821.

to suffer from slight pain in the chest and in the back, under the left scapula, at last pain in the left side of the chest was also added, severe, but occurring only after eating, and gradually subsiding at times; it was accompanied by great anxiety and oppression of breath. In June of the same year (1733) fulness was observed in the left side of the chest. By the end of August great difficulty in swallowing, both liquids and solids, was experienced. She was seen by Boerrhaave and by Schuck. Regurgitation of food in from 12 to more hours after it was taken set in, mixed with large quantities of viscid tenacious mucus. After this had taken place the passage of food into the stomach became free for a short time. Emaciation set in; she had night fever, and cough, with mucus expectoration streaked with blood. She sunk from disease of the chest and the loss of a little blood, on the 8th of July. On raising the left lung a preternatural enlargement of the œsophagus was found in the centre of the chest, which, when laid open, was found to contain blood and ichorous matter; some hard fleshy tubercles existed on its internal wall, and on one side a dilatation capable of holding a goose's egg, communicating with the canal by an opening capable of admitting the finger.

Œsophogocœle.—The following is, I believe, the only instance recorded. A person, observes De Guise (*Dissert. sur l'Anévrysme*) had suffered for 15 years from regurgitation of food after eating; at last deglutition became very difficult, and death from inanition ensued. A membranous sac was found at the side of the œsophagus, which, from its position when distended with food, prevented the morsel from entering the stomach.

ARTICLE V.—*Case of Chronic Cerebral Abscess.* By JAMES YOUNG, M.D., Edinburgh.

I WAS first summoned to see Mr A. B. on the 22d of February 1854, when I found him labouring under general debility, with a little occasional pain of chest, with severe cough, and dark-coloured expectoration, which had been of long standing, his attention being mainly directed to the idea that he was labouring under a syphilitic taint of the system. On inquiry, I found that about ten years before he had contracted syphilis; but on examination I ascertained that he now enjoyed perfect immunity from this disease, of which fact many medical men had previously tried to persuade him, but without effect. He persisted in swallowing mercury contrary to my most rigorous injunctions, thinking that by this, he would overcome the supposed "impurity of blood"—an idea he had got from *Davies'* book on *Syphilis*. I endeavoured to direct his attention to the state of the chest, which appeared to be the main seat of his disease; all, however, proved ineffectual, and I accordingly advised him to consult Dr Christison, which he did, the result being a confirmation of my opinion of his case, that the system was free from syphilitic taint. Dr C. prescribed pills of the ammoniated citrate of iron, and the fluid extract of teraxicum, along with a little laxative medicine; by the continual use of which for some weeks, his health was greatly improved, and continued so till the end of September, when he only still complained of a little

occasional pain in the chest, although the cough and expectoration continued. He rarely perspired at night; his appetite was good, and bowels generally regular, some simple laxative medicine being used when required. His habits, latterly, had been very temperate, although in former years it seems he occasionally indulged in wine, etc.

About the beginning of October 1854, an aggravation of his cough induced him to send for me early in the morning, but, by and by, he once more enjoyed his usual good health, and was able to drive out every fine day.

This improvement was only temporary, for in the morning of the 20th of October, he was suddenly seized with severe tremulous motion of the right leg, which continued for six or eight minutes. The attacks continued to recur for three days, at shorter intervals, and with great severity, when the arm of the same side became affected, and was soon followed by a sense of numbness over the right half of the face, with difficult articulation in speaking. The hemiplegia continued to increase down to the 31st of the month, when it might be said to be complete. Mr A. B., after this period, was of necessity confined to the horizontal posture, having lost all sense of feeling, and power of motion of the right side. A professional friend in the neighbourhood had visited him with me, but without any beneficial result. The pulse at this period was firm, but with a very perceptible difference between that of the right and left arms—the latter being stronger.

On the 5th of November, his speech was totally lost, the pupils refused to contract on the application of a lighted candle. Fæces and urine were voided involuntarily; he lost all power of memory, and was incapable of recognising surrounding objects. On the 6th and 7th of November the pulse sank to forty-five, on the succeeding day was hardly perceptible, and he died on that day comatose. It is unnecessary minutely to detail our course of treatment. At the commencement of the disease small doses of the tartrate of antimony were given; at a later period blisters were applied to the neck, and strychnia to the blistered surface, but without effect; gentle laxatives were given, and latterly stimulating enemata were administered. The head was shaved, and cold cloths constantly applied, but the patient invariably removed them with his left hand, and seemed rather inclined for warmth. This fact was frequently remarked to me by the nurse in waiting. Never, from all the history of this case, or during my attendance, did Mr A. B. ever complain of his head, and, indeed, I never could discover any symptom of an acute affection of the brain.

A *post-mortem* examination took place thirty hours after death. We found the left lung large and healthy, while the right was very much condensed and atrophied, dark and closely adhering to the posterior aspect of the chest; it only weighed eleven ounces; there was no tubercular disease, but we found a large quantity of bloody serum in the cavity of that side of the chest. On removing the calvarium, and incising the dura mater, pus flowed out, and after its removal from the right half of the cerebrum, pus was observed between all the convolutions; and on dissecting the brain from above downwards, we discovered a *large chronic abscess* immediately above the right ventricle, out of which flowed *three* ounces of green putrid pus, mixed with bloody matter; the cavity had the appearance of a pouch with a firm rough or villous looking lining membrane. There was no appearance of redness or ramollissement of the brain; in the neighbourhood of the abscess the membranes were quite healthy, but some of the cerebral veins on the surface were gorged with dark blood.

It is my opinion that this abscess must have existed in the brain for several months, and had only burst into the membranes some days prior to death. It affords a well marked instance of how capable the brain is of accommodating itself to disease of the most formidable character, so long as that disease is produced slowly.

The absence of vomiting during the formation of such an abscess is also worthy of observation. The facts arrived at by the *post-mortem* examination were carefully written down at the time, in the presence of my friend, Mr Todd, surgeon, of Colinsburgh.

Note by the Editor.

The really important fact in this case appears to us to be that the hemiplegia and the lesion supposed to occasion it, occur on the same side of the body. Instances of this occurrence are few; and it may well be doubted whether disease of the cerebral lobes ever causes paralysis directly—that is, without crossed action. Numerous instances have occurred of cerebral abscesses, or softening, having been found after death, in which there was no paralysis during life; and a still greater number are on record, in which there was well-marked paralysis during life, but no appreciable change found in the brain after death. It is by no means improbable, therefore, as paralysis may be induced without leaving any traces, that in the few exceptional cases to the law of crossed action, it was caused by unknown changes in the opposite hemisphere of the brain, and as is sometimes the case, that the lesion found in the hemisphere of the paralysed side had produced no symptoms whatever. At all events, we consider this to be a more probable explanation of these exceptional cases than would be the assumption that in them, and them only, the cerebral lobes exerted a direct rather than a crossed action.

Part Second.

REVIEWS.

The Principles of Physiology, by JOHN AUGUSTUS UNZER; and a *Dissertation on the Functions of the Nervous System*, by GEORGE PROCHASKA. Translated and Edited by THOMAS LAYCOCK, M.D., (Göttingen), Physician to the York Dispensary, etc. London: Printed for the Sydenham Society. 1851. 8vo, pp. 463.

The Brain in Relation to the Mind. By JOSEPH SWAN. London, 1854. 8vo, pp. 113.

Clinical Lectures on Paralysis, Disease of the Brain and other affections of the Nervous System. By ROBERT BENTLEY TODD, M.D., F.R.S., Physician to King's College Hospital. London, 1854. Small 8vo, pp. 462.

Epilepsy and other Affections of the Nervous System which are marked by Tremor, Convulsion, or Spasm; their Pathology and Treatment. By CHARLES BLAND RADCLIFFE, M.D., Assistant Physician to the Westminster Hospital, etc. London, 1854. 8vo, pp. 144.

Eutherapeia : or, an Examination of the Principles of Medical Science, with Researches in the Nervous System. By ROBERT GARNER, Surgeon to the North Staffordshire Infirmary, etc. London, 1855. 8vo, pp. 282.

On the Pathology of Delirium Tremens ; and its Treatment without Stimulants or Opiates. By ALEXANDER PEDDIE, M.D., Edin. Edinburgh, 1854. 8vo, pp. 51.

It is impossible to review our knowledge of the physiology and pathology of the nervous system, without recognising three great epochs or eras in discovery, which are inseparably connected with the names of three distinguished men. The first of these epochs is characterised by the establishment of contractility and sensibility as inherent properties of the muscular and nervous tissues. Such was the great work of Haller. The second is indicated by the demonstration of motor and sensitive columns in the spinal cord, and by the existence of nerves of sensation, of nerves of motion, and of mixed nerves in connection with these columns. Such was the doctrine established by Charles Bell. The third epoch is marked by the separation of numerous combined actions, from sensation, volition, and contractile movements ; the demonstration that the spinal cord was their centre, and the fact that it was endowed with a reflex function acting through a series of incident and excident nerves, which were named. Such were the views introduced and successfully maintained by Marshall Hall.

Each of these great doctrines has given rise to an astonishing amount of discussion, mingled with no small degree of acrimony. The controversy between Haller and Whytt, on the doctrine of inherent irritability and sensibility may still be considered as one of the most important and famous to be found in the whole history of medicine. Sir Charles Bell's life was embittered by the necessity of combating the claims of Dr Walker, who first conceived the idea of distinct function, in the anterior and posterior columns of the spinal cord, and of Magendie, who maintained he had first demonstrated it experimentally. Lastly, Dr Marshall Hall has been accused first of having merely given to the sympathetic actions of Whytt a new name, and secondly of having borrowed all his ideas from Unzer and Prochaska.

It would occupy far too much time to enter into the history of these discussions. The two former have ceased to excite attention. The latter was almost entirely carried on by the *British and Foreign Medical Review*, a journal which, under the editorship of Sir John Forbes, from first to last attacked Dr Hall, with a pertinacity truly remarkable, and raked up incomprehensible passages from Unzer and Prochaska, in order to diminish his just title to the establishment of the reflex junction of the spinal cord, in a way anything but

creditable to the medical literature of this country. The controversy, however, led to the translation of the works of Unzer and Prochaska into English, for the Sydenham Society, and has thereby enriched our literature. The result seems to have been that all these attacks have ceased, and that the merits of Marshall Hall with regard to the important theory of reflex nervous actions, is now universally acknowledged.

The experiments illustrative of reflex action are in their nature the same as those performed by Le Gallois and Sir Gilbert Blane, and the same results were derived from them as were observed by Flourens, Rolando, Hertwig, and many others, who found that after removing the brain from animals, they could walk or fly. It is the inference derived from these experiments which we regard as important, and the demonstration that such actions are independent of sensation and volition, and strictly connected with integrity of the spinal cord. Now, this was not clearly stated by Unzer or Prochaska, for whom priority in this matter has been claimed. The latter physiologist, after enumerating the different seats given to the common sensory by his predecessors, all of whom understood by it the seat of sensation, distinctly says, "*The sensorium commune*, properly so called, seems not improbably to extend through the medulla oblongata, the crura of the cerebrum and cerebellum, also part of the thalami optici, and the whole of the medulla spinalis; in a word, it is co-extensive with the origin of the nerves" (p. 430). It is true he distinctly says that reflexions of sensorial impressions may take place, either with consciousness or without consciousness, but then he proceeds to confound together, convulsions in apoplectics and other motions truly spinal with the movements of the heart, stomach, and intestines, p. 432. Unzer taught that the ganglia reflected motions without going to the *sensorium commune*, and that they were special sensoria.

A careful analysis of the writings of Unzer and Prochaska should be made conjointly with a critical study of the works of Haller, and then we think it will be evident that great confusion has been thrown over the whole subject by the use of the term *sensorium commune*, which, since the days of Willis, has been considered as the seat of sensation. It is easy now when the subject has been unravelled, and the nature of sensation understood—at all events in its relation to consciousness—to maintain that Prochaska meant something different by *sensorium commune*, from what Haller did. We nowhere see any evidence of this, and the proof that so essential a point was not made clear in connection with reflex functions, is, that all his contemporaries as well as physiologists since his day, never understood it in this sense until Marshall Hall wrote.

The translation so elegantly made for the Sydenham Society by Dr Laycock, we regard as a wonderful specimen of patience and learning. We ourselves commenced the perusal of Unzer when his works were first mentioned in connection with reflex actions,

but of all the crabbed specimens of German and Germanic names we ever encountered it was certainly the worst. We could make nothing of it, and at once banished from our ideas the possibility of Dr Hall having understood it. In fact, we are told in the introduction, that according to Professor Marx of Göttingen, the antiquated style and singular phrases of the work, render it somewhat difficult for even the modern German physicians to comprehend. Dr Laycock himself has been obliged to draw up a synopsis of the German words and phrases used by Ünzer, and their English synonymes, by which we only hope he has arrived at the true meaning of the author. Assuredly if he has not, no other living man ever will.

Since the doctrine of reflex functions has been introduced, we do not observe that any very marked progress has been made in our knowledge of the pathology or treatment of nervous diseases. The great function of innervation, composed of mental, sensory, and motor phenomena, has only in a very trifling degree been elucidated by that histological research which, of late years, has thrown such a flood of light on the important processes of nutrition and development. The minute changes which take place in nervous matter during inflammation, ramollissement, suppuration, tuberculization or other alterations of its substance, have been shown to be strictly analogous to what takes place in other organs. There is nothing special or peculiar in the nature of nervous diseases themselves. The symptoms or phenomena they occasion are to be attributed to an increase, perversion, or diminution of nervous function, in the same manner that similar morbid changes produce exaltation or destruction of functions in other organs. The same diseases, therefore, affect brain or nerve, as attack lung or bone, the differences produced being dependent, not so much on the nature of the lesion, as on the derangement of those physiological laws which influence special textures and organs. Hence the only key which is capable of unlocking the secrets of nervous disorder, is a knowledge of the healthy functions of the brain, spinal cord, and nerves—in one word, physiology. Although this truth may, in one sense, be said to apply to our knowledge of every malady, there can be little doubt that it especially does so in cases of nervous derangement.

With regard to mental phenomena and their connection with the brain, we think there is still something to be accomplished. When we reflect upon the nature of the minute and delicate texture of this organ, the tubular and vesicular structure of which is only to be seen by employing a high magnifying power, and then consider that if any morbid lesion be present it must affect these structures long before it becomes visible to naked sight, it will be evident that the pathology of insanity cannot yet be said to be on a level with that of many other disorders. Notwithstanding the thousands of cases which have died in our asylums and large establishments for the treatment of the insane, since histology has been cultivated

by our younger physicians, we are not aware of the existence of any well recorded series of observations bearing upon this subject. Yet how is it possible to determine whether insanity depends upon organic or functional disorder of the brain, unless such investigation be made? We know of no inquiry which is likely to be more serviceable to the science of medicine, or to humanity, than would be one into the histology of insanity, even should it terminate in negative results, which, however, is by no means likely. The task, however, to be productive of good, must be undertaken after the most careful preparation, and after considerable experience has been acquired in the manipulation of the microscope, and in the investigation of nervous structure in its healthy and ordinary diseased conditions. In addition to a knowledge of the physiological and pathological histology of the brain and its membranes, such opportunities will be required as seldom present themselves to any but the superintendents and medical officers of insane establishments, to whom, indeed, alone the profession can be expected to look as the parties best qualified for carrying out such an inquiry. But if prosecuted with an earnest and single desire to arrive at truth, in the manner we have pointed out, we are satisfied that a few years would enable a talented investigator to arrive at some very important facts. In the meantime let us endeavour to ascertain what information may be obtained from the recent works on the physiology, pathology, and diseases of the nervous system enumerated at the head of this article.

Mr Swan, who has paid, perhaps, as much attention to the anatomy of the nervous system as any man living, favours us in this monograph on "the brain in relation to the mind," with a series of reflections and speculations, many of which are highly ingenious, and some suggestive of further thought. We do not find any attempts at observation or research, but rather various ideas, several of which, however, strike us as valuable, though it cannot be said that they are elaborately worked out, so as to carry conviction. In Chapter IV., for instance, the distinction between the acts of mind in copying a drawing and in designing one; and the hypothesis to which the author is lead explanatory of the mathematical figures of the cells in a honeycomb, are worth attention.

"If an engraver writes on a copper or steel plate ever so beautifully, he performs a mere mechanical art; if he copies an elaborate drawing, that gives some similarity of the objects contained in it, he does very little more than in the writing, for he has reduced the size of the original by rules and lines, and followed the previously-traced marks on the copper plate; but if he gives the true character to the objects, such as the expression of the countenances, the correct attitudes, and other properties, he shows his mental capacities and the mental qualities of the drawing at the same time; but his accompaniments are not equal to those of the original designer.

"There may be some difficulty in comprehending how the image of any thing is transferred to the brain, and from thence to paper or canvass, or to be moulded or engraved; but a little consideration will render the whole process

capable of explanation. The image enters the eye in a very diminutive form, but does not abide there, or in the optic nerve or the sensitive tract. When the image is about to be permanently or completely fixed in the brain and mind of the artist, the features are examined with great care, and then, or after other interviews, the image is transferred by the optic nerve to the sensory, where, through the vital agency of the voluntary tract, it becomes fixed on the mind, and thus formed is continued to the muscles of the hand and the pencil. The image thus formed in the brain is very diminutive; but through the connection of the voluntary nerves with those that impressed it on the mind, it can be made to extend to its just proportions by a larger or smaller sweep of the arm and hand, according to the required magnitude of the picture.

"In making the cells by the bee and wasp, there is nearly the same process as in drawing and modelling, the hexagonal facets of the compound eyes carrying the precise shape of the impulse to the voluntary tract in the brain, for directing the required action in constructing the hexagonal form of cells. The bee and wasp have been considered as possessing correct mathematical ideas in making their cells hexagonal; but their compound eyes are divided by hexagonal marks; and as the motions of the muscles of animals are directed very much by the mode of admission of light, the shape of the cells may be in accordance with that of the surface of the eyes. In all imitations of objects, the muscles take a form of action from an organ of sense—the pattern is received by the eye, and thence conveyed to the brain, and having produced in this a precise impression of its form, the action of the muscles is modelled so as to continue its representation. The images of external objects always falling on the brain of the bee or wasp through the hexagonal divisions of the surface of the eye in viewing near objects, produce such habitual motions of the muscles as constitute unconsciously the hexagonal form of cells; and thus one of the faculties of instinct is accounted for, and the cause of the mathematical exactness explained.

"If a net, whose meshes are of a particular form, obstruct the view of a part of the image to be delineated in a picture, it would appear numerous intersected by it. If the threads of the meshes be so fine, and the distance to the image sufficiently great, the threads are not seen, but only a dulness of colouring. If, however, the image is near enough, the threads must appear; and, if thick enough, divide it into numerous pieces. When a bee or other insect is flying, the divisions forming the hexagonal facets of the compound eyes may not therefore obstruct the general view of large objects; but whether they are thus seen, as perfect objects are through the plain cornea, is another question. When they come to work by the hexagonal eyes, which they must do very near to the cells, the light must pass through the eyes so as to give the precise hexagonal form to the objects on which they are working, and in this shape convey it to the brain, and thence to the muscles employed in the operation. Sometimes the cells of the honeycomb are not of the precise hexagonal shape: this deviation may arise from the obstruction caused by the numerous hairs placed about the eyes, or from some failure in the form of the facets, as it is not unusual to find, after making preparations of the eyes of insects, that some of the facets form squares and not hexagons."—Pp. 27-31.

This idea is reproduced again in page 102, where it is pointed out that birds make various kinds of nests, but that when they have a decided form, it is round like their eyes. We must confess, however, that if the hypothesis of the construction of hexagonal cells by insects be deemed satisfactory, on account of the multiple hexagonal lenses of their eyes, the round form of birds' nests, because their eyes are round, is a little far fetched, and, if carried out, would plunge the author into many difficulties. Still, the general idea maintained by Mr Swan, that many acts presumed to depend on instinct have precise mechanical causes, which are referable to the structure or

functional activity of certain organs of the body, is one which merits attention.

The following passages from Chapter V. deserve notice:—

"It seems that all the fibres of the human brain must undergo some change for bringing them to a proper condition for activity and steadiness, and this is the reason why all manual arts should be begun early, and even intellectual pursuits, as they are with so much difficulty allowed to come to perfection in mature age. Every fresh impulse requires one or more fresh fibres of the voluntary tract for its perfect reception, and thus so large a brain is necessary for the almost unbounded extent of the powers of the intellect of man; therefore, however hard he may work, and however much knowledge he may acquire, there is always room left for further stocks of information. He can occupy as much of his brain, however, as he pleases, and according to the degree of his industry, or idleness, will be his intellectual progress, so that the extent of his attainments rests with himself. By the necessity for such use and proceedings, he becomes possessed of a deliberative power of acting, and of using his muscles, and thus gains the form and character of his hand-writing, which he retains to the end of life, whether it be good or bad; the one has been perfected by care and attention, the other by slothfulness and negligence, unless bad instruction or some disorder has prevented the proper development of his powers. In the same way artists form and retain good or bad methods of drawing, and in which artificers excel or not, in their several branches."

"A large brain is generally considered to be favourable to the extension of the intellectual faculties, but a small brain has also been found capable of promoting high attainments. The difference may depend on the quality of the fibres; but the small brain, without any difference of composition, may, through assiduous study, have been sufficient for allowing a great extent of knowledge, and as much as is usually reached by a larger brain, which is seldom exercised to its utmost limits. It is difficult to determine how small a brain is capable of as much intellectual power as will not be considered low enough for constituting an imbecile person or an idiot, until attempts at instruction have been prosecuted extensively enough for determining its capability of ministering to intellectual functions."

"In early youth, the brain is not only the most susceptible of impression, but the most capable of retaining it. As life advances, the brain varies in respect of its powers, and loses much of its susceptibility of imprinting letters and words so that their memory may be permanent. It is then maturity of thought and judgment exercise the greatest power, and allow the mind to receive the highest qualities of information. If the two seasons are reversed, the advantages of either are gained with great difficulty and uncertainty. It is most probable that unless a person is gradually educated from his youth, he will not attain to excellence either in learning or the arts; and hence the great difficulty in changing his position and leaving off an accustomed business, which depended on manual dexterity."—Pp. 40-46.

The notion that the multitudinous fibres or tubes of the brain constitute a sort of magazine, through which the mind may operate at will—that a vacant mind is one in which these are little used, whereas an active one calls a greater number into employment, seems to us supported by certain facts in pathology. There are many instances where large abscesses or softenings have occurred among the conducting fibres of the brain and spinal cord, which have apparently destroyed them, and yet where, notwithstanding, the conduction of sensitive and motor influences has been uninterrupted. In these cases, it has always appeared to us certain that some fibres, however few, must have remained intact, and carried

on their functions. At all events, such a conclusion has always appeared to us more legitimate than the supposition that nervous influence can leap over disorganised tissue, or find its way to and from the brain by other channels than the nervous substance.

We have, however, given a sufficient idea of the character of Mr Swan's monograph, which we can recommend to the thoughtful reader, as one fruitful in suggestive and ingenious views.

Dr Todd, also, is well known for the great attention he has paid to the physiology, pathology, and diseases of the nervous system. In the volume of *Clinical Lectures* now before us, he continues a subject concerning which, at various times, he has published much. It embraces numerous cases of paralysis, and of other diseases of the nervous system, with remarks, to some of which we may draw attention. In Lecture I., we find the following paragraph :—

"I would say that the centre of volition is of very great extent : it reaches from the corpora striata in the brain down the entire length of the anterior horns of the grey matter of the spinal cord, and includes the locus niger in the crus cerebri, and much of the vesicular matter of the mesocephale and of the medulla oblongata. Disease of any part of this centre is capable of producing paralysis ; but as the intracranial portion of it exercises the greatest and most extended influence in the production of voluntary movements, so disease of this portion gives rise to the most extended and complete paralysis."—Pp. 4, 5.

We are perfectly aware of Dr Todd's opinions as to the seat of volition, and take this opportunity of saying that we cannot agree with him. Surely volition is a purely mental act, and to place it, as some do, in the ganglia at the base of the brain, instead of in the hemispherical ganglion of the convolutions, is opposed to the most obvious facts. We think a distinction should be made between volition and the motor centre for combined actions, which latter is, perhaps, what Dr Todd means. But when he speaks of the centre of volition, that is, of the power to will irrespective of the spinal cord and nerves, through which the influence of that will operates, as being seated in the corpora striata, we think a purely cerebral is confounded with a purely spinal function. It has uniformly resulted from experiment that the power of spontaneous—that is, voluntary movement—was lost when the cerebral lobes were removed on a level with the corpus callosum. In such cases, surely the true centre of volition was absent, and if so, must have existed above the corpora striata. Again, many cases are on record, and we have seen some such, where the corpora striata and base of the brain were greatly diseased with paralysis, not from absence of volition, but from its communication with the parts below being cut off.

The following constitutes Dr Todd's directions with regard to bleeding in apoplexy :—

... "The treatment of patients with apoplectic symptoms must not be regarded as a matter of routine, but as a question of grave import, and which demands the most anxious consideration of the practitioner. Let me add, that it sometimes requires the exercise of no small courage and self-possession to

resist adopting that practice ; for the popular feeling, led by a formerly too prevalent medical practice, is entirely in favour of it, and would readily condemn a practitioner as guilty of the death of his patient who suffered him to die unbled. It is a far more dashing and courageous thing to open a vein on the spot, and in the presence of a number of anxious friends, than to adopt less showy, and apparently less active, measures.

"But, indeed, you need not be inactive, even if you decide against adopting the plan of bleeding. Having placed your patient in an easy position, in which no excitement of muscular action is likely to take place (for you must bear in mind that reflex actions may often be readily excited in these apoplectic cases), you should immediately direct your attention to the state of the stomach and intestinal canal. Sometimes in these cases the stomach is overloaded, or the bowels are confined, and the administration of a quickly acting emetic, or even of some purgative medicine will often provoke a moderate sickness, which unloads the former. Nor can there be any objection to adopting measures to clear out the bowels, either by an active purgative administered by the mouth, such as calomel or croton oil, or by a stimulating and purgative enema, or both.

"If upon full inquiry into all the particulars of the case, you find that your patient is of full plethoric habit, with too much blood in his body, and with a sufficiently strong heart, you may bleed him with every chance of success ; but if he has been of intemperate habits, is labouring under organic disease of the heart and arteries, is of gouty or rheumatic constitution, then, whatever popular or medical custom may say, my advice to you is, hesitate much before you deplete by bleeding.

"The objects which it is proposed to gain by bleeding, are, a diminution of the cerebral congestion, and the stoppage of the hæmorrhage into the brain ; and where it is quite clear that cerebral congestion does exist, and that that congestion causes the cerebral hæmorrhage, this is clearly a rational practice. But you must bear in mind that in a large number of the cases—probably the majority—there is in reality no cerebral congestion, and that the hæmorrhage is of a kind not likely to be stopped by taking away blood—by establishing another hæmorrhage elsewhere.

"On the whole, then, I think that the results of experience denote that the majority of cases of apoplexy are best treated by purging, shaving the head and keeping it cool—perhaps blistering, and that bleeding is rarely applicable, except to the young, vigorous, strong, and plethoric."—Pp. 118-120.

In these judicious views we fully concur. If bleeding in apoplexy, as in pneumonia, be found more injurious than beneficial, in what disease shall that much abused practice be employed ?

The following passage contains Dr Todd's views with regard to a very common form of cerebral softening :—

"In looking into the pathology of this condition of the minute blood-vessels, you may fairly ask, is it, indeed, the primary and essential disease ? or is it not rather an effect of a depraved nutrition of the tissues to which these canals are the carriers of nutriment ? Now, a little reflection would lead you to say that both these questions may be answered in the affirmative, and the evil of which we speak may either commence in the brain-tissue, the brain-fibre, or the brain-cell, which consequently cease to draw upon the blood-vessels to their wonted extent ; and the blood-vessels, therefore, deliver to them their nutrient matter in diminished quantity ; their activity is, consequently, proportionably diminished, and an atrophic state ensues. Or the evil may commence in the blood-vessels ; and this will probably be the favourite view. We know that the larger vessels are the seat of numerous atheromatous deposits, and not only the cerebral vessels but the aorta itself, and the radial arteries, as we can often learn by feeling the pulse in the living individual. Why should

not this atheromatous state extend to the minutest vessels? I do not express any strong opinion in favour of either of these views; but I would say that it seems to me that pathologists do not enough regard the tissue itself as being the starting point of morbid change; and are too apt to overlook the fact that the power of attraction of the tissue for certain elements of the blood, is not the least important agent in determining the greater or less flow of blood to the organ or tissue.

"And, indeed, whichever of these two views we adopt, who will say that in the blood itself we do not find the chief source of evil, which may tell either upon the blood-vessels or upon the elements of the tissues?"

"Without waiting to decide upon this point, we may, then, lay it down that, whether in consequence of disease of the blood-vessels or otherwise, the nutrition of the brain becomes impaired, and this shows itself mainly in altered consistence of the nervous matter. Its colour does not undergo any appreciable alteration, and if you look at a portion through the microscope you will not detect any obvious change, the vesicles remain the same, and the fibres remain unchanged; but, as I said, the consistence is diminished, instead of being firm, the tissue of the organ has become soft and pulpy, and in some cases almost diffuent and of the consistence of cream. The diseased blood-vessels lie in the midst of this pulpy mass for some time without undergoing any further change, but sooner or later under some mental emotion, or during some increased heart's action depending either upon mental emotion, upon derangement of the digestive organs, some bodily exertion, or increased mental effort of any kind, the blood is sent with undue force, or in unusual quantity into the vessels, and in consequence the vascular canals in the pulpy portion of the cerebral tissue being deprived of their usual support, give way, and blood is effused into the softened part of the brain, which it breaks up, and the more readily in consequence of its already diminished consistence. This is the *rationale* of the development of many an attack of apoplexy, from which the patient may or may not recover, according to the extent of brain previously softened, and according to the amount of blood effused."—Pp. 127-130.

This condition of the brain, Dr Todd says (p. 132), is called white softening, and is essentially an atrophic condition. After some time it exhibits not only diminished consistence, but also the development of a peculiar series of large cells filled with fatty matter, which he thinks may be interpreted as abortive attempts at new fibres, or products of some change in the secondary destructive or assimilative process. Still farther on, we are told that white softening follows the restoration and diminution of the cerebral circulation by diseased arteries, or its complete stoppage by a plugged artery, and that this is as well proved as any fact in pathology (p. 177). Further, Dr Todd speaks of white softening of the corpus striatum (p. 180), where, certainly, we have never seen such a phenomenon. Indeed, by white softening we have always understood softenings of the white substance of the brain, or where the cerebral softening is white in colour. In the grey matter, it is usually grey or fawn coloured. In short, white softening merely expresses the tint, and, in our opinion, has nothing to do with the nature of the lesion. Unless this distinction be clearly kept in view, great confusion will necessarily be introduced into our descriptions of cerebral softenings. As far as our observations have gone, the true white softening, such, for instance, as occurs in the central portions

of the brain in a case of hydrocephalus, very rarely presents those fatty granular cells to which Dr Todd has alluded, whilst grey or fawn coloured softenings are rarely free from them.

We have seen from the passages quoted, and the same idea is frequently expressed in other parts of the work, that Dr Todd believes this softening with granular cells to originate in a fatty degeneration of the blood-vessels. In other words, that a faulty nutrition of the vessels of the brain is the *point de depart* of the morbid process. These vessels, he thinks, become fatty, their coats are weakened, and the cerebral substance around them loses its consistency by an atrophic process, the nature of which, however, is not explained. Palsy may now happen in two ways,—1st, by rupture or rapid deliquescence of fibres so softened. This melting down of a portion of the nerve fibres (promoted possibly by the passage through them of the nervous force, just as the platina wire will be consumed under the transit of the galvanic current), will produce a solution of continuity and stop the propagation of the nervous force. Such a view as this, Dr Todd thinks, serves to explain the recovery of the palsy on the supposition of the restoration of the normal nutrition of the nerve fibres, and the reunion of those which had given way (p. 178).—2d, By hemorrhage tearing across some fibres and compressing others.

Now, there are many points in the pathology of cerebral softening, as here given by Dr Todd, which we think too hypothetical, and which, it seems to us, admit of explanation, in a way more consonant with the facts observed. Here we may observe that it is much to be regretted that so many of Dr Todd's valuable cases are very defective with regard to a minute examination of the nervous texture after death. Some cerebral softenings are undoubtedly purely mechanical and attributable to imbibition of fluids after death. In these granular cells are not found. In other softenings the lesion is undoubtedly dependent on the formation and disintegration of these cells. This distinction, however, is not kept in view by Dr Todd, who, in too many instances, has evidently confounded them together, or at least not shown to which class of lesion the softening belongs.

How are these granular cells produced? We do not find that Dr Todd explains this anywhere, although he seems to think that the lesion, composed of these fatty granules, may arise in different ways. We do not deny the occasional occurrence of obstruction of the vessels in the manner pointed out by Virchow and Kirkes. Neither do we oppose the possibility of a fatty degeneration occurring spontaneously in the minute vessels of the brain, or in the cerebral substance itself. But this latter possibility we consider distant, when we take into consideration the extreme tenuity of the walls of the capillaries in the brain, and the excessive amount of fatty matter which sometimes surrounds them. If, as Mr Paget, and it would seem, Dr Todd, appear to think, the fatty granules are

formed in the walls or embedded nuclei of the vessels, then must these last be looked upon as structures secreting fat, and pouring it forth in many cases to an amount a hundred times as great as its own bulk. Then, how could distinct granular cells in such numbers be thrown off from fatty vessels? Indeed, the supposition of their origin in this way seems to us so opposed to all we know of cell formation, that we can have little hesitation in considering it altogether untenable.

Neither do we conceive it possible that these granular cells are produced by a fatty degeneration of the cerebral substance, for, how should degeneration give rise to cells which had no existence in that texture. In the liver, kidneys, and other cell organs, we find them, as Reinhardt pointed out, to be hepatic, renal, or other cells undergoing a fatty transformation. But unless it can be shown that the ganglionic vesicles constitute the granular corpuscles (which, considering their varying size in different parts of cerebral, cerebellar and spinal nervous matter, is altogether impossible), no analogy can be traced between such transformation in the cells of liver or kidney and those of the brain.

We deny, then, the proposition that the grey and fawn-coloured softenings, composed of these fatty granules and granular cells are owing to a primary fatty degeneration of the minute cerebral capillaries, or of the brain's substance; and we are equally opposed to the notion that it depends upon an unknown faulty state of nutrition as supposed by Delaberge and Monneret, and partly supported by our author; to a species of senile gangrene, as imagined by Rostan and Abercrombie, or to obliteration of the arteries, as first maintained by Bright and Carswell, and since in a new form contended for by Virchow and Kirkes. On the other hand, why should not the tissue of the brain be subject to a true inflammation and exudation of the liquor sanguinis, like all other vascular textures. The serous portion of the blood infiltrated among the delicate tubes of the brain would at once explain the diminished consistence from imbibition and maceration, whilst the fibrin coagulating round the vascular walls constitutes a blastema, in which new cells form, that in time undergo a fatty transformation. Hence is explained all the facts connected with softenings without hemorrhage. But when blood is extravasated the subsequent changes are easily accounted for, the colouring matter undergoing its usual transformations, the fibrin here also being transformed partly into cells and granules, whilst the serum after destroying cohesion, is for the most part absorbed. These views, advanced in a series of papers by Professor Bennett (*Edinburgh Medical and Surgical Journal*, 1842,) and supported by extensive researches into the minute structure and clinical history of cerebral softenings, appears to us the only correct method of explaining the real nature of these lesions.

The treatment of hemiplegia from softening we give nearly entire:—

"I believe that the most important end for the practitioner to aim at, in the early treatment of these cases, is to keep down the frequency and force of the heart's action. For this purpose the strict maintenance of the horizontal posture is of the highest moment; and when the patient is conscious, it is most desirable that the mind should be tranquillized by every means. It will, of course, likewise be necessary to remove all local impediments to the easy flow of the circulating fluid; and it is as well that the head should be slightly raised, sufficiently to prevent gravitation favouring the escape of blood from the ruptured vessels, but not so as to create any impediment to the flow, which might embarrass the action of the heart.

"To remove any source of nervous irritation which may be operating injuriously on the brain, the bowels should be cleared; and in order that there may be as little effort as possible on the part of the patient in the expulsion of the contents of the bowels, it is expedient that this should be done by enema; but if this fail, and the vital power of the patient do not forbid it, you may give croton oil, a drop or two of which, placed on the tongue, will operate freely; or calomel, in powder, to the extent of five or ten grains, which may be similarly administered. I would advise you to limit the further administration of drugs, to giving some slight corrective, as an alkali—ammonia being, on the whole, the most appropriate—unless, indeed, you find the patient in an extremely prostrate condition, when it will be necessary to combine with it the cautious exhibition of other stimulants and restoratives, as chloric æther, brandy, etc.

"The question of bleeding will arise; and, under the popular notion, that all head attacks are accompanied and caused by the rush of blood to the head, you will be pressed to have recourse to this expedient. There are three objects to be attained by bleeding; *first*, to diminish an undue amount of blood in the head; *secondly*, to check hæmorrhage, or to prevent it; and *lastly*, to quiet the heart's action.

We consider that in the present state of pathology the last named object is truly the only one that can ever be attained in such cases by bleeding. On this point, however, we shall not dwell, but continue the quotation from our author.

"If the patient be cold and collapsed, it is clear you should not take blood; nor should you have recourse to this practice if the heart's action be very feeble or intermittent; nor if there be an anæmic state; nor if the patient be of very advanced age; nor if the evidence of extensive disease of the arterial system, or of the heart, leave no doubt on the subject; nor would it be desirable to bleed if it were clear that already a large amount of hæmorrhage had taken place into the brain. Should none of these objections exist, then you will have to consider whether any or all of the indications above named need to be fulfilled, and whether bleeding (local or general) promises to fulfil them. As to the first indication, namely, the diminution of an undue amount of blood in the brain, I think modern investigation of the actual state of that organ clearly points out, that the brain is not in a hyperæmic state, in the cases in which the form of hemiplegia under discussion is likely to occur. Will taking blood check or prevent hæmorrhage? The sudden or rapid abstraction of a moderate quantity of blood, either from the arm or temple, or by skilful cupping from the nape of the neck, may, I can conceive, check hæmorrhage; and with this object it is, sometimes, a very justifiable practice, but the quantity taken should not be large. Now and then bleeding helps to diminish the frequency and force of the heart's action; but here, again, the quantity of blood withdrawn should be moderate, for the removal of much blood is apt to quicken the heart's action and render the blood poor. I would have you to look upon this question, to bleed or not to bleed, as almost the most important one you will have to decide; and, judging from my own experience on this point, as

well as from the results of the practice in a large number of cases collected from various sources, as I pointed out in a former lecture (Lect. VI. p. 105), I have come to the conclusion that, in cases of white softening, with or without hæmorrhage, you are less likely to err by omitting rather than by adopting the practice.

"You will often be consulted as to 'some expedient for promoting the restoration of the paralysed limbs to their normal condition.' To this question after having given a fair trial to the various means which have been proposed, I must reply, that I know of nothing which more decidedly benefits the paralyzed limbs than a regulated system of exercise; active, when the patient is capable of it; passive, if otherwise. As to the use of electricity, which is now much in vogue, or the employment of strychnia, which has been strongly recommended, I feel satisfied, as the result of a large experience, that the former requires to be used with much caution, and that the latter is very apt to do mischief, and never does good. I have seen cases in which, after the employment of electricity for some time, that agent has apparently brought on pain in the head, and has excited something like an inflammatory process in the brain. And so strychnia also will induce an analogous condition of brain, and will increase the rigidity of the paralysed muscles."—Pp. 209-13.

From what we have said of Dr Todd's Clinical Lectures, our readers will see that they contain matter of the utmost theoretical and practical value, and although we have ventured to differ on some points with the distinguished author, we know of few books which will so well repay the perusal of any medical man engaged in the earnest and careful exercise of his profession.

(To be continued.)

The Pathology of Drunkenness, a View of the Operation of Ardent Spirits in the Production of Disease; founded on Original Observation and Research. By CHARLES WILSON, M.D., Edinburgh, 1855. Fcp. 8vo, pp. 230.

It is a singular fact, that wherever the human race is found, and especially wherever it is civilised, alcoholic fluids have been introduced as stimuli. The maguey wine, or pulque, of the Mexicans; the palm wine of the Chilians; the beverage derived from maize in the countries of the Oronoko and Amazons; that obtained from a poisonous mushroom by the Kamschatskans, and lastly, the kiemiss, a drink ingeniously prepared by the wandering Tartars, from the milk of mares, they not having any arable produce—all contain this intoxicating principle. In Mahometan countries, where the religion is opposed to intoxicating drinks, the inhabitants have recourse to intoxicating drugs, as opium, tobacco, and hemp extract. In recent times, as the consumption of these substances have declined, it is worthy of remark that the infusions of certain vegetable substances, which act as strong stimuli to the nervous system, have become necessities of life, such as of coffee, tea, and the Brazilian holly. We can have no doubt that these substances serve to meet certain re-

quirements for nutrition in the animal body, and although chemists have not yet succeeded in explaining how this is accomplished with regard to all of them, the *modus operandi* of alcoholic drinks has, we conceive, been satisfactorily made out, principally by the researches and generalisations of Liebig.

But there is no good thing which may not be abused, and the excessive indulgence in the use of alcoholic drinks has of late years excited great attention from the physician, the moralist, and the legislator. It is only as a cause of disease, and with regard to the nature of the maladies so occasioned, that we can refer to this subject. Of the degrading character of drunkenness itself, and its tendency to the production of crime, we leave others to speak. Even with regard to the symptoms and morbid phenomena produced by this vice in the functions and organs of man, it is not our intention to enter at length, feeling persuaded that our readers are already familiar with them. All we need say is, that they are described in a very graphic manner, evidently from careful observation, in the elegantly written volume before us, which while it contains all that is known scientifically of its subject matter, is also well adapted for the general reader.

Chapter 9, on the physiological pathology of drunkenness, gives an excellent account of the effects of prolonged intemperance, firstly, upon the apparatus and the products of digestion, and through these upon the blood; and, secondly, such as are produced by this vitiated blood upon the liver, the heart, the lungs, the bloodvessels, the brain and nervous system, and the kidneys. The conclusion arrived at is,

"That alcohol is the most widely and intensely destructive of poisons. In large and concentrated doses there are few which are more promptly and inevitably fatal. In more moderate and diluted portions, continuously repeated, it is, with its own peculiar modifications of action, obviously one of those so called cumulative poisons, of which science possesses other well-known examples in corrosive sublimate, fox-glove, and arsenic; which also may be either rapidly fatal in large quantities, or small quantities of which may be given, with a first effect far less appreciable than that of alcohol, and continued, each separate dose alike unmarked by violent symptoms, till at last the deleterious qualities become amassed in the system, and life falls a certain sacrifice if they be farther perseveringly administered. Hence it is with a really just appreciation of their effects, that spirits, as taken by the drunkard, have received their ancient and common appellation of a 'slow poison;' an idea which Professor Huss has ingeniously developed, so as to systematise the greater part of their range of action. With not less truth, moral as well as physical, has gin been properly designated as 'blue ruin.' That a being possessed of reason should by such means destroy reason, and a being doting on life should thus be prodigal of life, is one of those weaknesses in man which may excite the sympathies of the coldest, while it is a dishonour which may humble the spirit of the proudest."—Pp. 192-3.

With regard to prevention and cure, we have nothing to say against what Dr Wilson has written, which in itself is excellent. For our own part, we think the diminution of drunkenness on a

large scale, to be almost altogether a matter of prevention rather than of cure. But *how* to prevent is the important question. Much has been done, and is still doing, to discourage intemperance by example, precept, and by rendering access to alcoholic drinks difficult. But hitherto it cannot be said that much impression has been made on drunkenness in the mass. A moment's consideration will show that it is a vice intimately associated with the social and moral evils of the lower orders, and if so, the removal of the one is intimately associated with the removal of the other. Good food, proper clothing, cleanliness, and cheerful homes are the real preventive remedies which we think are not sufficiently attended to by many well-meaning men, nor are they dwelt upon enough by our author. To denounce, repress, and render difficult, are all very well, but unless there be conjoined with these means an attempt to replace the temporary enjoyment you destroy, by something better and more rational, they will be of little avail. It is satisfactory to know that this idea is beginning to be entertained somewhat more seriously than heretofore, and from it we anticipate better results.

We can recommend Dr Wilson's work as one of the best yet written on the important subject of which it treats.

Medical Anatomy. By FRANCIS SIBSON, M.D., F.R.S., Physician to St Mary's Hospital, London. Folio, 1855. Fasciculus 1.

IN 1844 Dr Sibson published a paper in the *Transactions of the Provincial Medical and Surgical Association*, in which he observes, "It is now some years since I found that my notions of the usual and healthy sites of the various viscera were ill-defined. To clear up this obscurity, owing to which I was constantly at fault in examining patients suffering from chest diseases, I took diagrams of the position of the viscera, when making *post-mortem* examinations of the patients that died in the General Hospital near Nottingham. I first drew a careful outline of the ribs and sternum, and then added the internal viscera, taking care that their bearings to each other and the ribs, were accurately planned. After a time I procured a frame, and stretched strings across and along it, at distances from each other of three inches; the whole frame was thus subdivided into 45 squares. I ruled a piece of paper with squares of a like fashion, but of one-third the size; the frame I laid over the subjects to be copied, and with care and accuracy traced the objects that were behind each three-inch square upon the corresponding one-inch square on the paper." Afterwards Dr Hodgkin suggested another plan; it "consists in drawing the outlines of the organs on a piece of lace, stretched on a frame and placed over the body; the sketch is transferred by placing the lace over a sheet of paper, a piece of the 'manifold letter writer paper' being interposed. By

pressing firmly with a point on the chalked outlines, they are traced in black on the paper beneath. By this plan, employed with care, perfect accuracy is ensured."

We have quoted the above passages in order to show our readers how long and laboriously Dr Sibson has been qualifying himself for the publication of a work on medical anatomy. In the remarkable paper, indeed, to which we have referred, and which has been translated into German by Vogel, will be found a mass of observations, with careful illustrations, invaluable to the physician. We ourselves have had an opportunity of witnessing the exact manner in which Dr Sibson makes his drawings from the dead body, and there can be little doubt that any series of plates, having the high stamp of his name, will in future be regarded as the great authority on all those points having reference to the natural and morbid position of the various internal viscera of the body.

With regard to the first *Fasiculus* now before us, we can only express our astonishment at the beauty of the plates, considering the price which is demanded for them. Three folio coloured lithographs, with letterpress, not only giving explanations, but an amount of most useful information, are here published, for the moderate sum of five shillings. Of their accuracy, and value to the practitioner, the name of Dr Sibson, as we have already seen, is an ample guarantee, whilst their beauty of execution and trifling expense will, we are satisfied, secure for them a place in the library of every judicious medical practitioner.

A Manual of Elementary Chemistry, being a Practical Class-Book.
By ROBERT MORTIMER GLOVER, M.D., F.R.S.E., etc. London, 1855. Small 8vo, pp. 321.

Elementary Treatise on Chemistry. By WILLIAM GREGORY, M.D., F.R.S.E., etc. Edinburgh, 1855. Small 8vo, pp. 353.

BOTH these works give a very condensed view of the modern science of chemistry. That of Dr Glover, however, enters more largely into the subject of organic chemistry, and all those matters which interest the medical student. It contains a very useful chapter on the qualitative and quantitative analysis of mineral waters. Indeed we consider this manual one of the best that has yet appeared, and it is remarkably well illustrated by woodcuts and plates.

The *Elementary Treatise* by Dr Gregory is a reprint from the new edition of the *Cyclopædia Britannica*, and exhibits the remarkable power of condensation and thorough knowledge of his subject possessed by the author.

The London and Provincial Medical Directory, 1855. London. Small 8vo, pp. 696.

The Medical Directory for Scotland, 1855. London. Small 8vo, pp. 178.

THESE Directories are annually undergoing marked improvement, and go far to meet a want which has been long felt by the profession. At the same time, it must be confessed, that great numbers of medical men exist whose names are not comprised in the annual list here published. There are also many mistakes inserted with regard to names which are printed. Considering, as we do, that every medical man is interested in seeing these directories as accurate as possible, we would urge upon our readers, not only the propriety of returning his own schedule accurately filled up, but of correcting, as far as possible, any inaccuracies or omissions he may observe, in reference to the medical men of his own district.

Part Third.

PERISCOPE.

PHYSIOLOGY.

DR HAMMOND ON THE RELATIONS EXISTING BETWEEN UREA AND URIC ACID.

THE origin of urea is still somewhat undetermined, though numerous observations and experiments have been made by physiologists with the view of ascertaining the source or sources whence it is derived. That it is furnished, in part at least, from the metamorphosis of the effete nitrogenous tissues of the body is beyond doubt, and is admitted by all; but that it is also derived from the oxidation in the blood of the albuminates taken as food, without their previous conversion into tissue, is not so generally received.

Liebig, Bischoff, and others, hold that urea can have no other source than the metamorphosis of the worn-out tissues, and entirely reject the theory which ascribes its origin in part to the oxidation of the albuminates of the blood; whilst this latter view is supported by Lehmann, Frierichs, Bidder, and Schmidt, who adduce many experiments in confirmation of their opinion.

Without pretending to decide a point upon which there is so much conflicting testimony, but with the view of contributing towards its elucidation, I instituted the following experiments, the first series being performed upon myself:—

1. The object of the first experiment was to ascertain the normal quantities of urea and uric acid excreted by the kidneys in twenty-four hours, an average amount of food and exercise being taken.

I reckoned from seven o'clock A.M., having first passed the urine which had accumulated in the night. During the day, I ate sixteen ounces of animal food, and twenty ounces of vegetable food, consisting of bread, potatoes, and turnips; drank twenty-two ounces of water, and eight ounces of strong coffee; walked two and a half miles, and rode about three miles on horseback; took no other exercise. The mean temperature for the day was 74° Fahrenheit. I passed

thirty-one ounces and two drachms of urine, of specific gravity 1·021. The total amount of urea, as determined by Liebig's method with the nitrate of mercury, from 1000 grains of the whole quantity of urine, was 682·09 grains. The total amount of uric acid, as determined from 1000 grains of the whole quantity of urine, was 13·72 grains. These, then, may be regarded as the average normal quantities of these substances excreted by the kidneys in my own person, and as standards for the ensuing experiments.

2. The object of the second experiment was to determine the influence of exercise on the secretion of urea and uric acid, the diet being as in the first experiment.

As previously, commenced at seven o'clock A.M. During the day, ate and drank the same articles and quantities of food as in the first experiment. Walked briskly eight and a half miles, over a hilly country; rode ten miles on horseback; pitched quoits for two and a half hours; slept six hours. Mean temperature for the day 76°. Passed thirty-four ounces and one drachm of urine, of specific gravity 1·024. Quantity of urea (determined as in the first experiment), 864·97 grains; uric acid, 8·21 grains; showing an increase in the amount of urea of 182·88 grains, and a diminution in the uric acid of 5·51 grains.

3. To determine the quantities of urea and uric acid excreted, the food and drink being the same as in the other experiments, but the body kept as nearly as possible at complete rest.

Commenced at seven o'clock A.M. Immediately laid down on a sofa, and remained there continuously the ensuing twenty-four hours, with the exception of rising four times to urinate. Food and drink the same in every respect as before. Mean temperature for the day 73°. Slept nine and a half hours. Total amount of urine, twenty-four ounces and seven drachms. Specific gravity, 1·018. Quantity of urea, 487 grains; uric acid, 24·86. A diminution of urea from the normal standard of 125·09 grains, and an increase in the quantity of uric acid of 11·14 grains.

These results may be embodied in tabular form as follows, the diet being the same in each experiment:—

	Quantity of urine.	Specific gravity.	Quantity of urea.	Quantity of uric acid.
Moderate exercise,	31 oz. 2 drms.	1·021	682·09 grs.	13·72 gra.
Increased exercise,	34 „ 1 drm.	1·024	864·97 „	8·21 „
No exercise,	24 „ 7 drms.	1·018	487 „	24·86 „

From the above, it is, I think, conclusively shown that exercise, while it increases the amount of urea, diminishes very materially the quantity of uric acid; and that inactivity, though it diminishes the amount of urea, exercises a contrary effect upon the uric acid. As far, then, as experiments of this nature can do so, these decidedly tend to support Liebig's doctrine. It must, however, still be admitted that there are great difficulties to be overcome before this view can be regarded as actually proved.

In further reflecting upon this subject, it occurred to me that if I could, by providing a greater supply of oxygen than ordinary, induce the formation of urea in animals in which it does not naturally exist, it would almost demonstrate conclusively that the hypothesis advanced by Liebig is correct.

I therefore procured a young black snake (*Coluber Constrictor*), and confined it in a large jar. It was fed, *ad libitum*, on flies, grasshoppers, and other insects. At the end of a week, I examined the solid excrement which had collected in the bottom of the jar. As I expected, it contained no urea; but, on dissolving it in warm water, and adding a few drops of hydrochloric acid to the solution, a large quantity of uric acid was in a short time precipitated. I next fitted to the jar an air-tight stopper, through which passed a glass tube. Through this tube I introduced, three times a day, for a week, about two hundred cubic inches of oxygen, which was retained in the jar for two hours at a time. By its influence, the snake was rendered excessively lively; his eyes sparkled, and he

darted from side to side with surprising agility. This state of activity continued during the whole time the jar remained closed. When atmospheric air was admitted, he soon relapsed into his usual sluggish state. During these experiments, his food was the same as previously mentioned, and was devoured with increased voracity.

At the end of a week, I removed the excrement from the jar, and dissolved it in warm water. A drop of the filtered solution was suffered to evaporate on a glass slide. On viewing it under the microscope, I observed most beautiful crystals of urea, mingled with those of amorphous urate of ammonia. Not being willing to rely upon the crystalline form alone as a test, I submitted the remainder of the solution to chemical examination, as follows. The process employed was that recently proposed by Liebig as a modification of his method of testing for urea with the nitrate of mercury. Solutions of corrosive sublimate and bicarbonate of potash were prepared and mixed together. On adding this mixture to the solution containing the excrement of the snake, the white precipitate of urea and protoxide of mercury were immediately thrown down. This examination I regarded as showing conclusively the presence of urea, and that in no inconsiderable proportion.

From the several experiments recorded in this paper, and more especially from the last series; I consider that the theory of Liebig, accounting for the formation of urea, is probably correct. True, there are many facts difficult to account for on the supposition of its truth, and many well-founded experiments to set aside, but surely the evidence is not altogether opposed to it; and, though much still remains to be accounted for before it can be universally received, there is ground to expect that future investigations will do much towards its establishment.—*Amer. Journ. of Med. Science.*

VALENTIN ON THE GASES OF DIGESTION IN HORSES.

Valentin examined the gases of various parts of the intestinal canal of horses. The animals were killed by bleeding. Separate portions of the intestinal canal were isolated by double ligatures, and the gases collected immediately after death. We abstain from describing the chemical methods employed, but will construct a table showing the percentic composition of the gases of the two horses examined.

A. gives the figures belonging to a gelding, 20 years old, healthy, fed with oats and hay. The stomach contained a large quantity of food; the gases of the small intestines were collected from comparatively empty portions; the cæcum was in a great part filled with the remains of food; the gases of the rectum were from between the fecal balls.

B. was an old mare, healthy, fed with oats. Stomach and cæcum were almost filled with solid masses. The rectum contained so small a portion of gas that no sufficient quantity for an analysis could be collected. The interval between the last meal and the death of the animal is not named.

Names of the gases	Stomach		Upper portion of small intestine, A.		Middle portion of small intestine, B.		Lower portion of small intestine, A.		Cæcum		Middle portion of rectum, A.	
	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.
Carbonic acid	44.35	55.64	18.83	...	41.78	...	19.41	...	77.70	...	71.59	...
Carbur. hydrogen	0.90	...	0.45	...	4.98	...	0.77	...	4.09	...	6.96	...
Sulphur. hydrogen	2.70	...	1.61	...	4.52	...	1.46	...	2.02	...	3.71	...
Hydrogen	0.66	13.29	0.02	...	0.08	...	4.67	...	0.20	13.82
Oxygen	7.16	0.77	5.76	4.97
Ammonia	1.29	...	1.22	...	1.49
Nitrogen	44.23	25.38	73.35	...	48.70	...	73.31	...	10.23	...	16.32	24.39
	100.00	100.00	100.00	...	100.00	...	100.00	...	100.00	...	100.00	100.00

The coincidence of the large proportion of carbonic acid in the stomach and cæcum of both horses is remarkable, a circumstance probably connected with the functions which both organs have to fulfil in the digestive process.

The presence of sulphuretted hydrogen in the gases of all parts of the intes-

tinal canal, makes Valentin conclude that the formation of this gas commences in the stomach. He considers the albuminous substances of the food to be the source of this formation, and draws the inference, that in the stomach not only a mere solution of some of the substances containing nitrogen and sulphur takes place, but that there is also a partial decomposition. It must, however, as yet remain uncertain how much of the sulphuretted hydrogen found in the stomach is due to a development of this gas in the stomach itself, and how much may have been introduced from lower parts of the diffusion of gases. Ammonia, it will be seen, on the other side, is met with only in the large intestines. The proportion of hydrogen found by Valentin is smaller (except in the rectum) than that stated by former observers. He agrees, however, with them concerning the total absence of oxygen in the gases of the large intestines. The larger quantities of carburetted hydrogen and hydrogen in the rectum show that changes in the remains of food continue taking place up to the last portions of the digestive tube.—*Vierordt's Archiv.*; and *Brit. and For. Med. Chir. Rev.*

HIFFELSHEIM ON THE CAUSE OF THE HEART'S MOVEMENTS.

"Le Cœur bat parce qu'il recule." (The heart beats because it recoils).

Such is the theory held by Dr Hiffelsheim of the cause of the heart's action, in a paper which he recently read to the French Academy of Sciences, and which has created great interest in the medical circles of Paris. He divides the movements of the heart into two kinds:—absolute and relative. The *absolute* is the movement by which the organ strikes the thoracic parietes—"shock"—or the *ictus* of the ancients. The *relative* movements are the phenomena of systole and diastole—viz., of contraction, elongation, and spiroid torsion. All the kinds of movement occur at the same time. Therefore, while the heart is displaced, it is undergoing at the same time variations of form and volume—it is contracted, elongated, and twisted upon itself. Physiologists are not agreed as to the relations of succession and coincidence of the different relative movements among themselves, on the absolute and the relative movements; but they are unanimous in making the absolute depend on the relative movements; or, in other words, they attribute the locomotion of the heart to the direct effect of either the systole, or the diastole, or the spiroid movement. Dr Hiffelsheim believes that he can prove, from experimental researches, that the relative movement of the systole, determines the absolute movement of the heart, and consequently precedes it; and that the expulsion of the fluid is the immediate cause of this absolute movement. He founds this view on the fact that a contractile envelope, forcing a liquid from its interior through one or more apertures in its walls, experiences—above everything—a rectilinear re-action, occurring in an inverse ratio to the force exerted, and which is represented by the strength of the jets of fluid. In the case of the heart we have a contractile or elastic envelope expelling a fixed fluid by its compression. The phenomena alluded to will occur just the same—so far as relates to the reaction produced—if we had a fixed envelope, containing an elastic fluid, the volume of which became increased.

Every time that a cavity, formed by mobile walls, expels a liquid through an orifice in them, there will be a tendency to recoil. Hiffelsheim performed a series of experiments by means of an india-rubber pouch, made to represent the heart; and he ascertained that the amount of the recoil was always in a ratio corresponding to the quantity of the fluid, the thickness of the walls of the pouch, and the size of the aperture through which the liquid was forced. He adapted a caoutchouc aorta to the artificial heart in order to ascertain the effect which the expulsion of the fluid into the aorta had on the heart's recoil. He found that, under such circumstances, the strength of the recoil was greater than when the simple india-rubber heart was used without an attached aorta.

Hence M. Hiffelsheim proposes to reconcile the differences of opinion which exist among physiologists by the doctrine we have mentioned:—"Le cœur bat

parce qu'il recule."—*Comptes Rendues de l'Acad. des Sciences*, 27th Nov. 1854; and *Gaz. des Hôpitaux*, 5th Dec. 1854.

KLETZINSKY'S MODIFICATION OF TROMMER'S TEST FOR SUGAR.

This chemist recommends glycerine for the preparation of the testing fluid. Four grammes of crystallised sulphate of copper are to be dissolved in the smallest possible quantity of water, and this fluid, while boiling hot, is to be mixed with 6 grammes of glycerine, and with as much solution of caustic potash as can be obtained by making the most concentrated possible solution of 8 grammes of the alkali. The mixture is then set aside, and in cooling it deposits crystals of sulphate of copper. The deep azure-blue solution which remains, on being diluted with its own volume of water, constitutes the test fluid. This fluid gives the same reaction with saccharine urine as occurs in the ordinary Trommer's test; and enough of it should always be employed to produce the same results.

Kletzinsky mentions that he has kept the test fluid for two months without witnessing the occurrence of any decomposition on it.—*Zeitsch. der K. K. Gesells. der Aerzte zu Wien*, Feb. 1854.

PATHOLOGY AND PRACTICE OF PHYSIC.

A CYST AT THE BASE OF THE BRAIN FORMED BY THE SOFTENING OF SCROFULOUS DEPOSIT. BY DR J. W. OGLE.

THE specimen consisted of a brain showing a cyst of about the size of a pigeon's egg situated in the right portion of the substance of the pons Varolii, and having thin and friable parietes. This cyst, when recent, was full of a yellow glairy fluid in which a number of light-coloured soft particles of albuminous matter existed, and was lined by a delicate but firm membrane.

The cyst extended outwards to the extent of about three quarters of an inch, also forwards and backwards so as to indent the right lobe of the cerebellum, and the under surface of the middle lobe of the right cerebral hemisphere, interfering by pressure with the fourth, fifth, sixth, and seventh pairs of cranial nerves on the right side.

The arachnoid tissue in the neighbourhood of the cyst, was thickened and opaque, and the lateral cerebral ventricles were expanded, and distended with a quantity of clear limpid fluid. In other respects the entire brain and the membranes were quite healthy.

The specimen was removed from a girl, æt. 18, who was admitted into St George's Hospital in the following condition:—She was pale, evidently of a weakly disposition, and complained of a very "violent cold." When she attempted to smile or was made to whistle, the mouth was obviously, though not to any great extent, drawn to the *left* side. There was a kind of tottering and difficulty in using the left leg when she attempted to walk, and also loss of power in the *left* arm and hand. She complained of a general numbness, and also of loss of sensation, when pinched, up the whole of the left side as high as to the *middle* of the neck, and this was also the case with the *right* temple, and the right side of the nose and face as low as the base of the jaw. The right arm and leg were in a perfectly healthy condition. There was marked vascularity of the conjunctiva of the right eye and much lachrymation existed. The right eye-ball was drawn inwards, and though the patient could move it in other directions, she could not abduct it; and the pupil was rather contracted, but tolerably active under its stimulus. The left eye-ball had perfect motion. Its pupil was rather dilated, acting under the application of light. The vision of the *right* eye was imperfect, and the patient was unable to close the right eyelids completely, the pupil being turned upwards and inwards on the attempt being made. There was a difficulty in trying to swallow or open the mouth, and a peculiar noisy inspiration whenever she breathed. These latter symptoms seemed at the time referable to enlargement of the tonsils which existed to a great extent. The bowels were costive, and the urine,

which was free from albumen and was natural, was at times passed involuntarily. The tongue was clean and red, and protruded in a perfectly straight direction. The gums were spongy and red as if from the administration of mercury, and the pulse was small and quick. The mental powers appeared intact, and the patient intelligently related her own history and case. No pain was in any way complained of, and nothing unhealthy was discovered about the lungs or heart. On examination, her history proved to be as follows:—She had enjoyed good health until about two months before her admission into the hospital, but had never menstruated. At that time she was, owing to cold, affected with hoarseness and sore throat, and about three weeks afterwards was afflicted with giddiness and pain at the forehead, which remained for about one month. At the same time a “numbness and tottering” of the whole of the left side of the body came on, and she was treated by a medical man; along with this the eyesight on the right side began to fail, and the right side of the face became numb. For the first fortnight also after the numbness came on, the patient had double vision, though this did not last long. The giddiness and numbness, however, have remained ever since. The patient had never been affected with tinnitus or any visual illusions, or with any stiffness or pain of the neck or scalp, or yet with any cough or hæmoptysis or discharge from the ears or nose, but she had been deaf for some weeks of the right ear.

When admitted into the hospital she was placed under the influence of mercury, and a blister, dressed with blue ointment, was applied to the nape of the neck. The bowels were carefully regulated by senna, and she had the ordinary diet. She was shortly placed thoroughly under the influence of mercury; but of this she was herself ignorant, as she was insensible to its effects. When she had been in hospital about fourteen days, she was observed as having become very obtuse, she was affected with loss of appetite, and frequent attacks of vomiting. The speech had become very indistinct. The conjunctiva of the right eye had become very highly vascular, and the cornea very dull, though the other eye was also suffused. The pupil of the right eye which was more contracted than the left one was turned upwards and inwards, and she always slept with the right eyelids open. The pupils of both eyes acted. The pulse was 80, and the skin cool. The mercury was at that time discontinued, and another blister applied to the neck. Salines and milk diet were resorted to. After this the patient varied greatly. The tongue became dry, and the difficulty of deglutition and opening the mouth very great. The bowels were never open without the use of medicine. About ten days later the eyesight, on the right side, had become still more impaired, and the pupil, which was more contracted, would scarcely respond to light. Vomiting became a more general feature in her case, and the mouth was more decidedly drawn to the left side. Later still, pain at the back of the head came on, but up to this time the appetite remained very great. Purgatives and blisters were the chief agents resorted to. After the patient had been in hospital about seven weeks she was seized with an attack of what the nurse called a “fainting fit” whilst on the night chair, and a few hours later insensibility came on, with lividity of the face, foaming of the mouth, coldness of the surface, and depressed pulse; and after much “bronchial rattle” the patient died. On *post-mortem* examination, nothing except congestion of the lungs and accumulation of mucus within the bronchial tubes, beyond what has previously been described, was found.—*Trans. of Patholog. Soc. of Lond.*

CASE OF RUPTURE OF THE SEPTUM CORDIS. BY DR PEACOCK.

The gentleman who was the subject of this case was a patient of Mr Brendon's, and for the specimen and history, Dr Peacock was indebted to Mr G. F. Lane. He was about 62 years of age, had enjoyed, generally, good health, and was in the practice of going from Highgate into town every morning, and returning in the evening. Four or five days before his death, he felt, one

evening, after his return home, a slight pain in the chest, exactly over the ensiform cartilage, and, as he had occasionally suffered from muscular pains, he was supposed to be rheumatic, and treated accordingly. He was soon relieved, and, indeed, remained at home only two days, and that, not because he felt ill, but from having no engagements at his office. On Wednesday evening, the 22d, after his return home, he had a light dinner, which he enjoyed, and took three glasses of sherry with it. At about half-past ten o'clock, he said he should go to bed, and his wife noticed that he hurried up stairs, undressed rapidly, and immediately got into bed, but this would not have attracted attention had it not been for the result. His wife then observed that his breathing was rapid, and said that he had walked up stairs too fast, but he made no reply. Immediately after he jumped out of bed, went to the night chair, and again lay down. His wife asked him why he so immediately returned to bed, to which he merely answered that he supposed he might get into bed. His breathing then became more difficult, but not so as to alarm his wife much, till half an hour had elapsed, when she sent for Mr Brendon, who arrived at twelve o'clock, and found that the gentleman had then been dead some minutes.

Post-mortem examination.—The chest only was examined. The venous system was loaded with blood, and the veins, about the upper part of the mesial incision for opening the chest, bled freely. There was about an ounce of bloody fluid in the pericardium. The lungs were healthy; the left ventricle was empty, and the right contained a little semi-coagulated dark blood. The heart was large, weighing fourteen ounces and a half avoirdupois. There was a considerable deposit of fat following the course of the vessels, and especially on the anterior surface of the right ventricle. The attached pericardium, chiefly on the right ventricle and auricle, was thickened and displayed several white patches, and at the posterior part of the organ there was a deep discoloration of the surface, from the infiltration of blood into the subserous cellular tissue. The left ventricle was of large size, and its walls were thicker than natural. In the septum ventriculorum, about one-third from the apex of the left ventricle, there was a large rupture. This commenced behind the attached fold of the mitral valve, and extended across the septum, in a somewhat crescentic direction, penetrating to the apex of the right ventricle, into which it opened by a laceration sufficiently large to pass the point of the forefinger. Posteriorly, it perforated the entire thickness of the wall of the ventricle, so as to be separated from the cavity of the pericardium only by the serous covering; and, above the extravasated blood, produced a swelling, projecting into the right auricle. The substance of the heart was generally flaccid, and, in the neighbourhood of the rupture, obviously fatty. The mitral and aortic valves, and especially the latter, were opaque and much thickened. The coronary arteries were studded with patches of atheroma and bone. The aortic orifice was considerably less capacious than natural, only giving passage to a ball measuring thirty-three French lines in circumference, while the pulmonic aperture admitted one measuring forty-two lines.

I have been favoured by Dr Bristowe with the following report of the appearance under the microscope:—"The muscular tissue of the apparently healthy portions of the heart presented, universally, unusual indistinctness of the transverse striæ, and, in many of the fibres, beads of oil were arranged in longitudinal strings.

"The pale and mottled portion (that in which the rupture had occurred) presented variable appearances under the microscope. Many of the fibres were but little diseased, whilst others were degenerated to the last degree. The latter were opaque, and all trace of definite structure was often completely replaced by innumerable and densely aggregated refractive globules, in some cases exceedingly minute, in others of considerable size, and evidently oily in their character. Many fibres were observed, which, in one part of their course were tolerably healthy, and, in another, greatly diseased,—the transition

being more or less abrupt. The diseased fibres were not uniformly distributed, but appeared to be, for the most part, collected into groups; they presented also a marked brittleness, or tendency to break into fragments."—*Trans. of Patholog. Soc. of Lond.*

FRIEDREICH ON THE DIAGNOSIS OF TUMOURS WITHIN THE CRANIUM.

The above author has recently published a very interesting monograph on this subject, containing the particulars of 45 cases of intracranial tumours, 11 of which were observed by himself; and on these he has founded some valuable remarks as to the mode of their diagnosis. The following is a brief resumé of his observations on these points. He considers

1st. *The General Effects of Intracranial Tumours.*—Various derangements of sensibility occur. One of the most constant of these is *headache*, which is especially frequent in the early stages of the disease. The cephalalgia is remarkable for its persistence and intensity; it may be either continuous or intermittent; and it may be accompanied by *vertigo* and *vomiting*. Its site does not always indicate the place of the new growth. The organs of special sense may be affected; the *sight* becomes impaired on one or both sides; there may be *strabismus*; and the *hearing* generally suffers more or less. The minor grades of *paralysis* are common. The length of interval between the initiatory headache, and the occurrence of the paralysis constitutes the most characteristic mark of these intracranial tumours. *Convulsions* and *spasmodic conditions* occur in one half of all cases; and the former often assume an epileptic type. The *mind* is always more or less affected; its diseased condition generally commencing with *loss of memory*.

All these symptoms are very inconstant and variable; they are also liable to alternate remissions and exacerbations, which probably are due to the occurrence of transitory congestions either of the tumour, or the cerebral substance, or perhaps of both together. The course of intracranial tumours is always chronic. Friedreich never knew a case to be shorter in duration than 6 weeks, or longer than 14 years.

2d. *The Special or Differential Diagnosis of Intracranial Tumours.*¹—(a) Those situated in the *cerebral hemispheres* (18) are generally accompanied by obstinate headache (14); nausea and vomiting (9); by derangements of the motory functions (14); consisting of more or less extensive paralysis, and of convulsions which assume an epileptiform character. When hemiplegia occurs it is sometimes crossed (*gekreuzt*), and sometimes not; but it constantly occurs on the affected side. Derangements of the special senses are common (10), especially of sight (7); and intelligence is often impaired (11). In a few exceptional cases there are no headache or alterations of the motor functions.

(b) *Tumours of the base of the cranium in the neighbourhood of the pons*, occasion the following symptoms:—(9) headache, (8) almost always frontal; impairment of vision (7), commonly also of hearing and taste (5), and in some cases (3) of smell. All these symptoms, due to loss of power of the facial nerves, occur on the same side as the tumour; but paralysis of the extremities, when it occurs, affects the opposite side of the body. Complete hemiplegia and paraplegia are not very common; and convulsions occur less frequently than with the former class of tumours, and are not epileptiform. An important sign of these tumours is afforded by the great multiplicity of the existing sensorial disturbances, and the tendency of the optical derangement to become bilateral. The mind is sometimes affected (5).

3d. *Tumours of the Pituitary Region.*—Friedreich only saw 1 case of this. There was frequent frontal headache, often with pain in the orbit; and double amaurosis. There is rarely any disturbance of the motor functions.

¹ The numbers placed above in brackets refer to the number of cases, among those examined by Friedreich, in which the particular symptoms were present.—*Translator.*

4th. *Tumours of the anterior part of the Base of the Brain.*—Two cases were examined. The symptoms resemble those just mentioned.

5th. *Tumours of the Peduncles of the Cerebrum and Cerebellum.*—Paralysis of the face and extremities, occurring on the side opposite to that on which the tumour was situated, was observed in 3 cases. This makes tumours thus situated resemble those of the hemispheres. Complicated derangements of the nerves of special sense and of the face (as the oculo-motor and trifacial), were seen in 2 cases. This, on the other hand, approximates these tumours to those of the base.

(f.) *Tumours of the Cerebellum* (8) had the following signs:—Violent cephalalgia (7), often intermittent, and combined with vomiting (4); and situated in the occipital region (4). Occipital headache may be considered to be pathognomonic here, as it occurs in the case of no other intracranial tumours. Pain at the nape of the neck, increased by pressure, may exist. In one case there were no peculiar symptoms; and in none of the cases of tumours of the cerebellum were the generative functions at all affected.

(g.) *Tumours of complex situation.*—Correct diagnosis is here impossible.—*Beiträge zur Lehre von dem Geschwulsten innerhalb der Schädelhöhle. Würzburg, 1853. Also Prag. Vierteljahrs, 1854, Bd. III.*

PROFESSOR MAUTHNER OF VIENNA ON THE INJURIOUS EFFECTS OF CERTAIN TRADES UPON THE HEALTH OF CHILDREN.

The health of the children of the poorer classes is seriously injured, not only by the laborious household duties which they are called upon to perform, often at a very early age, but also by the close unwholesome dwellings in which they are housed, and in which, not unfrequently, trades are carried on by their parents that are directly prejudicial to health. In fact, the diseases of the children of the poor often stand in direct etiological relation to the occupation of their parents. The vapour given off by the paste (*Weberschlichte*) wherewith the linen-weaver moistens his yarn is a common cause of mucous catarrh among their children. The exhalations from the bodies of the workmen who are congregated together in the workshops of poor tailors, and the unwholesome smell arising from the heaps of old clothes which accumulate therein, are a fruitful source of rachitis and scrofula among the children of such persons. The children of fleshers, meat-curiers, and killers of swine and calves, are very liable to obstinate impetigo, and other chronic skin diseases; those of bakers are generally pale and bloated; while those of gilders are subject to chronic inflammations of the eyes, owing to the impregnation of the atmosphere in their dwellings with particles of fine gypsum dust. Mauthner witnessed a case of copper poisoning in the child of a carpet-colourer, who had to grind the verdigris for the formation of his father's green colour.

Professor Mauthner considers that the main cause of ill health among the children of the poor, is the thoughtless manner in which their parents select a trade for them, without considering whether or not they are physically adapted for it. They say "My son shall be a tailor like me, or a shoemaker like his grandfather," without ever reflecting whether or not the child's health is adequate to the requirements of the trade.

Artificial flowers are usually made by girls, and the trade is to them a most injurious one. It is only carried on during a few months of the year,—namely, at the Carnival time; and the girls work in small, close apartments, by dim artificial light, and they are very ill fed and ill remunerated. Disease is therefore common among them: and Professor Mauthner gives the details of a case of inflammatory *ramollissement* of the spinal cord which he witnessed in a child, 11 years old, who had been engaged in this work.

The straining of the lungs, required by the use of the blowpipe, renders emphysema (*Bläh-halse*) common among the apprentices of goldsmiths; and even the older workmen are generally more or less affected in this manner

In these shops, the inhalation of the particles of dust set free in powdering the coals for the use of the blowpipe, is very injurious to youths with delicate lungs : so also is the process of sifting the sweepings for small particles of silver and gold, which requires to be done by them. The eyes of young people are much injured by the great use of artificial light by jewellers and goldsmiths, as also by the employment of the globes of water which they use to concentrate the light. Spinal curvature is common among the apprentices of stone-setters, who lean down with the left shoulder against the work-table, while the right is obliged to be kept elevated, for the proper management of the tools. And the employés in goldsmiths' works are also exposed to chemical fumes set free during the process of melting : viz., to those of lead, sulphur, ammonia, nitre, and also to the vapour of mercury.

Children are variously injured by working in large spinning factories, for their labour therein is long continued and exhausting. The apprentices of lace manufacturers are often injured by the pressure of the sternum against the so-called "Breast-board ;" and Mauthner has even seen it occasion periostitis of that bone.

Although the physician cannot prevent the operation of the causes just mentioned, he should bear them in mind in reference to the treatment of the diseases of the children of the poor.—*Journ. für Kinderkrankheiten.* May and June 1854.

M. TROUSSEAU ON THE TREATMENT OF SPERMATORRHEA.

M. Trousseau thinks that the advantage of Lallemand's *porte caustique* have been considerably overrated, and that there are only certain cases in which its use is productive of benefit. It is very useful where chronic urethritis co-exists with the spermatorrhea, but where that is absent he thinks we ought to trust to other modes of treatment more suited to the cause of the disease.

The excessive debility induced by spermatorrhea demands our most serious attention. If, in serious cases, we find neither urethritis nor cystitis present ; if we discover neither calculi nor ascarides, nor any other thing which can explain the persistence of the emissions, we ought to ask ourselves whether the disease does not depend upon a condition of the vesiculæ seminales analogous to the spasmodic state of the bladder in certain forms of incontinence of urine. Puerile enuresis is not due to atony of the bladder, or to any undue accumulation of urine, but to a spasmodic condition of the bladder. The same phenomenon occurs in the vesiculæ seminales ; and belladonna, which acts so beneficially in the case of the bladder, is also very useful in this other spasmodic condition. M. Trousseau prescribes accordingly in such cases, powders containing each 1 centigramme of the powdered root of belladonna, mixed with sugar. He orders one to be taken daily during the first week of treatment ; two daily during the second, and so on until the patient experiences a sensation of dryness in the throat. At the same time he orders frictions of the perineum with an ointment composed of 10 grammes of the alcoholic extract of belladonna to 20 grammes of axunge. If necessary, he also uses suppositories containing each 10 centigrammes of the extract. belladonna.

M. Trousseau doubts the utility of cold hip-baths in this affection. They may do good the first time they are used, but although they may temporarily arrest venereal excitation in nymphomania and priapism, this calm disappears on the occurrence of reaction, and the evil is increased.

Heat acts in an opposite manner. Hence M. Trousseau believes that in cases where erotic feelings are conjoined to spasm of the vesiculæ seminales, it is the best sedative which we can employ simultaneously with belladonna. The form in which he employs it is that of bags of heated sand, which he applies to the perineum for a few minutes, morning and evening. The simultaneous administration of lupulin may be very beneficial ; but, where we desire decided anaphrodisiac effects, M. Trousseau recommends, on account of the certainty and efficacy of its action, the bromide of potass, in doses of from 15 grs. to 5ss.

daily.—*Journal de Med. et de Chirurg, Prat*, December 1854, and *L'Union*, 21st December 1854.

[We have elsewhere (see *Therapeutical Notes*), directed attention to the anaphrodisiac virtues of Lupulin, and the mode of its administration; and in last month's *Periscope* (p. 163) we recorded the results of Thielmann's experience of the brom. potass. as a sedative in cases of erotic excitement.]—*Translator*.

ZIMMERMAN ON PECULIAR CONCRETIONS IN TYPHUS STOOLS.

This author has repeatedly observed in the stools of typhus patients considerable numbers of peculiar concretions, which varied in size from that of a large Turkish to that of a common white bean. He considers that there exists some relation between them and the millet-seed-like white corpuscles which, it is known, are found in typhus stools. The concretions varied in form and appearance; some were round, others spherical; some were smooth, and others rough on the surface. They were of a yellowish-white colour, of tolerably firm consistence, but capable of being crumbled between the fingers. When chemico-microscopically examined they were found to contain, besides large numbers of epithelial cells, various fatty matters, (1000 parts contained 237.8 of solid residue, and 156.9 of fats), among which there was almost no cholesterine; they also contained biliary constituents—a sort of albuminous substance, which Zimmerman believes to be casein and fibrin; and finally various salts, (viz., sulphates, carbonates, and phosphates of soda; chloride of sodium, and earthy phosphates.) The concretions differ from the corpuscles in their containing protein compounds, while the others consist of fat, with merely a little earthy matter. In the concretions the mineral ingredients are to the organic in the proportion of 1 to 17; in the fæces they are as 1 to 1; the former contain little chloride of sodium, while in the latter, this salt exists in large proportions—constituting nearly a third of the whole.

Sigmund found similar fatty concretions in the fæces of a lady affected with biliary derangement (*Med. Chemie*); and Kletzinsky (*Heller's Archiv.*, 1., 1853) found similar fatty matter in the caecum of a person who had died of typhus, which was deficient in cholesterine, but contained biliary matters, intestinal mucus, and various salts, especially earthy phosphates.—*Deutsche Klinik.*, 28, 1853.

SURGERY.

DISCUSSION ON THE CURABILITY OF CANCER, AND ITS DIAGNOSIS BY MEANS OF THE MICROSCOPE, AT THE FRENCH ACADEMY OF MEDICINE.

(Concluded from page 170.)

M. VELPEAU resumed his discourse, and continued thus :—

I think I have already demonstrated the non-specific character of the cancer-cell. But even admitting that it is specific, it cannot be said to be a constant and peculiar character by which we can, at all times, distinguish that which is cancerous from that which is not. May there not be in cancer, as in all diseases, certain phases of the malady in which some of its characters may be altered or absent? May not the cancer-cell be wanting at the outset, for example? May it not be that, in certain cancers, which we remove early (as those of the lips, for instance), the cancer-cells are not present which would have become developed at a later period, if the tumours had been left alone? I do not say that it is absolutely so; but still it is quite possible. This would afford an explanation of those existing facts, for which it is better to seek an interpretation than to deny them altogether.

I have, moreover, often seen patients having tumours containing cancer-cells, in whom secondary tumours became subsequently developed, having the same characters as the primary growths, but which were quite devoid of cells. I would recall to your recollection the case of a woman, to whose history I have already alluded, who had two tumours, one on each side of the chest, the one

of which presented, in a very doubtful manner, the characteristics of cancer, and yet contained cells; while the other, which had all the appearances of malignant cancer, contained no cells. Hence I believe that, in tumours of the same kind (and in this case their identity could not be disputed), the cancer-cells may be present at one time and absent at another. This is what M. Robert disputes. He says that I have stated the same fact differently in my book and in my speech. I have gone over both carefully, and would ask wherein lies the difference?

M. ROBERT here explained that M. Velpeau had stated in his work that the patient died of pleurisy, and, in his speech in the Academy, that she died of cancer of the liver.

M. VELPEAU—I am not responsible for the account given of her death in my work, as it was given me by M. Labé, my *externe*, and by M. Duménil, *interne* at *St Louis*. It matters not of what specific lesion the patient died, so long as the main fact remains undisputed, viz., that there were cancerous tumours in the liver,—and *this* circumstance is stated both in my book and in my speech. What are we to make of the fact of cancer-cells having been found on one side and not on the other? I cannot tell; it is a problem which the microscope should explain."

M. Velpeau then adduced several cases illustrative of the presence of cancer-cells in secondary tumours; and he mentioned an interesting one, taken from the Clinique of Professor Langenbeck,¹ in which, some months after the successful removal of a mammary hypertrophy (cystosarcoma), a new tumour appeared on the finger, and other growths in various localities, and in most of the viscera. In all of these secondary productions cancer-cells were found *en masse*. These, said he, were facts to be explained, not to be denied.

M. Velpeau continued:—"M. Robert has committed a mistake in speaking about malignity, for he has considered the subject in a general manner, and not in its true sense. Thus, he has spoken of malignity in relation to severe cases of lupus which compromise the patient's health; but this is departing from the sense of the word malignity. The malignity of a cancer does not consist in the greater or less complication of secondary affections; nor does it consist in a cancer being more ulcerated, more extended, or that it exists in an important organ. A cancer is malignant by its own nature, because it is a cancer; and there are no tumours, except those which are cancerous, which present in a constant or inherent manner this malignant character, irrespective of all accessory conditions. Cancer is malignant whether it is large or small, alike at its commencement and its termination, and on whatever part of the body it is situated. Moreover, I would impress upon you that what I have said of the malignity of cancer in general, is equally applicable to those fibro-plastic tumours, regarded as benign by the microscopists, which progress continuously, recur, and terminate fatally in the same way. It is not, therefore, correct to apply the term malignity only to heteromorphism, as the microscopists do.

I would also remark that the microscopists have recently made large concessions. Heteromorphism is not a new doctrine; it extends back to Laennec. But the microscopists took it up, and made it the basis of their classification of tumours. Hence they admitted the existence of homeomorphous tumours, which were benign; and of heteromorphous tumours, which were cancers. But at present they renounce this distinction. A few days ago one of the histologists said that the distinction between heteromorphous and homeomorphous growths had vanished, and that it had no foundation. This is truly a step further in our direction; and only a little thing is now required to approximate us entirely. The profound differences which divided us at the commencement of these debates have already been left behind and forgotten. Yet these differences have led some of our adversaries to underrate clinical indications. On the discovery of the cancer-cell, they said that we must reconstruct the classi-

¹ Published in the *Deutsche Klinik*.

fication of tumours, inasmuch as the clinical history of tumours was only a chaos, which the microscope was destined to illumine. Now, what has the microscope done to dispel this chaos? Lately I had several tumours to remove at one occasion. I operated first on a cheloid tumour. The microscope was appealed to regarding its structure, which was one of the hardest and firmest I ever saw, and replied that it was fibro-plastic. I next removed a tumour of the lymphatic glands, and again the microscope declared it fibro-plastic. Then I extirpated a soft tumour, of the consistence of jelly, resembling those fibrinous concretions which are occasionally found in the heart after death. Still the instrument pronounced it to be fibro-plastic, and so on. A chancre is fibro-plastic, and so also are articular fungosities. The same is true of epithelial tumours;—corns of the feet, callosities (*durillons*), fungous tumours of the cervix uteri, cancers of the lip and tongue—the gravest, most certain of recurrence, and most surely fatal of all cancers—all are of the same nature,—all are epithelial tissues! Yet how different are all these growths in a clinical point of view! Is this the light microscopy has cast on the clinical chaos? and can we call it light at all? (Here M. Velpeau drew a demi-grotesque picture of the structures which the microscope exhibited in a small morsel of a tumour, which excited prolonged and hearty laughter.) “Here,” said he, “are cells of every form, dimension, and sex; parent cells, pregnant cells, and young cells; cells regularly and irregularly formed; young cells which are beautiful, chubby, and rosy; and old cells which are pale, discoloured, and withered. And all this is seen in a piece not bigger than a pin-head!

M. Robert has charged me with neglecting pathological anatomy. This will appear strange to those who know that I have made this department of science the special object of my life. I have not neglected pathological anatomy, but I have regarded it in a truer and juster light than what M. Robert and his adherents have done. Medicine existed before pathological anatomy, and the evils of diseases were known before the anatomical lesions to which they gave rise. Pathological anatomy made known to us the relations which existed between certain lesions and certain symptoms and phenomena observed during life; but it did not inform us of the *cause* of all these symptoms and phenomena; still less could it reveal to us the causes of those maladies, of which, generally, the anatomical lesions were only consecutive. Hence, it is not just to say that we must look to pathological anatomy for an explanation of the causes and phenomena of a disease.

To return to the main subject under discussion, I would say that cancer is a disease in which there exists a certain fixed element which is always identical, but which may at different times present every variety of form and aspect. Has the microscope discovered this fixed element, which is common and constant in all the varieties of cancer? Certainly not. The cell cannot be regarded as this fixed and constant element, because it is occasionally absent in true cancers, and present in non-cancerous growths. Has the microscope revealed the special character of the malignity of cancer? By no means. We have been forced to look elsewhere for the characters for which a correct classification of cancerous tumours might be founded. Clinical surgery furnishes us with these, and although they may be despised as the old-fashioned classifications, they are still preferable to those of the microscopists, which are sadly deficient in constancy or fixedness of character. We ought to distinguish those tumours which always recur and prove fatal, whatever may be our treatment (in which cases I do not operate), from those other tumours which we may sometimes hope to cure by operation, although the microscope reveals no characteristic distinction between the two classes. When the microscopists explain these differences—when they show me why one species of cancer always returns, while another only recurs sometimes,—then I will admit their doctrines, but until then, they must permit me to keep a prudent reserve, and to trust to other elements of diagnosis.

I do not, however, totally reject the microscope, as has been imputed to me.

I accept it, but only conditionally, as an accessory, which must be kept in abeyance to clinical indications, instead of being placed in superiority to them. I have only to go to my last written work, to show that "I believe the cancer-cell to be valuable in diagnosis, within certain limits. You will find that in my *Traité des Maladies du Sein*, I have written thus: "The presence of the so-called cancer-cells in a tumour which presents, in other respects, the characteristics of cancer, will tend to increase the probability of its recurrence after an operation, just as their absence will tend to give us confidence, in cases where we may judge, from the harmony existing between the tumour and the general physiognomy of the patient, that the tumour we have removed is of a benign character."

I have never been hostile to the microscopists; on the contrary, I was one of the first to welcome them, to encourage them, and to bring their experience to bear upon the results of my clinical investigations; and I have thus spoken of them in the work to which I have just alluded: "Seeing that they arrive too hastily at conclusions, they should set to work afresh, not losing sight of those notions with which the microscope has furnished them. They would thus, I firmly believe, make some other discovery, and arrive at other results still more conclusive, for the correct determination of cancers. No one desires this result more ardently than myself." Therefore, in spite of the attacks which the microscopists have directed against us—the clinical men—we welcome and do not reject them. And to them I would, in conclusion, address the words of the poet:—

"Des dieux que nous servons connais la différence,
Les tiens t'ont ordonné le meurtre et la vengeance;
Et le mien, lorsque ton bras voudrait m'annihiler,
M'ordonne de te plaindre et de te pardonner."

[We have devoted a considerable space to the condensed report of this remarkable discussion, brought to a poetical conclusion by M. Velpeau, which, though not very creditable to the scientific character of the Parisian practitioners, will, we believe, through its ultimate effects, be of great service to the advancement of medicine. The discussion is not only remarkable for its length, and the importance of the practical questions at issue, but for the unusual circumstance, that with one or two exceptions, it has been carried on by men who avowedly understood nothing of the matter. Thus it is an undoubted fact, that the value of the microscope in the diagnosis of cancer has been talked about for hours together, in one of the first medical assemblies of Europe, by men who have never used that instrument practically, and who know nothing about its application to the study of disease. The labours of modern histologists have been conversed about and reasoned upon by individuals whose speeches only serve to show that they are totally incapable of comprehending the present state of pathological histology. Not one man distinguished as a histologist took part in the discussion, and the medical press of Paris, with a single exception, has shown as much ignorance of the subject as the learned Academicians themselves. The exception to which we allude is that of the *Gazette Hebdomadaire*, which has honourably supported the claims of its conductors to a knowledge of the actual condition of medicine. Doctrines long since abandoned by their authors, and an absurd charge of histological research being opposed to clinical observation are the points on which this discussion has revolved, mixed up we must say with considerable ignorance of all that has of late been written on the subject of cancer, by everybody, except M. Lebert. Still it is a great matter to have roused the physicians and surgeons of Paris, to what is going on in scientific medicine, and although it is clear they are somewhat astonished by a state of things to which they can no longer close their eyes, much good must result from the amusing manner in which that bewilderment has been displayed. Now it is known what their difficulties are, we have every hope that with a little pains a method may be discovered

of removing them. The elder practitioners and teachers in other cities may perhaps imagine from the results of this one-sided discussion, that they will be warranted in continuing to disregard the progress of histological knowledge. But we shall be greatly deceived if, after all, it will not give an impulse to minute pathological research, and, notwithstanding the absurd denunciations of M. Velpeau, serve to draw closer together the practical surgeon and the scientific pathologist.]

TWO CASES OF SIMULTANEOUS RUPTURE OF THE LIGAMENTS OF BOTH PATELLÆ.

BY MR SHAW.

Mr Shaw exhibited a cast of both knees of a gentleman of middle age and height, and of light active figure, who, last Christmas, had torn the ligaments of both patellæ from the tubercles of the tibia at the same time. The accident occurred when he was coming down two low steps in a hall, from the heel of his thick shoes catching on the edge of one of them; and he was sensible that the tear took place before he came to the ground, or, as he said, "in the air." Both legs were doubled up under him; and it was only by taking them with the hands that he could straighten them. For a fortnight he kept the sofa, with the legs extended, but without bandages or splints. He was then instructed to sit on a high chair and swing both legs, and to spend part of the day sitting with his knees bent, and pressing down the patellæ with the palms of his hands. In a month he began to walk with caution.

The following were the appearances of the knees ten weeks after the accident, when first seen by Mr Shaw:—

It may be premised that the same description answers for both joints, only the right was slightly worse than the left. The first peculiarity observed as the patient sat with the knees bent, was an unnatural prominence of the patellæ, caused by that bone riding on the upper surface of the lower head of the femur. In the normal condition of the knee when bent, the patella is drawn down to the front; its apex pointing to the ground, and its long axis in the same line with that of the tibia; but in the patient it was not so drawn down; its apex pointed forward, and its long axis was in the line of that of the femur. On examining next by the touch, there was found, immediately below the apex of the patella, a circumscribed, irregularly globular, firm mass, the size of half a large walnut; and beneath that, as far as to the tubercle of the tibia, was a depression into which the points of the fingers could be deeply inserted. The shape of the tubercle of the tibia was distinctly defined, and no trace of the remains of the ligament attached to it could be felt; whence it was concluded that the ligament of the patella had been wholly torn from it; and that the firm mass attached above to the patella was the ligament contracted and altered in figure by fibrinous deposit. To ascertain the extent of retraction of the patellæ, Mr Shaw compared the measurements in the patient's knee with corresponding ones in his own. In the extended position of the joint the distance between the apex of the patella and the tubercle of the tibia was, in the patient, two inches and a half; in himself one inch and a half; excess in the patient's knee, one inch. In the bent position of the joint the distance between the two points was, in the patient, three inches and a half; in himself, two inches; excess in the patient's knee, one inch and a half. When the patient sat, and exerted the extensor muscles to lift a weight, say a footstool, with the point of his foot from the ground, the action caused the patellæ to be jerked about an inch upwards on the thigh, above the proper articular surface, and with a sudden pain which obliged him quickly to replace the bone by pushing it downwards. At these times the lower head of each femur was so much bared by the slipping up of the patellæ, that, for the moment, the form of the condyles and trochlea in front and above, was visible through the skin and capsule. For five inches above the knee there was considerable wasting of the fibres of the quadriceps extensor muscle.

Notwithstanding the serious nature of the injury to both knees, the patient was not so lame as might justly be expected. Being a member of Parliament, and in a high position at the bar, he has pursued his active duties for some time without accident. In standing, he has a sense of inclining to fall backwards; yet one night lately he was able to address the house for an hour and a quarter, without fatigue. In rising from his seat he usually begins by extending his legs, then he gives his body an upward jerk with his arms, and so lifts himself on his feet. On level ground he can walk a distance of two miles; he fears crossing a street, or being jostled in a crowd, as he cannot easily increase his pace or recover his balance. On going up stairs he is obliged to straddle; and having thus extended one leg, he swings it up one step, and then repeats the same act with the other. In descending, he prefers going backwards; but if he cannot do that, he manages to straighten the legs, and puts one down cautiously after the other. To get out of a railway-carriage, he seats himself on the floor at the edge of the door, and letting his legs fall straight, slips down, and poises his body upon them as his feet reach the platform.

Mr Shaw related a second case, recently under his observation, of simultaneous rupture of the ligaments of both patellæ; but where the laceration, instead of being from the tubercles of the tibia, was from the apices of the patellæ, or a little below these points. It occurred in the person of a medical friend, æt. 45, five feet ten inches in height, fourteen stone in weight, but powerful and of active habits, residing in Ayr. The accident took place in January 1853, from the heels of his boots catching on the edge of a low step on the outside of a door, when leaving a patient's house. In falling, he heard no snap, but felt a sharp sudden pain; and when on the ground, found his legs doubled under him. To extend them, he had to drag his feet from below him with his hands. Being unable to rise, and alone, he gave the alarm by beating the door with his fists; and on its being opened, he moved himself backwards into the hall by using his arms like crutches, and dragging his legs extended after him. In the left knee the patella was drawn up two inches above its proper level; in the right, the retraction was not so great. The treatment was that usually adopted for fracture of the patella. He found advantage from applying above the bone a thick compress of Indian-rubber, of a crescentic and wedge shape, connected by straps to a laced stocking on the lower part of the leg; from the surface of the Indian-rubber clinging to the skin better than other substances, it could be secured in its place without tight bandaging. In two months he was able to walk, with the assistance of sticks, on level ground; and was not long in resuming his professional occupations. Mr Shaw first saw him in December, eleven months after the accident. Here, as in the former case, the description of one joint will serve for that of both. When the knee was bent, so that the leg was at right angles to the thigh, the patella projected considerably, so as to cause a visible alteration in the natural contour of the joint; again, instead of the long axis of the patella being directed perpendicularly downwards, the position which it ought to occupy in that condition of the joint, its direction was oblique, or that which belongs to it when the joint is bent only to an angle of forty-five. The shape of the apex of the patella was clearly defined; continuous with it, in a uniform line, and without perceptible depression or elevation, the ligament of the patella could be distinctly traced, of its usual thickness and strength, to the tubercle of the tibia. The patient stated that for several months after the accident, a circumscribed firm nodosity, like a callus in fracture, existed about half an inch below the apex of the patella; but the traces of that had disappeared. In the extended condition of the joint, the distance between the apex of the patella and the tubercle of the tibia measured two inches and a half; which, on comparison with Mr Shaw's knee, gave an excess of an inch to the patient's; and in the extreme bent condition, the distance measured three inches, which, by a similar comparison, gave an excess also of an inch. For five inches above the knee, there was distinct atrophy

of the quadriceps extensor muscle. On level ground he walked securely, taking short steps. He was sensible of weakness and danger on going down a declivity; and in descending stairs he had to hold one of the rails with both hands stepping down sideways. He has met with only one fall, which occurred when he was getting over a puddle; he felt himself losing his balance, and instead of making an effort to recover it, he allowed himself to come to the ground. He has abstained from using knee-caps or bandages, being afraid that their pressure would cause greater wasting, and consequent weakness, of the extensor muscles.¹

Remarks.—The only case which Mr Shaw has witnessed bearing a resemblance to the first related, was that of a lad, *set.* 17, who, having a stiff bent knee, with spurious ankylosis, fell, and tore the ligament of the patella from the tibia; but on close examination, Mr De Morgan, under whose care the patient was, found that the tubercle had been separated along with the ligament from the tibia. In this case it was concluded that the tubercle, which is an epiphysis, had not been firmly united to the tibia; and that it was owing to that, and the immobility of the patella, that from the violence directed on the front of the joint, diastosis of this tubercle took place. Union occurred as in common fracture.

It is to be regretted that, in the first case, the treatment was not such as to have afforded an opportunity of the ends of the ligaments becoming united to the tubercles of the tibia. Besides the absence, at first, of proper measures to keep the legs securely fixed in the extended position, passive motion was adopted much too early; and to add to the evils, the patient wore a loose elastic knee-cap, the edges of which, from its being too short, indented into the hollow space between the remains of the ligament above, and the tubercle of the tibia below, so as effectually to separate the surfaces which ought to have been in contact.

To explain how the patient, notwithstanding he was deprived of the use in both legs of such an important part as the common insertion of the quadriceps extensor muscle into the tibia, retained so much power of extending the limbs, it requires to be remembered that, owing to the general attachment of the patella to the front part of the capsular ligament of the knee-joint, all those parts of the capsule which pass down from that bone to the head of the tibia, may become substitutes for the ligamentum patellæ, and, in a more or less perfect manner, perform its offices. In this respect, the case is very different between rupture of the ligament of the patella and that of the tendo Achillis.

—*Trans. of Patholog. Soc. of Lond.*

DR CROKER KING'S CASE OF DISLOCATION OF THE LOWER JAW, UPWARDS AND BACKWARDS.

On the 3d of September, 1851, at three o'clock in the afternoon, a boy named Peter M'Dermott, aged eight years, in endeavouring to climb over a wall composed of loose stones, fell, and was struck in the face by a large stone.

On being lifted up, it was observed that he was severely injured, that his features were much distorted, and that blood flowed from his left ear. Surgical

¹ On a visit to Scotland in July of the present year, eighteen months after the accident, Mr Shaw saw this patient again. There was no increased elongation of the ligaments of the patellæ; and a manifest improvement had taken place in regard to the strength of the quadriceps muscles. Although still lame, and obliged to be cautious, he felt himself greatly more secure on his limbs. He attributed the improvement principally to the frequent exercise which he had been able to take, in the intervals of professional occupation, at the national game of golf, played on a ground where falls would not be dangerous, and walking required unwonted effort; namely, on low hills, or domes of blown sand, which lie along the coast in the neighbourhood of his town.

assistance was immediately obtained, and repeated attempts were made to relieve the patient, but being ineffectual, he was brought to the Infirmary, and was first seen by me at half-past ten o'clock at night, seven hours and a half after the receipt of the injury. The deformity was remarkable and gave an uncommon cast to the countenance; this was evidently caused by the peculiar position of the chin, which was depressed and retracted. The parents were greatly distressed at the accident which had happened to this their favourite child. The father prophesied that he would be an object for life; the mother said his face resembled a pig's, and declared that if the deformity could not be removed, she would far prefer that her child should die. The following was the state of the case when admitted into the Infirmary. The chin was much retracted; the mouth was open, and saliva poured from it. Articulation was so indistinct, that what the boy attempted to say could not be understood. The chin was pointed to the left side, and on pressing it upwards the patient appeared to suffer much pain, but the mouth could be closed; and it was now observed that the internal tubercles of the inferior molar fell outside of the external tubercles of the corresponding teeth of the upper jaw, and that the lower incisors were at least one inch *behind* the upper. No fracture, through assiduously sought for, could be found, and the appearances were very different from those which I would have expected to meet in any form of dislocation with which I was acquainted. The pointing of the chin to the left side, and the position of the inferior molars, might occur from dislocation of the right condyle in the usual direction forwards; but the injury was altogether confined to the left side; and moreover, the right condyle lay undisturbed in its socket; and lastly, in the event of the above luxation having occurred, the chin would be unusually prominent, and the lower incisor teeth in *advance* of the upper, whereas in this case they were situated one inch *posterior* to them.

The diagnosis of the case appeared to be surrounded by unusual difficulties. So much so, that I came to the conclusion, that it would be injudicious, and perhaps injurious, to attempt mechanically to afford relief, until I had formed some definite notion of the nature of the injury. On maturely considering all the circumstances of the case, I formed the opinion, that in the absence of fracture of the ramus or neck of the bone, the peculiar position of the teeth could only be produced in the event of the left condyle of the jaw having left its socket, and having passed in a direction upwards and backwards. This conclusion was at variance with my former experience, and with the records of surgery. My anatomical knowledge arose in rebellion against it; and I proposed to myself the question, how could such a displacement occur? A moment's consideration satisfied me that I had possibly solved the difficulty. It occurred to me that the condyle might pass in the above direction, if the dislocation were preceded by a fracture of the inferior maxilla, near or at its symphysis; this would allow the arms or sides of the bone to separate, and thus permit the condyle to be driven, in the first instance, outwards, and subsequently, upwards and backwards.

I now returned to the case. On examining the line of the lower teeth with greater diligence, it still appeared quite perfect; no irregularity or fracture could be detected; however, on grasping the bone on each side of the symphysis, although there was not any apparent displacement, nevertheless I could detect a hinge-like motion, which enabled me to separate the curved sides of the jaw from each other, just as if they were united by cartilage. I now felt confident that the diagnosis at which I had arrived, by a process of reasoning, was the correct one, and I proceeded to treat the case accordingly.

Having wrapped a napkin around the thumb of my right hand, I placed it as far back as I could on the inferior molar teeth of the injured side, and exerted pressure in a direction downwards, at the same time that I drew the jaw forwards. I distinctly felt the bone to descend, to move forwards, and then with a jerk and a snap the condyle to re-enter the glenoid cavity. In an instant all

deformity was at an end, the teeth fell into their proper position, the mouth closed, the child could articulate distinctly, and the peculiar expression of countenance vanished.

Remarks.—It may be asked what was the nature of the injury of the lower jaw which caused it to yield with a hinge-like motion; was it what has been described as the "green stick fracture;" I should say it was not, as in the latter case of incomplete fracture, the injury would, in all probability, have been apparent in a bone so lightly covered as the inferior maxillary. But might it not have been that the union of the lateral halves of the bone, which usually takes place within the first year after birth, had not yet been accomplished?

It is not easy to conceive how violence applied, as in the above case, should have caused this peculiar displacement, which appeared for its accomplishment to have required so many impulses, all acting in methodical succession; first the forcible expansion of the parabolic curve of the inferior maxillary bone; and secondly, the upward and backward direction of the condyle. But when we consider the extraordinary and eccentric effects which occasionally result from external injury—take, for example, gunshot wounds, where the projectile has been known to pass into the most unexpected and remarkable positions—we need not feel surprised at the nature of the above injury, unique though it be.—*Dublin Hospital Gazette.*

DR HENRY'S CASE OF NECROSIS OF THE LOWER JAW.

April 21, 1854, I was requested by Dr Garner, an eminent dentist of Burlington, Iowa, to visit, with him, John M'Callon, four years old, who was suffering from some disease of the face.

I found a boy, born of healthy parents, who had always enjoyed good health himself, had consequently taken no medicine, and had *been well* until last October, when he was troubled with toothache, from which time he has been more or less ailing. As far as I could gather the history of the case, it was this:—An abscess formed in the alveolus of the root of the first molar tooth, which was not opened, and the tooth had not been removed as soon as it should have been; from that time a foetid discharge of matter has been kept up, destroying the appetite of the boy, and affecting his general health.

The tumour of the jaw had been gradually increasing until the left side of the face was at least three times as large as its fellow; the swelling had extended to the temple, so that it became a question whether it was not an encephaloid or other cancerous affection of the bone; the matter was discharging from the rest of the bicuspid and first molar teeth, which had been removed, and also from a sinus at the angle of the jaw. I found that, by strong traction with forceps, slight motion could be perceived between the upper and lower portions of the tumour, which made the impression upon my mind that the disease was limited to the alveolar process; but, on cutting down to the jaw, through the alveolus, we found that the bone itself was involved, and, on completing the section of the maxilla, that there was healthy bone beneath it, closely approximating to it—in fact, forming a shallow groove for the reception of the old maxilla. After removing the remaining teeth upon the left side, and several splinters of bone, we concluded to await the efforts that nature was making to throw off the old bone and replace it with a new formation. I prescribed chloride of soda and other detergent washes, to destroy the foetus and to cleanse the parts. This they partially accomplished; and, on the 9th of June, we concluded to remove the diseased portion, and, if necessary, the entire jaw.

The mouth being held open with cork, we were enabled to proceed without any disfigurement of the face from external section, by cutting down upon the bone from within, and grasping it firmly with forceps just in front of the angle, lifting it cautiously and steadily upwards until the maxilla was detached from the groove it was in, and then, using forcible traction forward, the whole bone from the articulation, with condyle and coronoid process, was removed as far forward as the prior section. The bone was bathed in a thin, unhealthy pus; the suppurating

process had entirely denuded it of periosteum, and every other attachment to the soft parts, except the portion of gum and mucous membrane which lay over it, and through which the section had been made. We then removed the portion of jaw in front, including the symphysis mentis, and extending four-fifths of an inch to the right side; but little hæmorrhage followed the operation, and this entirely of a venous character; the secretion of pus diminished immediately, and ceased entirely in a few days.

At this time, the boy has perfect use of the jaw; the articulation is a little larger and higher than usual, *but a new bone has formed from the articulation on the left side to the point on the right side where the necrosed portion was separated from the healthy bone.* With the exception of the want of teeth, the new bone is as serviceable, and, apparently, as strong as the old one, and having as perfect motion in the articulation; within a week, a tooth has shown itself on the left side, corresponding to the lateral incision, but it is small, and not very firmly fixed, and has the appearance of being an appendage to the periosteum, rather than that of a tooth with a regular socket.

I exhibited the removed maxilla, at the annual meeting of the Iowa State Medical Society, to many physicians and surgeons of the State. It is a perfect jaw, not splintered or atrophied at all, excepting slight absorption of the condyle; it had very much the appearance of a bone just taken from the macerating tub, after six weeks' exposure to heat and moisture. The new bone, I presume, was secreted by the periosteum of the old, during its struggle between life and death; and, in this beautiful manner, a most admirable substitute has been provided for a necrosed portion of the body.

The horizontal position of the ramus in the young subject facilitated very much the extraction of the bone without external section, and permitted it to be withdrawn in a manner that would have been impracticable with the adult.

The boy is now in good health, and masticates his food without difficulty; in fact, the only inconvenience that he experiences is from being compelled to chew upon one side alone.—*Amer. Journ. of Med. Sciences.*

DR STEPHENSON'S CASE OF EXOSTOSIS OF THE ORBIT.

Mrs S. W., æt. 18 years, of good constitution, three years preceding the date of consulting me, perceived for the first time a slight enlargement of the bone immediately beneath her eyebrow, which continued gradually to increase year after year, but without any pain or constitutional disturbance. When I first saw her, it was over two inches in diameter at its base, and hung down like an inverted cone over the eye. It completely counteracted her vision, excepting to a very limited extent; she could discern objects at her feet, or anterior to her, when the head was thrown backwards. No satisfactory cause could be assigned for it, and she has never experienced any inconvenience from it excepting its mechanically obstructing vision.

I at once informed her that nothing short of an operation could remedy her difficulty; she gave her consent, and fixed upon a day for its removal. The operation was performed on the 30th January 1850, in which I was assisted by Drs Buck, Stone, and MacFarlain.

The patient was put under the influence of chloroform, after which a straight incision was made over the superciliary ridge extending from the internal to the external angular processes of the frontal bone; and from the centre of which a vertical incision was made at right angles with the first, and nearly of the same length—dissecting up these gave us a triangular opening. The next step in the operation (after denuding the bone of a portion of its pericranium) consisted in removing the bony tumour. This was done by the use of Hey's saw, but, owing to the hardness and thickness of bone, it required some time to reach the frontal sinus, after which, with the aid of a strong cutting bone-forceps and an elevator, the bony prominence was removed, exposing to view a portion of the lachrymal gland. The lining membrane was found partially diseased, and a very small fungus attached to it. The parts

were then cleansed, and the flaps of the wound secured by interrupted sutures, adhesive plasters, etc. The integuments united by the first intention, excepting at the internal angles of the wound, which remained a fistulous opening for eighteen months, during which time it continued to emit a muco-purulent discharge. This, doubtless, was owing to a chronic inflammation of the mucous membrane lining the inferior surface of the sinus.

Although the patient was *enceinte* some four months, and was under the influence of chloroform for nearly one hour, yet no unpleasant consequences resulted from it, or the operation; and in due time she was delivered of a healthy living child.

The ligament through which the tendon of the *lavator palpebræ superioris* passes being attached to the orbital portion of the frontal bone, I was fearful that the functions of the muscle would be destroyed, but the movements of the superior palpebra is as perfect as it ever was.

It is now over four years since the operation was performed, and there is not, at this time, the slightest evidence of disease remaining, and little or no deformity; nor has there been for the last three years.—*Amer. Jour. of Med. Sciences.*

PROFESSOR LANGENBECK OF BERLIN ON THE RADICAL CURE OF HYDROCELE BY INJECTIONS OF CHLOROFORM.

Not being satisfied with the effects of tincture of iodine as an injection in cases of hydrocele, the above surgeon has recently been employing chloroform as a substitute for it with excellent results. He finds that it acts in the same manner as Tinct. Iodinei, but that it sets up adhesive inflammation more quickly and more surely than that remedy. After withdrawing the fluid of hydrocele, he injects 5i or 3iiss of chloroform through the canula, and after allowing it to remain for a short time in contact with the tunica vaginalis, he allows it to escape, so as to avoid its absorption. The pain occasioned by it is not intense,—even less so than that induced by iodine,—and it is not of long continuance. Langenbeck, in his paper, relates the particulars of four cases treated thus, in all of which a complete and radical cure was accomplished in from two or three weeks.—*Deutsche Klinik.* 31. 1854.

THERAPEUTICAL NOTES.

I. LUPULIN AS AN ANAPHRODISIAC.—Lupulin, or the active principle of hops, is possessed of powerful sedative effects on the generative functions. This was first ascertained by Debout, but more recently Zambaco has published a paper on its anaphrodisiac virtues (*Bull de Therap.*, 30th August 1854), in which he more than confirms this author's observations. Zambaco administered the medicine in doses varying from 1 to 16 grammes, and he never found sickness or constitutional disturbance attend its use. He has recorded the history of eight cases of painful erections, following gonorrhœa, in which it was most successfully employed as a sedative. He affirms that in four-fifths of the cases it allays the morbid erethism, and prevents chordee. It answers this purpose much better than camphor, which often irritates the digestive functions, and fails to produce the desired effect. Besides being possessed of sedative and anti-blenorrhagic properties, which depend on its essential oil and resinous principle, lupulin contains a bitter element which acts as an admirable tonic. Zambaco has seen lupulin given, as a tonic, to strumous patients, with the best effects, the appetite becoming improved, and the digestive organs strengthened.

II. ON THE EXTERNAL USES OF CONEIN.—Conein (or Conia) as most of our readers are aware, is an oleaginous fluid, which is the active principle of hemlock. Murawjeff, a Russian physician, has found it to be efficacious in the following diseases:—(a). In *chronic skin diseases* it allays irritation and favours the cure. He has used it successfully in eczema, psoriasis, acne, lichen, psora, prurigo, etc. He recommends it in favus, and regards it as a specific for that disease

when affecting the scrotum. (b). In *toothache* one drop allays the pain more rapidly and effectually than even chloroform. (c). In *neuralgia* and *sypilitic pains of the bones*, he alleges that it is not a mere palliative but a radical cure. (d). In *chronic synovitis* its use is beneficial, as also in allaying the pain of (e) *incised and gunshot wounds*. (f). In *scrofulous and rheumatic inflammation of the eyes* it speedily diminishes the severity of the symptoms. (g). It palliates the pain of *scrofulous and cancerous ulcers*.

Mode of its Employment.—In *skin diseases* an ointment is used composed of from 12 to 24 drops of conein to 3i. of ung. simpl. or cold cream. The diseased parts having been first rubbed with soft flannel, the ointment is applied to them, and they are then covered with oiled silk and a bandage. In *neuralgia* he first washes the part affected with spirits of wine, and then applies to it 3 or 4 drops of pure conein, covering it thereafter with oiled silk and a bandage. In *cancer* he mixes the alkaloid with mucilage, and applies it to the diseased surface with the finger, previously covered with a glove. For a *collyrium* he adds 1-3 drops to 5vi. of Aq. destil. and 5ij. of decoct. cydonii; and when used as an *enema* he administers 2 or 3 drops in starch emulsion.—*Med. Zeit. Russl.* 17, 1854; and *Schmidt's Jahrb.* 1854, No. 9.

III. ATROPIN IN EPILEPSY.—Dr Lange of Königsberg tried this remedy in 9 cases of epilepsy, in 3 of which the patients were males, and the remainder females. The 3 male patients who had been epileptics for years, were cured after its employment for periods varying from 3 to 6 weeks. Among the women 2 cases remained uncured; 1 patient died; and 3 seemed cured, as they had been free from attacks from 5 to 11 months. The dose used was scarcely $\frac{1}{10}$ th of a grain.—*Deutsche Klinik*, 10, 1854.

IV. INHALATION OF THE FUMES OF OPIUM IN CORYZA.—Dr Lombard, of Geneva, has found that in those severe cases of coryza which are accompanied by great pain and sense of weight in the frontal sinuses, the inhalation of the fumes of burnt opium afford the patient the most marvellous and speedy relief. The pain¹ ceases as if by enchantment, and the patient passes from a state of misery into one of comfort. Dr Lombard recommends a few grains of powdered opium to be thrown upon a slip of metal, previously heated in a spirit lamp; and the patient is desired to hold his head over, and forcibly to inhale the fumes of the drug.—*Gaz. Méd.* July 1854; and *Bull. Gen. de Thérap.* 30th Aug. 1854.

V. VAPOUR OF IODINE IN MAMMARY TUMOURS.—M. Eichmann relates the following case:—A healthy woman, æt. 47, none of whose relatives had ever had cancer, received a slight blow on the breast. This occasioned a hard and mobile tumour. After an absence of the catamenia for 17 months, the tumour became painful; smaller glandular enlargements appeared on the breast and extended to the axilla; lancinating pains were felt; the skin became adherent to the tumour, and the sebaceous follicles were distended with blackish matter. The patient refusing to submit to extirpation, M. Eichmann applied to the tumour a bag filled with tow, and containing also iodine, which was retained *in situ* by means of adhesive plaster. The iodine was renewed fortnightly. After wearing this bag for a month the patient was greatly improved, and after the lapse of 17 weeks the mammary enlargement had completely disappeared.—*Gaz. des Hôpitaux*, 19th Dec.

VI. DIGITALIS POMMADE IN CASES OF HYDROCELE.—Last spring an Italian surgeon named Bellucci published accounts of 5 cases of hydrocele cured by

¹ We ourselves can confirm this, having recently tried Lombard's plan with complete success. We sprinkled a pinch of powdered opium upon a kitchen shovel heated to redness. The odour is unpleasant, but the patients are by no means sensitive in their smelling powers.—*Trans.*

the external use of digitalis. Little attention was paid by surgeons to these results; but very recently M. Laforge, chief surgeon in the *Hopital de la Grave*, at Toulouse, has made trial of the new mode of treatment, and found it to be as efficacious as was alleged by M. Bellucci. A man, æt. 60, had a large hydrocele of the right testicle, and being unwilling to submit to the usual operation, besought M. Laforge to try some other method. Daily friction of the tumour was ordered with the following ointment:—R. Pulv. folior. digitalis, 6 grammes axung, 30 grammes; *misce*. The patient was also ordered to wear a suspensory bandage. In a few days the man presented himself completely cured. He had enjoyed excellent health during the process, and had used, in all, 18 grammes of the powder of digitalis.—*L'Union Méd.* 30th Sept. 1854.

VII. CYANURET OF MERCURY.—M. Desmartis, of Bourdeaux, after a careful comparison of the effects produced by the different preparations of mercury, has come to the conclusion that the cyanuret is superior to all others, especially in syphilis. He believes it to be at the same time efficacious and innocuous in its action; he never saw it occasion salivation or any intestinal irritation; and often when all the preparations of the metal had failed to produce benefit, he has seen it restore to health patients whose cases seemed hopeless in the extreme. He has found its use to be efficacious in certain cases where the patients had suffered for a long period, obscure pains, for which no cause could be discovered. He has employed it with benefit in iritis, and in syphilitic affections of the nose and fauces.

VIII. APTITUDE OF INDIVIDUALS FOR ANÆSTHESIA BY CHLOROFORM.—M. Ancelon read a memoir on this subject to the *Academie des Sciences* on the 9th of October last. The following are his conclusions:—

1st, The aptitude of subjects for anæsthesia is in a direct ratio to the degree of emptiness or fulness of the stomach. Its action is more rapid, and more innocuous when the stomach has been long empty, and when, consequently, absorption is more active.

2d, It is also in relation to the rapidity and action of the digestive processes in the individual.

The medium dose, according to M. Ancelon, is ʒij ; and the quantity to be used at the longest operation should not exceed ʒv . (!) He thinks we should only add 3 or 4 drops at a time when we are increasing the dose of the anæsthetic.—*Gaz. des Hôpitaux*, 21st October 1854.

The Edinburgh experience of chloroform shows that no such extreme caution is requisite in its administration, as M. Ancelon recommends, and that in doses much larger than those indicated it is perfectly harmless and beneficial.—*Translator*.

Part Fourth.

MEDICAL NEWS.

THE INEFFICIENT STATE OF THE PUBLIC MEDICAL SERVICES IN GREAT BRITAIN—AND ITS REMEDY.

If ever there was a period when the importance of medicine in its relation to the welfare of nations, is capable of being impressed on the public mind, it seems to be the present. We can no longer have any doubt, that a noble army, equipped in every essential for mere warlike purposes, has in a few months melted away to one-fifth of its original strength, principally from the effects of

disease. It is also certain that these effects might, to a great extent, have been avoided by a proper staff of medical men, properly provided with medicines, diet, and hospitals. The British nation has had the humiliation of seeing that, whilst the medical arrangements of its army has presented nothing but confusion and inefficiency, the French army by its side has been supplied with active medical officers, abundant medicines, and every comfort. After a glorious battle in which both armies were engaged, whilst the British wounded soldier was obliged to remain for days on the spot where he fell, his French comrade was conveyed in a spring cart or ambulance to a general dépôt, where an efficient staff of medical officers attended to his wants. Whilst the British army was shivering for want of clothing, the French army, out of its abundance, furnished it with 12,000 great coats. Whilst the British soldier was starving on biscuit, a quarter of a pound of salt pork, and unusable green coffee, the French soldier was enjoying abundant rations. The very transport of the wounded over a few miles of country to be embarked for a distant hospital, could not have been accomplished without the aid of mules and panniers for the sick, supplied for that purpose by our allies. Then the conveyance of these wounded from Balaklava to Constantinople, reminds us of the horrible accounts we have read as occurring on board slavers, more than of anything else,—five hundred men, suffering under various kinds of wounds, some sick and others affected with cholera, packed together in a single vessel, rolling on the sea, and attended by some one or two assistant-surgeons. We read of amputations, performed on account of frost-bite, although stoves were abundant, but could not be obtained; of hundreds sinking from diarrhœa, when the medical attendants could not procure the most simple remedies. The result is that, while the French army is strong, vigorous, and prepared to reap the glory of a successful enterprise in war, the British army has given up to its allies the defence of its batteries, and been converted into a rear-guard, to watch the progress of events.

The real cause of all these disasters is, in our opinion, the inferior position occupied by the profession of medicine in this country, in its relation to the state. The medical directors of the army are mere no-bodies, unregarded and unhonoured by the other departments. Here they are embarked without stores—there censured without cause. In the army a physician or surgeon holds a very subordinate position to his equal in rank who is a mere fighting man. Dr Wilson, who rallied a few men to repel a charge in a skirmish, was lauded to the skies, whereas the skilful exercise of his profession whereby thousands of lives might have been saved, would never have gained him the slightest attention. In the navy medical men are so treated that scarcely any man of education and knowledge of his profession will enter it. It is a service completely *tabooed* to men of ability and distinction, and we consequently read of a long list of ships in commission with not one-half of the necessary complement of surgeons. To supply the deficiency, our Royal Colleges have remitted some of the studies of students, so that the evils of an improper administration, are sought to be counteracted by diminished professional education. In short, these institutions look after their petty interests, quarrel with one another, oppose every plan having for its object a national rather than a local policy, and so assist in rendering the present confusion worse confounded.

The only remedy for the cure of the evils under which the profession and the country groan, is the introduction of a national system of medical reform, by a minister who will think only of the public good: Government ought surely to see by this time how essentially the efficiency of an army is dependant on judicious medical superintendence, and that medical influence should be superior to that of the commissariat, rather than the contrary. What is required, is the introduction of a simple state mechanism, similar to what prevails in France, whereby the education and examination for medical practice shall be equalized throughout the empire, and whereby the privileges of the qualified practitioner shall be everywhere the same. It must

be clear that a young man well educated to practise medicine, is as fit to treat a soldier or sailor, as a civilian. What then can be the use of extra army and navy boards; such a one also could practise as well on one side of the Tweed as on the other. Away, then, with your local collegiate privileges. Let our examiners be chosen, because they are qualified for the duties they have to perform, and not because they belong to this or that party, and let those who best distinguish themselves at such or other examinations, receive the medical appointments of the country, whether civil or military. On the other hand, let the whole system of job, which now infects the entire profession, be abolished, and let the claims of merit and hard work have fair play. Let good judgment, sound knowledge, and mental activity be the characteristics of the heads of our medical departments, and we will venture to say, no commander-in-chief, no commissary-general, and no slavish system of routine, will be allowed to produce those evils which the nation has now to deplore—evils, however, which are not confined to its military system, but exist in every public department where medicine is concerned. For our own part, we can scarcely conceive the possibility of a really eminent chief of the medical department, permitting such melancholy disorder in affairs purely professional, as have existed in the Crimea. Can we imagine a man like Larrey being controlled in his own department, even by such a general as Napoleon? No, and the attempt failed. Nor would it have been possible for a great and sagacious physician or surgeon to have embarked unprepared with the troops from Varna, even though commanded to do so by Lord Raglan, or even by a Mr Commissary-General Fidler.

These remarks, it will be readily seen, are of much wider application than to the state of things in the Crimea, and extend equally to the health of our towns and rural population. To this view of matters we may, perhaps, take another opportunity of adverting.

ON THE PROPRIETY OF TEACHING THE GENERAL LAWS OF PHYSIOLOGY AND OF HYGIENE IN OUR PUBLIC SCHOOLS.

At a meeting of the Governors of George Heriot's Hospital on the 8th ultimo, it was moved by the Rev. Dr R. Lee—"That a Committee be appointed to consider the propriety and practicability of instructing the boys in the Hospital and the scholars in the Foundation Schools, in physiology and the laws of health, and other matters connected with the physical wellbeing of the community." In support of this motion, the Rev. Doctor said that it had long appeared to him, and indeed to every one who paid any attention to the subject, that the misery which oppressed so large a portion of our poorer citizens and countrymen arose from causes which might be either removed or at least greatly mitigated. It arose evidently in a very large measure, from their ignorance—their ignorance of those things on which their physical wellbeing depends. The question he seriously pressed upon the Governors was, whether, with the means in their possession, they could do nothing to teach those thousands of the community who attended their schools how to prevent diseases—how to preserve that health which, however precious to all, was to the working-man even more precious than to others. Every boy and girl before leaving the school might be taught so much of the structure and functions of the human body as should give them a good notion of the house they live in, and they might all be made familiarly acquainted with those conditions which are indispensable to health, as also with those which are deleterious and fatal to it. They should know—and therefore we should let them know—what are the laws which God has ordained for them to observe in relation to health, and which they cannot violate without incurring the penalties which He has also ordained. Was it not strange that we should pretend to educate youth without informing them regarding the effects of bad air, bad water, want of cleanliness,

intemperance, and other things which are the great springs of those maladies from which the community suffers so much? Thousands of the most promising youths could testify that they had been taught everything but what concerned most their health, comfort, and life—of all which they paid the penalty through that ignorance. He was of opinion that the education of the girls attending their schools should engage particular attention. It was notorious that the ignorance of the female part of the community was the cause of the most wide-spread and dreadful evils. Not one woman in twenty knew how to manage a labouring man's house or cook his victuals in an economical and comfortable manner. All was discomfort, mismanagement, and waste. The consequence was that whisky was resorted to, to make up for the want of nutritious food and to satisfy the craving which that want created. This was the standing cause of drunkenness; and if they could succeed in diffusing among the thousands of girls, who will soon be wives and mothers, a knowledge of the proper methods of cooking, washing, and rendering a cottage a comfortable abode, they would supply one of the most effectual checks to intemperance and all its train of vices and miseries, and would perform a service of inestimable benefit to the community. The same ignorance showed itself in another way which was most distressing. The mismanagement of themselves and of their infants by the class of women now alluded to was such as to produce a mortality frightful to contemplate, and an amount of suffering on which they should not look with indifference. The proportion of children who die in the crowded habitations of this city, and he believed of all other cities, and generally among the poorer classes everywhere, is such as may well excite compassion. He did not believe that any amount of intelligence would be able to prevent this entirely, but he felt sure that it might and would prevent the greatest part of it. Some persons, he understood, thought this kind of knowledge could not be made intelligible or interesting to children. The sufficient reply was, that it had been made, and was daily made, both the one and the other. If, indeed, you should give lectures on physiology, full of hard words and long disquisitions, you might save yourselves the trouble. But children, though deaf to words, are quick-sighted for things; and if you will exhibit to them the objects which you wish to instruct them about, you will find they will look very earnestly and listen very attentively to your explanations. It would be easy to produce a great mass of evidence to show that such children as attend the more advanced classes in the Heriot Schools can both comprehend this sort of instruction, and that they greatly relish it; but he would content himself with reading a document which would settle that question; it was signed by all the most eminent Professors and medical men in London, including Sir James Clark, Sir Henry Holland, Dr Forbes, Dr Paris, Professor Owen, Dr Southwood Smith, Dr Todd, and a whole list of great names:—
 “Medical Opinion on the Importance of Teaching Physiology and the Laws of Health in Common Schools.—Our opinion having been requested as to the advantage of making the elements of human physiology, or a general knowledge of the laws of health, a part of the education of youth, we, the undersigned, have no hesitation in giving it strongly in the affirmative. We are satisfied that much of the sickness from which the working-classes at present suffer might be avoided: and we know that the best-directed efforts to benefit them by medical treatment are often greatly impeded, and sometimes entirely frustrated, by their ignorance and their neglect of the conditions upon which health necessarily depends. We are therefore of opinion that it would greatly tend to prevent sickness and to promote soundness of body and mind were the elements of physiology, in its application to the preservation of health made a part of general education; and we are convinced that such instruction may be rendered most interesting to the young, and may be communicated to them with the utmost facility and propriety in the ordinary schools by properly instructed schoolmasters.” He hoped that such a testimony as this, coming from sixty or seventy of the most eminent scientific men in Europe, so clear, strong, and

unequivocal, would make a very deep impression upon the Governors of George Heriot's Hospital.

Dr Lee's motion was unanimously adopted, and a large Committee appointed.

[We are of opinion that such kind of instruction would be as useful among the higher as among the lower classes. The ignorance of the laws of health is the same, but the desire for information being greater, they are led to adopt any plausible theory presented to their minds. They consequently become the patrons of every species of quackery, and thereby not only inflict an amount of physical suffering on mankind, but by fostering deceit and other vices, give rise to an amount of moral evil that is too great to be readily appreciated.]

RECENT EXAMINATION AT THE INDIA BOARD.

We are, like some of our contemporaries, much surprised at the result, not to say the failure, of the first examination of candidates for the medical service of the Honourable East India Company. We have always looked upon that service, not only as the best *paid*, but the best *pensioned* service in the wide world. How, then, has it happened that, for thirty appointments to be filled up, only twenty-eight candidates have appeared? We see that a report of the examinations has been laid on the table of the House of Commons, which does not throw much light upon this subject; we are therefore left to conjecture that the advantages of the Indian service are but imperfectly known in the medical schools.

We apprehend also that the Government has not been fortunate in the selection of an examining board; that board, as at present constituted, has not secured the confidence of those most conversant with the nature of the East India Company's service. We have nothing to say against the talents of the gentlemen appointed, and we are very far from adopting the objection sometimes urged against teachers, or hospital physicians and surgeons, being placed on examining boards, namely, the allegation of partiality to the pupils of particular individuals, or particular schools. This argument is, in our opinion, altogether unfounded. No man, we believe, ever objected to the great surgeons of Guy's, or St Bartholomew's, such men as Sir Astley Cooper or Mr Lawrence, being examiners at Surgeons' Hall; but, on the contrary, every one possessing the diploma of that body considers its value greatly enhanced by having such names attached to it.

For such a purpose, however, as the Examining Board of the East India Company has been appointed,—for the purpose of sifting the qualifications for a *special* service, of gentlemen who have already passed the colleges,—we conceive that not only teachers of reputation, but some men who had themselves acquired experience in the camps, cantonments, barracks, and hospitals of India, should have been selected. The present appointments, we find, are looked upon as “a heavy blow and great discouragement” to the many excellent men who, from the comparatively early period at which they are enabled to retire from the Company's service, have returned to this country full of health, vigour, energy, and intelligence. Many such men might have been found altogether unconnected with what Dr Bird calls the “scholastic medical establishments” of the metropolis; men who could have had no object but the honour and interest of that service to which they had devoted themselves; men who would have been in the position of Cæsar's wife, not only pure, but unsuspected.

There is one question connected with this subject which we think of grave importance to the medical schools of Edinburgh and of Dublin. If the examinations are conducted exclusively in London, our Scotch and Irish students will be placed at a disadvantage. The Scottish student, with that constitutional caution for which his countrymen get credit, will be slow to undertake the trouble and expense of a journey to London with the chance of being sent “empty away.” The Irish student again, we have sometimes found prone to

entertain fears (we hope altogether unfounded), that he was placed at a disadvantage in competing with his fellow-students in the Scotch and English schools. These considerations may possibly keep back many eligible candidates, and the best means of placing all upon a par would be that the Examining Board should be of a migratory character, sitting occasionally in Edinburgh and in Dublin; or that separate boards should be appointed for these localities. This hint we throw out as the first that occurs to us on the spur of the moment; but the subject is one which appears to deserve the mature consideration of all those interested in the prosperity of the Scotch and Irish medical schools, and to which the members for the cities of Edinburgh and Dublin should be solicited to give their attention.

It is to be observed that the surgeons in the East India Service, although occasionally employed in civil appointments, are all, in the first instance, at the disposal of the military authorities for the duties of the army and navy, and we do not think that it will redound to the credit of the Government, or the interests of the public service, if the pupils of the Edinburgh and Dublin schools—the only two in which military medicine and surgery have been heretofore regularly taught—should be placed at *anything like* a disadvantage in a competition from which the best results are expected.

THE LATE DR ALEXANDER STRUTHERS.

DR ALEXANDER STRUTHERS was one of three young surgeons specially selected from the Edinburgh School to proceed to the seat of war, and lend their assistance to the medical staff in the East. With his companions, Drs Greig and Johnstone, he left Edinburgh at a few days' notice, and after passing the usual examination at the Army Medical Board, left London, October 23d. Proceeding *via* Marseilles, he arrived at Scutari, November 4th, and speedily entered on duty as acting assistant-surgeon in the barrack hospital there. A large number of the wounded after the battle of Inkermann were placed under his care, and also many of the sick, and we have reason to know that his services were highly appreciated. He continued on active duty for nearly two months, and was in good health up to Christmas day, when he was attacked by the fever of a low type, common in the hospital, of which he died January 20th, aged 25. During his illness he was carefully attended and supplied with the comforts of a sick-room by Miss Nightingale, and everything which medical skill could suggest was done by his two attached friends, Drs Wason and Greig. Others of his old college friends were also near, and from one of these we since learn that they are erecting a monument to his memory, where he lies buried on a height by the Sea of Marmora.

Dr Struthers possessed in a remarkable degree two qualities, which, when observed in a student, generally lead to subsequent distinction, namely, earnestness and enthusiasm for his profession. These led him to extend his education beyond the usual prescribed routine; and especially after he graduated in 1850, to continue his studies first as a demonstrator to his brother, then as an ordinary, and, subsequently, as a resident clerk in the Infirmary. With his superior medical officers he was a great favourite, on account of the evident interest he took in the cases under his charge, an interest which often led him to maintain stoutly, but respectfully, any views of his own that he was led to adopt regarding them. His avidity for discussion will long be remembered in the clerk's room of the Infirmary, as well as in the Medical Society, of which he was president during the session 1852-3. Indeed, we know of no young man of his standing, whose eagerness for knowledge, unwearied assiduity, and general good qualities, were more conspicuous, and for whose loss in consequence, more sincere regret could be experienced by all to whom he was known.

At the moment of going to press, we are grieved to learn that Dr Wason has also fallen a victim to fever.

VARIETIES.

THE ABSURDITY OF FREE-TRADE PRINCIPLES IN MEDICINE.—The principles of free-trade, of non-protection by the State, are applicable only where there is an indefinite, or at least a highly extensive, demand. Now, in what are usually called the professions, in contradistinction to the trades, there cannot exist an indefinite demand, or even any greater extension of demand than is proportionate to the increase of the population. Free-trade in the profession of the law is not called for, because it never can be the policy of a nation to increase the demand for law, but quite the reverse. For the same reason the recent notions of some, that the profession of medicine and surgery may be safely reduced to the ranks of free-trade, are crude and dangerous conceptions. If we suppose a country to be supplied, under a careful system of education and licensing, with an adequate number of skilful physicians and surgeons, the great majority of whom are able to gain no more than that competency which is necessary to maintain for them their proper station in society, and to provide for those dependent upon them, it would be the height of absurdity for the State to open the door for inundating these professions with ill-educated and unlicensed, or ill-licensed, practitioners, because there is not, and ought not to be, any appreciable extensibility of demand for them. Diseases and injuries do not materially increase; or, if they do, the increase must be occasioned by the ignorance and unskilfulness of the superfluous members of the profession. But, in sober fact, the only result would be the undue sub-division of the necessarily limited gains of the profession, and successively the impoverishment of the pre-existing members, the repulsion of men of education from the practice of the medical art, and the gradual degradation of it from a scientific profession to a mere trade.—*Professor Christison's address to the North British Pharmaceutical Society.*—*Pharmaceutical Journal.*

THE ADVANTAGE OF OPENING UP OFFICIAL APPOINTMENTS TO MERIT.—Second in the list of successful competitors at the recent examination of the East India Company, for medical appointments, was Dr Chuckerbutty, by birth a Brahmin of the highest caste, and a native of Dacca in Bengal. His history presents a memorable instance of what may be achieved by industry and energy, and leads us to rejoice that we have introduced a system under which merit can conquer all the difficulties of race, of poverty, and of friendlessness. He was born in 1827, and at six years old was left an orphan by very poor parents; notwithstanding which, he contrived to become master of Sanscrit, Persian, and the Bengalee vernacular. He was thirteen before he heard English, but with a few clothes in a bundle, and a little parched rice for food, he set off on a journey of sixty miles to the nearest English school, to learn that language. Without money, friends, or introductions, he concluded a bargain with the schoolmaster to perform the duties of cook, on condition of being taught English. In 1843, Mr Alexander, of the Bengal civil service, offered to pay his expenses as a student in the Medical College of Calcutta, where he was admitted in 1844. In 1845, he visited London, and studied at University College, where he obtained several prizes for proficiency. In 1848, he became a surgeon, and in 1849 a physician. He returned to India in 1850, and in 1854 he became Professor of Materia Medica and Clinical Medicine in the College Hospital. On learning that he could enter the service of the East India Company by competition, he again returned to London, and, as we have seen, was second in the list of competitors.—*Times.*

THE ASSOCIATION JOURNAL AND THE RECENT ELECTION AT THE EDINBURGH COLLEGE OF PHYSICIANS.—There never was a job perpetrated, however gross, which did not find an apologist. In a leading article abounding in mis-statements, such as, that the Edinburgh College is composed of 70 fellows, whereas the exact number on the roll is 48—that its president's chair was the chair of Abercrombie, although it is well known that physician never sat in it, and

so on—this Association Journal defends the votes of the Junto, for the following extraordinary reasons:—1st, That the names of the present council are guarantees to Europe of the desire of the College to do honour to well-earned professional reputation; 2d, That this College presents a noble example of a representative body, departing from routine, and the distinction of professional grades, in order to open up a successful career to talents alone! Would, indeed, that such were the case, for then would the College not only be supported by us, but by the medical world at large. But what will the profession think of a journal disingenuously attributing such exalted reasons for an act, which, in the place of Professor Simpson, introduced Dr Wm. Seller; which rejected Professor Bennett, in order to elect Dr John Burt, and which slighted some dozen highly respectable general practitioners, all seniors to Dr Begbie, in order to put that gentleman in the chair? What must our continental readers and the profession suppose, when informed by the Association Journal, that Drs Seller, Burt, and Begbie are recognised above all others in the Edinburgh College of Physicians as possessing the greatest talents, and the most extensive European reputations?

GRANT OF PUBLIC MONEY TO THE DUBLIN HOSPITALS.—It appears that up to 1849, a sum of L.18,000 had been annually voted by the House of Commons for charitable purposes in Dublin, and that last year a sum of L.12,000 was proposed. A Select Committee of the House has reported that the sum now required is L.16,000, and recommend that it be granted. The committee, in fact, recommends the continuance of this grant upon the condition that it shall be allocated principally with a view to medical and surgical education, and reports that "it has been shown that the hospitals afford to the medical school of Dublin the requisite means of instruction in the several branches of medicine, surgery, and midwifery."—*Dublin Medical Press*. The enormous sums lavished for so many years past on the magnificent hospitals of Dublin, on the ground that its medical school is thereby supported, should cause the Scotch members of the House of Commons to demand a similar grant to support the great medical schools in Scotland. We have yet to learn that Government has given one penny towards the maintenance of the Royal Infirmary of Edinburgh, and we cannot see why the people at large should be taxed to keep up splendid medical institutions in Ireland, whilst those in Scotland have to struggle on with inadequate voluntary contributions.

MEDICAL STAFF OF THE FRENCH ARMY.—The Medical Staff Officers attached to the army are constituted as a corps under the title of *Corps de Santé de l'Armée*, etc. It consists of two classes, viz., Surgeons and Apothecaries. Of the first there are 7 inspectors, 40 *principaux* of the first class, and 40 of the second; 100 majors of the first class, and 220 of the second; 340 aides-majors of the first class, and 340 of the second; making a total of 1087. The Apothecaries are divided into—1 inspector, 5 *principaux* of the first class, and 5 of the second; 15 majors of the first class, and 30 of the second; 45 aides-majors of the first class, and 45 of the second; making a total of 146. The number of both these classes is the same in war as in peace; but, in case of necessity, auxiliaries, whose number is unlimited, are appointed by the Minister of War. Their number varies with circumstances. None can be appointed to this corps who have not passed through the Military School of Medicine. A portion of the Inspectors form a council, whose duty it is to watch over all improvements in Surgery, and to report from time to time to the Minister of War.

ONE OF THE BURDENS WHICH PRESSES ON THE MEDICAL PROFESSION.—The whole mass of the poor in this country is thrown upon the almost unassisted charity of the medical profession; a charity to the support of which the public contributes scarcely a tithe. No burden in any degree resembling it is sustained by any other profession, or by any trade. From the working clergy, indeed, in many places, even a greater measure of gratuitous toil is extracted; but

their case, in several respects, differs greatly from that of the surgeon, who gives time which is of money value to him, drugs which are costly, the services of an assistant whom he must pay; and often is compelled, also, to keep a horse at the disposal of the poor. He is obliged not seldom to turn from the door of the rich man, who would pay him for his visit, to fulfil his duty to a poor man in more urgent need; and for all such labour he receives nominal payment, with few thanks from boards of guardians; some of whom behave to him with autocratic condescension or with inflated incivility, as if surgeons were slaves, and they assemblies of three-tailed bashaws.—*Household Words*.

CHAMPAGNE.—Nowhere is champagne the common beverage of the people, any more than pastry is anywhere their daily bread. Champagne is the confectionary of wine making, and both that and pastry are superfluous luxuries. Neither a garrison in a state of siege, nor a populous island on which provisions run short, would think of brewing champagne or making puff tarts. This year the vintage is comparatively a blank at Epernay, but we may safely predict that, though prices will rise, there will be no perceptible deficiency in the general supply. It is much easier to make good champagne wine beyond the limits of the ancient province, than it would be to manufacture Burgundy wine far away from Burgundy. You can fabricate pinchbeck, but you cannot make gold. Champagne wine is so completely a fictitious thing, that if the duty on French wines were taken off in England, champagne could and would be prepared in London, so good as to threaten a serious rivalry to the genuine article from Châlons-sur-Marne. There is but one Côte d'Or, and human skill cannot create another.—*Household Words*.

FORMATION OF SUGAR IN THE LIVER.—The theory of the formation of sugar in the liver, which was established by such numerous and careful experiments by M. Bernard, was on the 29th of last January attacked by M. Figuier. This gentleman then read a memoir to the Academy of Sciences in Paris, denying the glucogenic function of the liver, and, from several experiments and analyses, attributing the sugar so produced to the blood, as a result of digestion. M. Bernard has, in consequence, repeated his experiments in public at the College of France, and maintains the correctness of his former deductions. We shall report the result of this discussion in a future number.

ASSOCIATION OF THE GRADUATES OF THE UNIVERSITY OF EDINBURGH.—We are happy to announce that the graduates have instituted an Association with a view of obtaining, 1. Some recognised connection of the graduates with the University; 2. The promotion of a higher education in the University; 3. The recognition of Scottish medical graduation in England; 4. The representation of the Scottish Universities in Parliament. We sincerely trust that this Association will be conducted vigorously, but we must caution it against taking into its councils medical graduates whose views are of a kind which must necessarily injure any young institution having noble aims in view regarding education.

TYPHUS AND TYPHOID.—The very vexed question of the identity or non-identity of these two diseases is now before a committee of the French Academy, composed of Andral, Serres, and Rayer. In London, with Jenner, the question seems long ago set at rest; and we are all of late months looking out for what are called "rose-spots," which we sometimes see very learned theorists and advanced students show to one another; these rose-spots very often not being mulberry-spots or rose-spots at all, but a grand climacteric of flea bites!—*Dublin Medical Press*.

ON THE APPLICATION OF FRIGORIFICS IN COLD WEATHER.—Dr Arnott informs us that ice of a lower temperature than 32° does not dissolve with sufficient rapidity; consequently whoever uses it for anæsthesia, must, when it is brought to him of that temperature, do, as the lady, peevishly complaining of the coldness of the ice-cream which he had prescribed, was recommended to

do by Sir Henry Hallford—he must warm it. The courtly physician may have been better acquainted with the chemistry of confections, than those who have laughed at his polite recommendation.—*Lancet*.

CIVIL HOSPITAL FOR THE MILITARY AT SMYRNA.—The new Minister of War is about to establish an hospital at Smyrna for the sick and wounded of the Eastern army, which is to be under the control of civil surgeons, who are to be properly remunerated. This is a good temporary measure, but will not serve in the long run to improve the effective power of the army medical department as now established.

THE ACADEMY OF MEDICINE.—This Academy, it is said, is now overwhelmed with surgical memoirs, in consequence of the vacancy caused by the death of Lallemand. The number of candidates is unusually large, and from the equality of merit, the academicians will have great difficulty in making the election.—*L' Union Médicale*.

PUBLICATIONS RECEIVED.

- Surgical Anatomy.** By J. MacLise, F.R.C.S., London. Folio. Fasciculi iv., v., and vi.
- Medical Anatomy.** By Francis Sibson, M.D., F.R.S., Physician to St Mary's Hospital. Folio. Fasciculus i.
- The London and Provincial Medical Directory.** 1855. London. Small 8vo. Pp. 178.
- The Medical Directory for Scotland.** 1855. London. Small 8vo. Pp. 696.
- Eutheraia; or, an Examination of the Principles of Medical Science, with Researches in the Nervous System.** By Robert Garner, Surgeon to the North Staffordshire Infirmary, etc. London. 8vo. 1855. Pp. 282.
- Lithotomy Simplified, or a New Method of Operating for Stones in the Bladder.** By George Allarton, M.R.C.S., etc. London. Ash and Flint. 1854.
- The Pathology of Drunkenness; a view of the Operation of Ardent Spirits in the Production of Disease: founded on Original Observation and Research.** By Charles Wilson, M.D. Edinburgh. Foolscep. 8vo. 1855. Pp. 230.
- Elementary Treatise on Chemistry.** By William Gregory, M.D., F.R.S.E. Professor of Chemistry, University of Edinburgh. Edinburgh. Foolscep 8vo. 1855. Pp. 348.
- L'Huile de Foie de Morue envisagée sous tous les rapports comme moyen Therapeutique.** Par L. J. de Jongh, Docteur Médecin à la Haye. Paris. 8vo. 1853. Pp. 262.
- The Diagnosis of Surgical Cancer.** (The Liston Prize Essay for 1854.) By John Zachariah Lawrence, Surgeon to the Northern Farringdon Dispensaries, etc. London. 8vo. 1855. Pp. 77.
- Notes on some of the Developmental and Functional Relations of Certain Portions of the Cranium.** Selected by Frederick William Pavey, M.D., London, from the Lectures on Anatomy delivered at Guy's Hospital. By John Hilton, F.R.S. London. 8vo. 1855. Pp. 93.
- The Pathology of the Broncho-pulmonary Mucous Membrane.** By C. Black, M.D. Bachelor of Medicine, London and Edinburgh. 8vo. 1855. Part ii.
- The Journal of the Royal Agricultural Society of England.** Vol. 15. Part ii. No. xxxiv. London. 8vo. 1855.
- Progressive System of Medical Education of the Penn Medical University of Philadelphia, and announcement of the Fall and Winter Session of 1854-55.** Philadelphia. 8vo. 1854. Pp. 16.
- Eighth Report of St Mark's Ophthalmic Hospital and Dispensary for Diseases of the Eye and Ear, Lincoln Place.** 1853-54. Dublin. 8vo. 1854. Pp. 11.
- Report, dated 13th January 1855, on the Examination of Candidates for the Appointment of Assistant-Surgeon in the Service of the East India Company.** London. Foolscep folio. Pp. 10.
- The Micrographic Dictionary.** Part vii. Asylum Med. Journal.

We are compelled to postpone our List of Exchanges until next month.

The papers of Messrs Kerr and Brown have been received.

We regret to inform *Médecus* of Clifton that we cannot find his letter, but shall be happy to answer his queries if he will write again. In the meantime, we may say, that the salt to be used is the carbonate of soda, the old subcarbonate, and that the application must be constant and incessant, but renewed morning and evening.

ERRATA.—Two errors in our Review of Jones and Sieveking's Manual, completely alter the sense: at p. 144, 16 lines from top, "ever" ought to be never; and 17 lines from bottom, "spinal" ought to be special.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Cirrhosis of the Lung.* By THOMAS B. PEACOCK, M.D., Assistant-Physician to St Thomas's Hospital, and Physician of the City of London Hospital for Diseases of the Chest.

FRANCIS MACQUEEN, æt. 17, first came under my notice in Nov. 1849, when he was an in-patient at St Thomas's Hospital, under the care of Dr Bennett. He then stated that he had been ailing for three years; his illness commenced with cough, followed by expectoration. His general health then became impaired, and he had continued ill ever since.

When seen on the 3d of December he complained of a severe cough, and expectorated a peculiar, brownish-coloured purulent fluid, of a very fetid odour. This expectoration was extremely profuse, and was brought up in large quantities when he reposed upon the back, and especially on the left side. He ordinarily occupied the right side. He complained of headache but was free from pain in the chest. The tongue was clean, the pulse 100 and feeble, the bowels regular, the appetite and digestion moderate. His face was somewhat livid; he was much emaciated, and his fingers were bulbous and the nails incurvated.

On examination the right side of the chest was in front flatter than the left, and especially between the outer end of the clavicle and the nipple, but in the right dorsal regions it was fuller than in the left. The cardiac pulsation was visible on the right side below the nipple. On percussion the resonance was impaired in the sub-clavicular region, and entirely dull below the nipple. On auscultation vesicular respiration was audible only immediately below the sternal half of the clavicle, while below the outer extremity of that bone there was loud gurgling respiration and cavernous sounds with the voice and cough, and occasionally with a metallic resonance. Elsewhere respiration was attended with loud mucous rhonchus. The left side of the chest sounded throughout clear on percussion, and the respiration was there loud and puerile. He was then taking three teaspoonfuls of the cod liver oil three times daily, and at night five grains of the compound ipecacuan powder, and was allowed a generous diet.

On the 21st of August 1850, the following notes were taken:—There is now considerable improvement in his general appearance, and he has gained both flesh and strength. He has not much appetite, the tongue is somewhat furred, the pulse accelerated, the bowels regular. The falling in below the acromial end of the right clavicle is very obvious, and in this situation there is an obscure tympanitic resonance, and the pot félé sound is occasionally heard on percussion, and there are loud cavernous respiratory, vocal and cough sounds,

but little evidence of fluid. On the left side of the chest there is clear resonance on percussion, and loud compensating respiration is heard.

Dec. 4.—He has continued to improve since the last notes were taken. With the exception of the flattening beneath the acromial end of the right clavicle, the inequality between the two sides of the chest is not very marked. The right side is still somewhat dull on percussion, the left quite clear; in the flattened space there is an imperfect tympanitic resonance on percussion, and occasionally the cracked pot sound is elicited, with cavernous respiration but without rhonchus, except on forced inspiration. The same signs are heard at the supra-scapular region. He still expectorates the fetid brownish coloured fluid but in lesser quantity, it is brought up in gulps, and, except at that time, he has little cough. His appetite is tolerably good and he is gaining strength.

At the commencement of the year 1851 he was discharged from the hospital, and became an out-patient of my own, and continued to attend till April, when, after exposure to cold, he was seized with great difficulty of breathing, increased cough, and expectorated much blood. He was readmitted into the hospital on the 5th, and died asphyxiated on the 11th, or about four years and a half from the commencement of his illness.

The following notes of the examination of the body were made by Dr Bristowe.

Body short and small, and altogether resembling that of a much younger person than he is known to be. It is spare but not very much emaciated; the fingers are remarkably clubbed.

On removing the sternum and costal cartilages no part of the right lung was observable, the left lung extending much further to the right than is usual, and the heart being displaced to the right side. The layers of the left pleura were adherent by scattered firm cellular bands, and the left lung was of large size, crepitant and healthy throughout, except at the apex, where there was some puckering and a small speck, not larger than a pin's head, of calcareous matter imbedded in its substance. The right lung was very much diminished in size, and its pleural surface was almost entirely adherent to the parietes by old and thick cellular attachments, the diminution in the size of the lung being compensated by the contraction of the right side of the thorax, by the ascent of the diaphragm and liver, and by the altered position of the heart. At the base of the lung the adhesions were somewhat softer than elsewhere, and had a vascular appearance, as if from some recent inflammation. On section the lung was found to be consolidated every where, except at the upper and anterior edge, where there was a portion of its substance partially permeable to air. The condensed portions were of a pale fawn colour, mottled with black pigmentary matter, and very firm. The primary branches of the right bronchus were increased in size, and the bronchi arising from them were more than proportionately dilated, being between a third and a quarter of an inch in diameter. The bronchi opened abruptly into cavities of various sizes, and these were separated from the surface of the organ by a layer of condensed tissue, from a quarter to half an inch in thickness. Some of the cavities, especially towards the apex of the lung, were of a regular oval figure and presented a smooth surface, which appeared to be continuous with the mucous membrane of the bronchi, so that they appeared to have been formed by the dilatation of the tubes, but in others the lining membrane had not the appearance of mucous membrane, so that the cavities seemed to have resulted from ulceration at a former period, and this was further rendered probable by the cavities, in some cases, communicating with two or more bronchial tubes. In the lower lobe of the lung there was a large cavity, which contained a considerable quantity of coagulum, and this cavity was unprovided by any lining membrane, and appeared to have resulted from recent ulceration or softening. The mucous membrane of the bronchial tube leading

towards this cavity was very vascular and had a villous appearance, and in places there were ulcerated excavations, giving it the appearance of a network of coarse irregular meshes.

The tubes in the upper part of the lung were nearly free from secretion, but in the middle and lower parts they were filled with muco-purulent matter, deeply tinged with blood. The cartilages of the tubes were much increased in size and thickness, and the longitudinal fibres were large, distinct, and separated into bundles having numerous depressions between them.

The pericardium and heart were healthy. The abdominal organs were also healthy, except that a large lumbricus was contained in the colon. The brain was not examined.

The case above related possesses much interest. The form of disease of which it affords an example, is one which is not of common occurrence. Though alluded to by Laennec as a variety of dilatation of the bronchial tubes, and subsequently by Dr Williams,¹ the affection was first fully described by Dr Corrigan,² and has consequently been generally known by the name of "cirrhosis of the lung," a term which, from a supposed analogy in its mode of formation to cirrhosis of the liver, he applied to it.

Laennec regarded dilatation of the bronchial tubes as always the sequence of chronic mucous catarrh, and as caused by the accumulation of secretion in the inflamed tubes, by which they became expanded, and by the pressure which they exercised on the adjacent pulmonary substance, caused the collapse of the cells, in the same way as collapse of the lungs is produced by the pressure of pleuritic effusions. Dr Williams supposed that the ordinary form of bronchial dilatation was caused by the efforts of inspiration and cough, acting upon tissues rendered more readily expansible from the effects of inflammation; but that the sacculated expansions with consolidation of the interjacent pulmonary substance, had an entirely different mode of formation. He regarded this form of disease as the result of attacks of pleurisy, in which, from long-continued compression, "the air tubes and cells become obliterated by the adhesion of their sides, so that when the liquid is removed from the pleura, they will not expand again with the enlargement of the chest, but the large and middle-sized bronchi are not obliterated; they bear the whole force of the inspired air, and become completely dilated by it." Shortly after Dr Williams advanced these views, Dr Corrigan, in a valuable paper, gave a much fuller description of the disease than any previous writer, and suggested an entirely new view of its pathology. He supposes the contraction of the lung, the obliteration of the air cells, and the dilatation of the tubes, to be the result of a morbid condition of the cellular tissue, which is the matrix of the vessels and air tubes, and of the general fibro-cellular envelope of the lung, assisted by the elastic contraction of the longitudinal fibres of the air tubes. "In proportion

¹ Lectures in Medical Gazette, 1838, and Rational Exposition of Diseases of the Chest.

² Dublin Journal, vol. xiii. 1838. P. 272.

as the contraction of the fibres of the fibro-cellular tissue obliterates the small air vesicles, and as these contracting fibres, like so many elastic strings, extending from the root in all directions, tend to contract or draw in the tissue of the lung, obliterating its small air tubes and its blood-vessels, the larger bronchial tubes dilate to supply the place thus left, until, when the disease has reached its last stage, the tissue of the lung diminished to a very small size, presents no longer any permeable air vesicles, but a dense fibro-cellular or fibro-cartilaginous tissue, with its fibres radiating in every direction, through second and third sized bronchial tubes, dilated into cells, or ending in *culs de sac*, of every variety of size." "The dilatation of the bronchial tubes is partly owing to the contractile process going on in the tissue of the lung, partly to the expansive action of the parietes of the chest in the acts of inspiration." "If there were but one bronchial tube, with contracting fibro-cellular tissue placed around it, then the contracting tissue, would, as in the instance of stricture of the œsophagus or rectum, cause narrowing of the tube; but when there is, as in the lung, a number of bronchial tubes, and the contracting tissue not placed around the tubes, but occupying the intervals between the tubes, then the slow contraction of this tissue will tend to draw the parietes of one tube towards the parietes of another, and necessarily will dilate them."

Rokitansky¹ adopts a view which was advanced by Dr Stokes, that the common form of dilatation of the bronchial tubes is due to the "atony and paralysis of the contractile and irritable elements of the tubes dependant on chronic inflammation and blennorrhœa, aggravated by the obstruction of the other tubes by secretion." The other description of disease he states to be the result of bronchitis in the ramifications of the bronchi, beyond those which become dilated; and he regards the dilatation as dependant on "their obstruction by the accumulation of secretion, on the tumid state of their mucous membrane, and finally on their actual obliteration." "It is produced by the hindrance which is presented to the free ingress of the inspired air, and is proportional to the difficulty of breathing, and the prolonged length of each individual inspiration; and is especially developed in and about the perfectly impermeable bronchial tubes. The parenchyma surrounding this portion of the bronchial system collapses, and this produces a space which becomes filled by the dilating bronchus. The dilatation thus lies entirely, or for the most part, in a collapsed, and apparently compressed, portion of the parenchyma; hence, the latter appears to be the *primary* anomaly, and the bronchial dilatation merely a *resulting* and consecutive morbid change." It will be seen from the above statements, which have been purposely quoted in the words of the writers, that Dr Williams, Dr Corrigan, and Rokitansky, agree in regarding the primary change which takes place in this disease as consisting in

¹ Sydenham Society's Translation.

the collapse of the air cells, and the dilatation of the tubes as, in a greater or less degree, resulting from the expansion of the chest during the acts of respiration; but they differ as to the cause to which they assign the production of the collapse, Dr Williams regarding it as due to pressure exercised on the periphery of the lung, Dr Corrigan to a morbid change in the pulmonary substance itself, and Rokitanaky to obstruction to the entrance of air into the terminal portions of the tubes. Recently, Dr Gairdner,¹ in his very able papers on the morbid anatomy of bronchitis, has discussed the mode of formation of the different forms of bronchial dilatation, and expresses opinions opposed, in some respects, to those of all the writers which have been mentioned. He remarks:—"The conclusion to which I have been led by this survey is, that almost all the so-called bronchial dilatations, and all those presenting the abrupt sacculated character here referred to, are in fact the result of ulcerative excavations of the lung communicating with the bronchi;" and, after explaining his reasons for adopting this opinion, he adds, that "the usual origin of bronchial dilatations is in cavities formed in atrophied lung, in consequence of bronchitis or tubercle, and afterwards expanded beyond their original dimensions by the inspiratory force."

The view advanced by Laennec as to the mode of production of this form of disease cannot be received. It is difficult to conceive that, apart from any other change in the lung, the accumulation of secretion in a tube, which maintains its communication with the main bronchus, could take place to such an extent as to give rise to the large cavities which are frequently seen; and still less is it probable that such dilated tube, by compressing the adjacent pulmonary substance, should cause the marked solidification which often occurs in these cases.

Dr Corrigan's views also, though very ingenious, do not afford a satisfactory explanation of the mode of production of the dilated tubes and consolidated pulmonary tissue. I have seen the most marked dilatation of the tubes without any great degree of adjacent consolidation; when, indeed, the whole of a portion of lung was converted into large dilatations communicating with the bronchi, and only separated from each other by thin septa. In other cases, where there are large tubular cavities and consolidated tissue in the same lung, the two conditions do not correspond in situation, so that the one could not be regarded as the cause of the other. A very remarkable example of the former description of disease was exhibited at the Pathological Society during this session by Dr Hare. In reference to the views of Dr Williams and Dr Gairdner, I believe both to be correct in some cases, and that by one or other of the modes mentioned by these writers all the various forms of so-called dilatation of the bronchial tubes which are observed may be explained. Of

¹ Lond. and Edin. Monthly Journal, vol. xiii. 1851. Pp. 248, 249.

the frequent dependence of some of the cases of the so-called "cirrhosis of the lung," on compression of the pulmonary tissue by old pleuritic effusions, there can be no doubt. In most instances the lung is found firmly adherent to the parietes by old cellular attachments, and often there is a thick layer of fibro-cartilaginous false membrane on the surface of the pulmonary pleura. Recently I had the opportunity of examining a case in which an effusion in the left pleura had existed during six years, and had formed communications both with the bronchi and externally. In this case a portion of the lung was adherent to the parietes by fibro-cartilaginous attachments; the lung was greatly compressed, and the bronchial tubes much dilated,—the dilatation being more marked towards the periphery of the lung, where, indeed, some of the tubes were nearly the size of the little finger, and these cavities were evidently lined by natural epithelium. It is, however, unnecessary to urge the operation of this cause in producing dilatation of the tubes, as it is evident that if the lung be permanently collapsed from compression, the space not occupied by the contraction of the parietes must necessarily be compensated by the expansion of the tubes. On the other hand, Dr Gairdner is, doubtless, correct in the explanation which he gives of the mode of formation of the sacculated expansions, which open more or less *abruptly* from the tubes. Such cavities are not found to be lined by true epithelium, but by a dense fibrous layer, which has every appearance of being a new formation. In the specimen which I have referred to, as exhibited at the Pathological Society by Dr Hare, the lining of the large dilatations had a smooth glistening surface, and was in places crossed by fibrous bands; and, both to the naked eye and under the microscope, presented no appearance of true epithelium. The cavities in the lung in the case which is the subject of this communication affords decided evidences of having been produced by the mode referred to by Dr Gairdner. Their irregular form, the absence of any distinct lining membrane in some of them, and the existence of two or more communications with the bronchial tubes in others, sufficiently show that though some of the cavities were lined by a membrane apparently continuous with the mucous membrane of the bronchi, and though the bronchi generally were undoubtedly much dilated, the sacculated cavities had resulted from ulceration of the pulmonary substance, afterwards expanded during the act of inspiration, and not from simple dilatation of the tubes. It is not, however, equally clear what was the nature of the disease by which the ulceration of the pulmonary substance had been occasioned, and there was nothing in the history of the case, or in the appearance of the lung, to show whether the cavities had resulted from bronchitis with bronchial abscesses, or from consolidation and subsequent ulceration or softening of the tissue of the lung.

The second point of interest which the case presents is the difficulty which existed in deciding during life the precise nature of

the disease of the lung. In its general features the case bore a close resemblance both to ordinary tubercular phthisis and to contraction of the lung succeeding to an empyema which had been evacuated through the bronchi. Indeed, after careful examination and consideration, I was led to conclude that the disease was of the latter description.

To phthisis it was closely assimilated by the frequent occurrence of more or less hæmoptysis, by the cough and expectoration, by the emaciation, and the physical signs which evidenced the existence of cavities in the lung. On the other hand, the duration of the disease was much greater than in ordinary phthisis, the emaciation and the hectic symptoms were less marked than in a case of common consumption at so advanced a stage; and especially the absence of any evidence of disease of the left lung was irreconcilable with the idea of true tubercular disease. I was therefore, as before stated, led to suppose that the case was one of disease of the lung ensuing upon an empyema which had been evacuated through the bronchi; and the great contraction of the affected side of the chest, the mode in which the matter was expectorated by a kind of half-vomiting act, the profuseness of the expectoration, and its peculiar character, confirmed me in this opinion.

The pathologists of the Dublin school, to whom we are indebted for the illustration of this form of disease, have devoted much attention to its diagnosis. Dr Corrigan dwells much upon the absence of constitutional disturbance correspondent to the amount of pulmonary disease, the less marked hectic symptoms, and the signs of contraction of the chest, as distinguishing the cirrhosis of the lung from phthisis; and Dr MacDowell¹ thinks that the slow progress of the case, the limitation of the affection to one lung, the greater extent of disease at the lower than at the upper parts of the chest; the predominance of the signs of consolidation over those of softening, the displacement of the heart, and the absence of hectic and of all signs of laryngeal affection, will sufficiently distinguish the disease from phthisis. Dr Stokes, in describing a case of the kind, attached much importance to the absence of hæmoptysis; but this, so far from being a frequent distinction between cirrhosis and phthisis, is really a feature in which the two affections closely correspond, for hæmoptysis appears to be a very constant symptom in cases of cirrhosis. The signs pointed out by Dr MacDowell will, I think, generally enable us to effect a diagnosis between this affection and phthisis; but to distinguish some cases of cirrhosis from the second form of disease to which I have alluded, would appear to me to be extremely difficult. Indeed, after careful consideration of this case, and after having had the opportunity of observing several instances of chronic fistulous communication between the pleural cavity and the bronchi, I am incapable of pointing out any decided signs between that

¹ Dublin Journal, 1852.

affection and the form of disease of which the case here related affords an example; and I am much inclined to doubt whether a decided differential diagnosis could be effected.

ARTICLE II.—*An Experimental Inquiry into the Existence of a Sixth Sense, here called the Sense of Force.* By RICHARD F. BATTYE, Esq., London.—(Concluded from p. 223.)

THE last articulation to which I shall refer, is the radio-carpal articulation, or the wrist-joint. This joint, like the elbow-joint, has no intervening articular fibro-cartilage, saving the triangular disc, between the cuneiform bone and the ulna; hence there is nothing to materially hinder the propagation of force in the same line of direction to that of the joint towards which it is tending. The carpal bones are small, well bound together by ligaments, and contain considerable hard compact tissue. The metacarpal bones also, with the phalanges, are well adapted to transmit force from their apices to the carpus, and thence to the synovial membrane of the joint. The synovial membrane is always well lubricated with synovia, is extensive, very complete in its continuity, and enjoys a high degree of sensibility to the slightest mechanical injury, or from the mildest form of inflammation that can affect that membrane. In every thing it is a joint well adapted to determine the correctness or validity of the present hypothesis. Again, between the ulna and cuneiform bone, there is a cartilaginous disc, covered with the same synovial membrane as the radial surface on one side, whilst the same disc is partially covered, and also the rounded head of the ulna, with the membrana sacciforma, on the opposite side; but this last named synovial membrane is very imperfect in many, and, in its location between the ulna and the fibrous disc, it is rarely found lubricated with synovia, or appears to fulfil the function of facilitating motion, but rather is lodged in this part as a point of attachment to retain it *in situ*, whilst the remaining part of the sac fulfils the function of aiding motion during pronation and supination between the radius and ulna, and is usually found slightly lubricated with synovia. These remarks, upon the membrana sacciforma, are made with the view of marking the contrast between the synovial sacs of the articulation of the inferior maxillary and the ulna, both of which have an inter-articular fibro-cartilage, and a double synovial membrane covering them; but the slightest reflection will immediately suggest the difference between the two, in extent, function, and sensibility.

This digression is not made without weighty reasons, since the writer is of opinion that the membrana sacciforma, in its relation to the head of the ulna, does not assist in the least in supplying consciousness of weight to this part of the wrist joint; and, moreover, that the triangular fibrous disc acts (as a damper in a piano-forte)

in suppressing any consciousness of weight, arising from impressions made upon the principal synovial membrane of the joint, when this membrane, by inverting the hand, is placed in close contact with the cuneiform bone and the head of the ulna, whereby, when any weight is thrown upon this part of the articulation, consciousness of weight ceases to be experienced.

Without, then, making any further digressions, by soliloquizing upon the beauties and excellencies of that most perfect piece of mechanism, which either nature or art have completed—the hand—I will at once enter upon the detail of a few experiments.

Supposing I place in my hand a thick heavy book, say Ainsworth's Dictionary, I instinctively place the hand and forearm in one line of direction, and gently raise and depress the extremity, whereby a tolerable estimate of its weight is obtained. Again, if I still retain the book (elevated on the fingers and thumb), and *invert* the hand, as far as I conveniently can, taking care not to abduct the elbow, but retaining it at the same relative distance from the side of the body, I shall find the amount of weight at first experienced to be considerably diminished. To perform this experiment without raising and depressing the hand and forearm is the best.

But, in place of holding upon the hand a considerable weight, suppose a halfpenny or a shilling be placed upon the fingers, I shall instinctively *invert* the hand; but supposing that volition overcomes instinct, and the hand is extended in a *straight* line with the forearm, as in the first experiment, all feeling of weight will disappear, and especially if the hand is, of the two, rather everted.

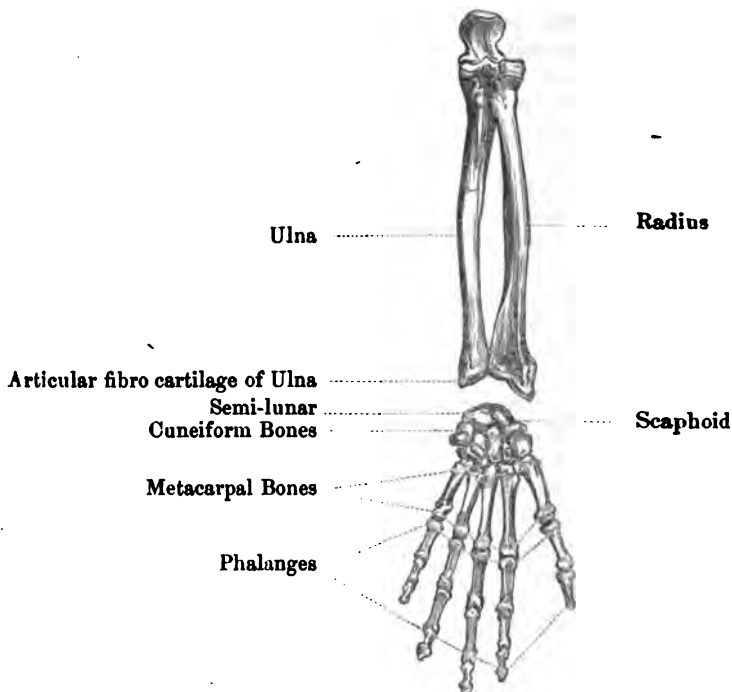
Now, in place of slightly everting the hand, let the hand be inverted as much as can be accomplished with convenience, and the shilling be placed with the fingers slightly flexed, between the third and little finger, and the hand be gently raised and depressed, or rather gently raised and quickly depressed, when more weight will be experienced than in any other position in which the weight can be placed in the hand.

Here, then, are four experiments: with the first, by having the hand extended in a straight line with the forearm a large weight is well and easily discerned; but in the third experiment, with the hand still retained in the same position, a very small weight scarcely can, if at all, be discerned. Again, by inverting the hand, the larger weight, as in the second experiment, is less perfectly estimated and felt; whilst, as in the fourth experiment, by retaining the hand in that position, the smaller weight is well discerned, and the more it is inverted, without inconvenience to the experimenter, the greater is the feeling of weight experienced.

How, then, are these contrasts to be explained? With the larger weight, as in the first and second experiments, there is little difficulty in readily comprehending the rationale; thus, when the hand is extended in a straight line with the forearm, the scaphoid and

semi-lunar bones, which articulate with the radius, are placed in exact opposition to each other, and, consequently, there is a large free surface of synovial membrane, well adapted to receive force, in any direction in which it may be tendered from the hand, impressed with the resultant force of two antagonizing forces, whose course, in their common diagonal, is the natural stimulus, in the nerves there distributed, to excite in them the sense or feeling of weight; therefore, according to all fair modes of reasoning, when the hand is thus placed in relation to the forearm, weight ought to be well estimated.

FIG. 5.



But for the same reason that, in the first instance, weight ought to be easily and well estimated, in the latter it ought not so to be; since, by inverting the hand, the scaphoid and semi-lunar are not so closely approximated to the radius as is the cuneiform with the ulna, and, consequently, the chief synovial surface is very imperfectly impressed with its appropriate stimulus, and therefore the amount of weight experienced is less marked and evident (*vide* fig. 5).

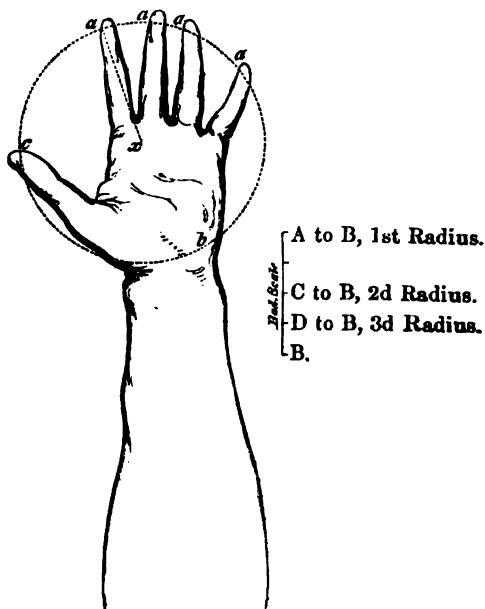
The third and fourth experiments cannot be so easily explained, but, I think, quite as satisfactorily; but to arrive at a correct explanation more analysis is required. All facts are not equally evi-

dent; some are known as much by the absence of one class of facts or phenomena, as by their own actual presence: thus, the shining of the stars by day is as certain as that of the sun's; but the very existence of stars, were it not for the absence of the sun, would never have been detected; and by day they can only be seen by such instruments as exclude, in part, the sun's rays. Again, in chemical analysis the effects of chemical re-agents, where the agent to be acted upon is in very minute quantities, are so slight that their action might be altogether questioned, were it not for the fact that the re-agent, with the agent, can be contrasted with pure water to which the re-agent only has been added, when the difference is evident; but without such contrast, so slight is the change produced upon any given agent, when in very minute quantities, by the re-agent, that its presence or absence could not be satisfactorily determined. Such, then, is exactly the case in the two last experiments recorded. How a very minute weight is tested best by the hand being inverted, arises from *all weight being entirely absent* when the hand is gently raised, and pressed against the inter-articular cartilage of the head of the ulna, by inversion; but by rather quickly *depressing* the hand, the cartilage is gently compressed, and the hand in falling downwards naturally turns a little outwards, and so the articular surfaces of the scaphoid and semi-lunar approximate the radius, and thus induce a feeling of weight, by impressing a larger synovial surface gently and feebly, but certainly; whilst, when this pressure is taken off, by elevating the hand again, *during the act of elevation*, the feeling of weight is *suspended*. It is a curious fact, also, and worthy of great attention, that if the hand alone, or with a slight weight, be long gently elevated and depressed, whilst *inverted*, to assist the *eversion*, when the hand is depressed, and so enable the free articular surface of the radius to be gently counterpoised by the opposing carpal bones, that *the whole of the thumb becomes congested and loaded with blood*, whereby the external part of the hand is made heavier than the internal, and in the act of depressing the hand greatly assists in its very slight eversion, as a natural consequence of gravity acting from that point. Therefore the feeling of slight weights, when the hand is inverted, is not owing to the hand being better able to feel weight in the inverted position than in the straight position; but from the fact that very slight weights are scarcely discernible to the sense of force, and the only way of proving the presence of weight is to contrast the feeling experienced by its *entire absence*, as compared with its most limited, or minute, presence. Such an explanation, with such a contrast, whilst it is quite in accordance with correct anatomy and physics, cannot, I think, be legitimately explained upon a more definite basis than that herein assumed.

I shall now give my last written experiment, but my first practical, which led to the investigation of which this paper may be termed the chief summary.

Let the forearm be placed at nearly right angles to the arm (or humeral division), with the hand extended, and the fingers and thumb semi-flexed, the *first* phalanges being but very slightly flexed. In this position let a book about the size and thickness of "*Whewell's Bridgewater Treatise*," be placed upon the tips of the fingers and thumb, taking care to let each member distinctly touch the book. This being done, after having gently raised and depressed the book, for the purpose of weighing it, carefully remove it, taking care at the same time to retain in their exact sites the thumb and fingers. Whilst thus retained place a pair of compasses, with the point of one shaft over the transverse ligament, binding the heads of the index and middle fingers, and extend to the tip of the index finger the point of the opposing shaft. With the compasses thus adjusted describe a circle, whose centre, *x*, shall be over the transverse ligament before referred to, and whose circumference shall pass over, in order, the tips of all the fingers, the *base* of the metacarpal bone of the little finger, and the pad of the thumb (for no weight naturally rests upon its tip).

FIG. 6.

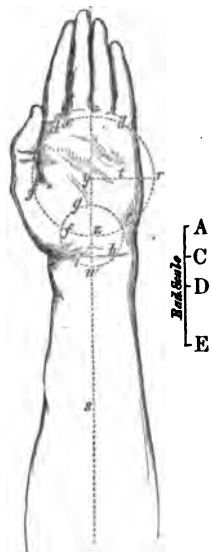


But to better apprehend this description, instead of continuing to burden the memory, where abstraction is chiefly required, I will endeavour to explain it by diagrams, with the aid of geometrical precision, rather than anatomical detail, as far as the subject permits.

Let a book be balanced on the tips of the fingers and pad of the thumb, and when removed let the various parts be retained *in situ*, as in fig. 6. Thus adjusted, describe the circle a, a, a, a, b, c , which shall respectively pass over the tips of the fingers a, a, a, a , the base of the metacarpal bone of the little finger, b , and the pad of the thumb, c , and whose centre, x , shall be immediately over the transverse ligament between the heads of the first and second metacarpal bones.

FIG. 7.

Let any radii of the circle a, a, a, a, b, c , be bisected at equi-distance from either terminal point, as the radius x, a , at the point p , then the bisected radius, from either terminal point, as at the points x, p , shall be the extent of any radii of a second circle, d, d, e, f, f , passing over (when the fingers and thumb are brought together, with the hand extended), in order the metacarpo-phalangeal articulations of the index and little fingers, d, d , the base of the metacarpal bone of the little finger, e , and, less perfectly, over the base and head of the metacarpal bone of the thumb, f, f , and whose centre shall be y , situated near the centre of the hand, between the metacarpal bones of the second and third fingers.¹ (*Vide* fig. 7.)



Let a straight line, s, s , be drawn between the second and third fingers, over the palm of the hand and the wrist, to three or four inches, or less, midway between the radius and ulna, over the interosseous ligament. This line shall intersect the circle, d, d, e, f, f , whilst crossing the wrist at the point z .

The point z shall be the centre of a third circle, g, h, i , the extent of whose radii shall be equal to any radii of the circle d, d, e, f, f , bisected at equi-distance from either terminal point; as the radius y, r , bisected at the point t .

Where the circle, g, h, i , intersects the straight line, ss , at the part farthest from the fingers shall be the point desired, w , being immediately over that part where gravity, from a book, etc., will pass through the wrist-joint.

After having given these successive proportions, it may be said, Why not feel for the joint at once, and so settle the matter? In reply, I may state that, as yet, we have not got quite through the experiment; when it is completed we shall be better able to measure the importance of the point, w , so obtained.

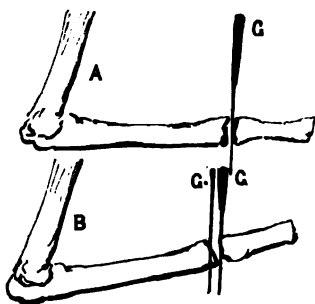
¹ The centre, y , is best attained after having adjusted the compass to the points x and p , by retaining one shaft of the compasses on the point x , and revolving it till the opposite shaft has attained the anatomical seat indicated.

The point *w* being now attained mark it with ink, and then *re-weight* the book as at the first; after having carefully estimated its weight in the mind remove it, but let the fingers and thumb be retained *in situ*, and then balance the book upon one of its corners, at the point *w*; at this point the book will weigh as heavy, or rather heavier, than when balanced on the fingers and thumb. If, then, at this point there was not some special provision for taking cognizance of gravity when placed as a counterpoise to muscular force, it is impossible to conceive how a weight, with so much increased leverage at the extremities, should weigh as heavy, or heavier, at this point, unless at this point the resultant of two forces is more directly applied to the sentient nerves recognizing this property in matter—weight, than at the tips of the fingers and thumb, where, if it have to travel to the chief joint before truly recognised, must lose much of the original force by friction and composition, and, consequently, be less perfectly felt than where, from its proximity to the sentient membrane, or nerves, such expenditure of force will not take place.

It must be observed that, if the book be placed anterior or posterior to the point *w*, the weight felt will be considerably less than at this particular point, *the hand and fingers being preserved in their original position*. Of course this check to the experiment only tends to confirm the previous inference.

Why it is better to place the book upon the point *w*, and not directly over the joint, as *determined by feeling* at the wrist, arises

FIG. 8.



from the fact that, in balancing a book upon the fingers and thumb, the hand and forearm are either kept in a line at right angles with the arm, or else adjust themselves to a line more or less acute to that part: in one man, the ability to weigh with comfort is obtained by the hand and forearm naturally assuming one position with regard to the arm; whilst, in another, a *slightly* different position is assumed; and according to the position of the forearm and hand to the arm, so must the weight be placed exactly over the joint, or half

a line to two lines anterior to this part, for the gravity to pass through the greater part of the joint, or sentient surface, as is illustrated in the etchings, A and B, where gravity, G, is represented as passing through the joints at variable angles, according to the degree of flexion of the forearm upon the arm.

It will be asked again, But do not different individuals present very different proportions in their hands, let them be adjusted in whatever way it may be thought proper to measure their dimensions? To this I

must reply by saying, that where the hand is not much used in severe hard labour, very little difference will be found in the most delicate hand, or the strongest, in the proportions here given, when the hand is properly adjusted—the little finger being the member most usually out of proportion, and perhaps all in a family will have the same peculiarity, yet this is any thing but common. The left hand, as a rule, is the best to measure from; this hand being generally least exposed to injury from severe or constant pressure, etc.

Before closing my remarks upon the hand, perhaps it will not be deemed unbecoming, though not directly related to the subject here discussed, if I give one or two measurements of the thumb in relation to the hand, in as few words as the subject will permit—a well proportioned hand being here assumed.

With a pair of compasses measure from the base of the metacarpal to the head of the first phalangeal bones of the thumb—the dorsal side is the best to measure from. Adjust the compasses, by one shaft, over the base of the metacarpal bone of the thumb on the *palmer* side, and with the free shaft describe a circle; the same shall pass over the metacarpo-phalangeal articulations of the index and little fingers. Next, extend the hand, approximate the fingers, but *abduct* the thumb: with the compasses adjusted as at first, place the point of one shaft on the outer, or radial side of the metacarpo-phalangeal articulation of the index finger, with the free shaft describe a circle, which shall pass over the thumb and little finger opposite their nails. Lastly, with the hand extended and fingers closed, adjust the compasses *across* the palm of the hand, parallel with the metacarpo-phalangeal articulations, when the first, or thumb measurement, shall be equal to the last. As it is not the design of this paper to enter upon the mechanism of the hand, I shall not press any inference from the above measurements, but leave the same to better and more able hands than mine. Yet I would suggest to any who might be so disposed, that in deducing inferences from the measurements of the thumb, it will be convenient to view the thumb from the metacarpal to the first phalangeal bones as a *moveable fulcrum*, and the *last* phalangeal as the lever.

In the foregoing sketch scarcely any thing has been said of the sense of force in relation to the feeling of *resistance or strength*, neither is it deemed suitable to make any lengthened remarks upon this feeling, when we consider that vigour or strength is the *normal* state of this sense in health, since muscular force, whilst unantagonised by gravitation (saving that pertaining to the limbs themselves), naturally channels along the osseous structures, through the joints, in that direction best adapted to excite the feeling of resistance or strength. Whilst, on the other hand, if a weight be placed upon any of the parts already indicated in making experiments for weight, and in place of using muscular force *sufficient* to counterpoise the imposed weight, that force is considerably *plus* over the gravitation (from rigidly contracting the muscles), then the gravitating force

will be brought to composition and resolution ere it reaches the joint, and therefore muscular force will alone remain, and will produce a feeling of vigour and strength in proportion to the amount of muscular contraction plus over the gravitation. The careful weighing of a book in the hand, and afterwards, with the same book in the hand, rigidly extending the arm and hand, will make apparent what is here desired to be understood. In weighing the book *little* muscular force is used to counterpoise gravity, and weight is the feeling induced; but when *much* muscular force is used, a feeling of vigour or strength follows, from the muscular force being considerably plus over the gravitating force. Of course the lines of direction in the two different feelings experienced—weight and resistance—will be in conformity with those maintained for the respective feelings at the commencement of this paper.

If very great muscular exertion is used to overcome gravitation, with a considerable plus of muscular force, there is a feeling or sensation over and above that of mere vigour, namely, a feeling of violent exertion distinct from either mere resistance or weight; this sensation arises in all probability from some other sentient impression than that of force only.

Having thus feebly finished the task I allotted to myself at the commencement of this paper, it is not becoming in me here to make any general remarks upon the application of the sense here advanced; for that would be premature whilst it is, as yet, *sub judice*. Moreover, this paper has only reference to the distribution of this sense to synovial membranes, but the writer is disposed to think that this sense has other seats of distribution besides that of synovial membranes, which will require a very careful examination in connection with several collateral subjects, which bear a nearer or more remote relation to the sense discussed.

The only thing the writer can with propriety at present refer to, is the testy subject of the *substratum* of matter; or, that there is something pertaining to matter of which neither sight, touch, nor smell, etc., can give us any information, these having relation to the superficies, yet it is evident that these are so many indications of its existence—the feel, colour, angles, etc., but not the substance; yet the fact that we are sure that these are the superficies, and not the substance of the same, arises from our consciousness of its possessing *weight*. And how? Because, though we cannot touch it, hear it, see it, smell it, or taste it, yet we can *feel* weight. And how do we feel it? Not, surely, because it is unrecognisable by any of our senses, or else how should we feel it? And if we feel it, and yet none of the recognised senses inform us of the presence of this said substratum, it remains, therefore, that a sense yet unrecognised gives us that assurance which the rest have failed to detect, and that that sense must embrace within its function the ability to detect weight. This, certainly, always assures us of the presence and amount of matter, or of its untouchable substance, or substratum,

which none of the orthodox senses have been able to detect, neither can they.

But, it will be said, that the philosophers and metaphysists do not affirm weight to be that elemental property in matter which gives to the examiner the conviction of the existence of a substratum which the orthodox senses cannot recognise. This I freely admit; yet I am disposed to think that the usual mode in which the subject is argued and referred to implies a conviction, arising from some impression matter gives to the philosopher distinct from the five recognised senses, and not having wherewith to express his inward impressions of the same, has recognised that conviction by the term *substratum*, which weight would have expressed *equally as well and more concisely*. Nevertheless, let it be granted that that elemental property in matter, called weight, does not include all that is implied by the term *substratum* (I mean when the term is used correctly), yet, if we had no sense capable of recognising weight and resistance in matter, those very characters, as weight and resistance, as distinct from roughness and smoothness, which above all others assure us of its presence and mechanical properties, would leave us in a perfect chaos of ignorance, both as how to use and apply matter, and whence it was governed, and what laws it obeyed,—direct revelation being in such case necessary for our subsistence and guidance.

ARTICLE III.—*On a Simple Method of performing the Operation for Fistula Lacrymalis.* By E. R. BICKERSTETH, Surgeon, Liverpool.

As far as I am aware it has hitherto been considered essential to the success of the operation for fistula lacrymalis, that the point of the knife should pass into, and be carried along, the lacrymal canal before attempting to introduce the style. As this is a proceeding which required some tact and minute anatomical information, it has at all times received from surgical authors considerable attention, and long and complicated directions are common, describing the method to be adopted to secure its ready and safe performance. The French writers have, as usual, surpassed the English in the minuteness of their description. Thus we have the steps of this little operation divided into the “*Premier temps*,” the “*Deuxième temps*,” and the “*Troisième temps*,” and half a page or more devoted to the discussion of each. Desmarres, in his work on eye diseases, when speaking of the “*premier temps*,” says:—
 “Et je lui recommande de tirer l’angle externe des paupières pour tendre les parties; et surtout pour faire saillir le tendon de l’orbiculaire. Ce tendon représente alors une ligne horizontale formant le côté supérieur d’un triangle dont le côté inférieur légèrement courbe est tracé par l’orbite. Partant du sommet du triangle, je compte de

dedans en dehors 4 à 5 milli-mètres, et là, je tire une ligne verticale dont la hauteur n'ayant pas plus de 5 milli-mètres, mesure la base du triangle dont je viens de parler. Je partage cette ligne en trois parties égales, et c'est à la réunion du deuxième tiers inférieur avec le tiers supérieur que la ponction sera faite—Ces dispositions prises," etc., etc., etc.

The utter futility of such a complicated plan for finding the proper point to open the lacrymal sac, must be obvious to all practical surgeons, for in the vast majority of cases requiring this operation, the swelling and tumefaction are such as to render it altogether impossible to make these dispositions. And when there is little or no alteration of the external parts, and the operation is desirable, nothing can be more simple or effectual than to feel the orifice of the canal, by pressing the nail of the forefinger deeply downwards immediately below the tendo-palpebrarum, and to introduce the knife, guided upon the nail, directly into the passage. This, in fact, is the proper method of proceeding whenever it can be adopted, but generally it is impracticable from the cause just mentioned. To pass the knife directly into the canal must then be more or less a matter of guess-work.

Until a recent period I was in the habit of judging its position by observing precisely its relative situation on the unaffected side, and then bearing this in mind, I was generally able to direct my bistoury at once into the obstructed passage. However, a case occurred last September, in which, owing to the almost horizontal direction of the canal, I signally failed, although after a good deal of poking about with the probe I succeeded in finding the passage, and then in introducing a style which passed without unusual resistance. The patient quickly recovered, but after the first few days a good deal of trouble was experienced in keeping the style from falling out, when she bent the head forwards.

This case naturally led to reflection regarding the pathological nature of obstruction in the lacrymal canal. It is no doubt various in different cases. It may arise from tumours occluding either extremity; it may be caused by necrosis of the bony textures entering into the formation of the tube, and this is by no means uncommon in children of a strumous habit; but by far the most frequent cause is from the extension of inflammation from the lacrymal sac to the lining membrane of the canal. Swelling takes place, and as an almost inevitable consequence the passage is choked up by secretion, and by the tumid state of its mucous membrane. When once produced, this state of matters may continue indefinitely, unless remedied by mechanical means; but I am not aware of any reason for believing that the canal ever becomes so completely filled up by densely organized material that the passage is obliterated, and a necessity created for using the knife in order to establish a new canal. Certainly, if such cases do exist, they form the exception to the general rule.

With this view of the subject I have ceased to operate as before, and now content myself by making a simple puncture into the abscess or lacrymal sac; and then introducing the style, guide it to the orifice of the canal, and with gentle pressure pass it along the tube. The ease and simplicity of this method of procedure is surprising as compared with the usual plan, and I believe it will be found very generally practicable, and equally efficacious in restoring the canal to a healthy condition. During the last five months I have adopted this simple means in six consecutive cases of confirmed fistula lacrymalis, with results at least as satisfactory as by the former system. In one case the fistula was of eighteen months' duration, yet the style passed without difficulty, and the patient made a rapid and complete recovery. In another case, the patient—a nervous and excitable female—could not bear the idea of being cut, I therefore insinuated the style along a fistulous opening, which fortunately happened to be nearly over the duct, and succeeded in passing it into the canal without trouble and without pain. She also made an excellent recovery, but an ugly scar remained from ulceration caused by the pressure of the style against the fistulous orifice, which was not quite over the canal.

ARTICLE IV.—*On the Epidemic Measles of 1854 in Leith.* By JOHN BROWN, L.R.C.S. Edin., H.E.I.C.S., Hon. Pres. Hunterian Med. Soc., Edin., late House-Surgeon, Leith Hospital.

IN the following communication it is my intention to describe the principal features of interest, which have occurred in the cases of measles, attended by me during 1853–4, while that disease was epidemic in the town of Leith.

It is necessary to remark, that these cases can only be held to represent the epidemic as occurring among the poorer classes, and that in consequence they may furnish in their complications and mortality some variations from the cases observed among those in more affluent circumstances; still, this will be found insufficient to explain the variable nature of the results at different periods, while the appreciable modifying causes, particularly at work among the poorer classes, were to a great extent stationary.

The town of Leith is favourably situated for the extension of epidemic influences. Lying low, and ineffectively cleansed and drained, in its centre, an overcrowded churchyard, and bounded on one side by a harbour which receives the vegetable and animal decay of most part of the towns of Edinburgh and Leith, and which, twice in the twenty-four hours, at low tide, gives off, by evaporation, the most offensive emanations. The town, in addition, is very densely inhabited, containing a population of 30,919, the greater majority of whom reside within an area of a few acres, in narrow streets and closes. If we look to the internal appearance of the houses of the

poorer classes, we find that they are generally small and ill-ventilated, over-crowded, and having free communication with each other, and in many instances *several* families are found living in the same room.

This epidemic may be said to have commenced in December 1853, and to have terminated at the end of June 1854. The cases attended during that period have been 170, of whom 16 died, being 9·7 per cent., or nearly 1 in 10.

The patients affected were generally below the age of five years, —a fact similar to what has been observed in former epidemics of this disease, and which is probably not so much dependent upon the predilection of measles for children, as upon its frequent occurrence as an epidemic, so that few attain that age without being under, and affected by, such an influence.

The following table shows the proportion attacked to the age:—

Under 1 year,	12	Between 7 and 8 years,	6
Between 1 and 2 years,	24	8 and 9 "	3
" 2 and 3 "	49	" 9 and 10 "	4
" 3 and 4 "	22	" 10 and 11 "	1
" 4 and 5 "	22	" 12 and 13 "	1
" 5 and 6 "	18	" 13 and 14 "	2
" 6 and 7 "	6	" 23 and 24 "	1

This table shows that of these 170 cases, 129 occurred below the age of 5; that the number affected was greatest between the second and third years, and that from this period a gradual decline in the number of attacks is noticeable.

General description of the disease in these 170 cases.—The premonitory symptoms (or period of febrile commotion) were generally mild febrile disturbance, rough cough, with slight dyspnoea, watery eyes and sneezing followed on the 5th day¹ by the appearance of the characteristic eruption, at first on the face, and gradually extending to the extremities. In three cases the eruption was delayed; in one appearing on the 6th day, in the other two on the 7th. In 3 cases symptoms of cynanche laryngea occurred during this stage; in 2 subsiding on the appearance of the rash; in 1 becoming less severe during its continuance, but again becoming aggravated on its recession. In no instance was the eruption ushered in by convulsions or cerebral disturbance.

The eruption was generally fully developed for 3 days, on the evening of the third day becoming faint and scarcely perceptible on the 6th; in none but severe cases was there any cuticular desquamation, and when this occurred the face was the only part distinctly affected.

Irregularity in the persistence of the eruption was observed in 9 cases; in 3 the eruption was ill coloured, and suddenly disappeared on the morning of the third day; in 2 cases, proving fatal in 24 hours by coma, in 1 by typhoid fever (7 days); in the 4th the eruption

¹ The appearance of eruption on fifth day was also observed by others in Leith.

was not previously seen by me, but was found absent on the 4th day; coma supervened, terminating in convulsions, and death on the 4th day from commencement of eruptive stage; in the 5th, 6th, and 7th cases the eruption remained vivid for five days; in the 8th case appearing scantily for 24 hours, suddenly receding, appearing again three days after, and permanently disappearing in 24 hours; in the 9th case the eruption was persistent for three days, reappearing every third day, and on the ninth day after its first appearance it became again vivid, and finally disappeared in 24 hours thereafter.

An imperfectly established but normally persistent eruption was present in eleven cases.

With one exception the period of the eruption in these 170 cases was unattended with any special complication requiring treatment; the exceptional case being one of croup, where that disease had manifested itself in the premonitory stage. In three cases the eruption supervened while the patients were convalescent from pertussis, a number too small to corroborate the opinion of Copland, West, and others, as to the connection between these two diseases.

In one case a second appearance of the disease was observed—the first attack having occurred two years before, and having been attended by catarrhal symptoms.¹

In no instance was the rubeola sine catarrho or sine exanthemate observed.

And now having discussed the general features of the disease in its premonitory and eruptive stages, we proceed to consider the general features of the disease in its decline and disappearance as exhibited in the sequelæ, and the complications arising therefrom.

1st. Of the Nervous System.—A remarkable immensity from sequelæ of this class was observed. In one case only, was there complete coma and convulsions; in other two cases, slight coma followed the sudden recession of the rash; these cases, however, are not so much specimens of sequelæ as of accidents arising suddenly, and referable not to a local lesion, but to a general one, namely, blood poisoning.

2d. Of the Respiratory System.—In fifty-four cases symptoms of acute bronchitis supervened, and in three cases proved fatal, the disease arising during the recession of the rash, and in all the cases observed the eruption being vivid, and the accompanying fever of the inflammatory character. In one case croup supervened and proved fatal, complicated with bronchitis and extreme collapse of lung. In two cases lobar pneumonia supervened—these cases recovered.

3d. Of the Digestive System.—In eight cases symptoms of dysentery supervened upon the subsidence of the eruption, and in one case proved fatal; latterly complicated with cancrum oris. In all the

¹ Upwards of 20 cases of rubeola have been twice attended by the medical men of Leith; in all, catarrhal symptoms were present. The seizures occurred at various periods of this epidemic, with four exceptions; two with an interval of two years, two with an interval of one year.

eruption was vivid, and the accompanying fever of the inflammatory form. In five cases aphthæ of the mouth supervened. In two cases only was enlargement of the submaxillary glands observed.

4th. Of Organs of Sense. No Sequelæ connected with the Ears was observed.—In two cases only did an ophthalmia occur requiring treatment; these were instances of the more severe form of catarrhal conjunctivitis, attended with some degree of palpebral swelling, and a muco-purulent discharge, but leaving no corneal ulcer. In no instance was the cornea primarily affected; in two cases to be noticed under the complications of typhoid fever, it was implicated secondarily.

5th. Sequelæ of a General Nature and probably connected with Blood Poisoning.—Under this head at least 2 of the cases of sequelæ of the nervous system might be put, probably all of the cases, but as this might be objected to, we have put them under the aforesaid head.

Excluding these accidental cases, we find that the sequelæ under this designation were consequent upon an imperfectly established, ill-coloured, and sometimes quickly receding eruption, and consisted of a typhoid form of fever, which supervened in nine cases, and in eight proved fatal. In two cases the fatal termination was ushered in by diptherite, in four by pneumonia, in one by tuberculosis and sloughing of both corneæ, in one by collapse of the lung; the case which recovered was not attended by any special complication. The arrangement of several apparently distinct diseases, under the head of complications, may be considered to be rather arbitrary, but as they seemed to be the direct effects of the sequelæ, it was thought advisable so to arrange them. The following table presents at a glance these various sequelæ, and the complications arising therefrom:—

SEQUELÆ.			COMPLICATIONS.		
Of respiratory organs,	.	57	Collapse of lung,	.	4
Of digestive organs,	.	15	Cancrum oris, and collapse of		
			lung,	.	1
Organs of sense,	.	2			
Typhoid fever,	.	9	Diptherite, 2; pneumonia, 4;		
			collapse of lung, 1; tuber-		
			culosis and sloughing of		
			both corneæ, 1	.	8

Having thus briefly sketched the leading features of this epidemic, we now proceed to describe the various forms which the disease assumed, and the tendency to sequelæ which characterised each variety. Dismissing from our consideration those cases which proved suddenly fatal from toxæmia, we may consider the disease to have exhibited itself in three forms, which were generally well defined.

1. The type of the disease, accompanied by a well-marked copious eruption, which appeared on the 5th day, with alleviation of the symptoms which preceded its appearance, a pulse but little accelerated, slight catarrhal symptoms; a critical discharge of urine on the 3d day, containing urates of soda; albumen in quantity suffi-

cient to be appreciable, frequently by chemical tests, if not by chemical tests, almost always disclosing the presence of blood corpuscles by the microscope in the cases examined, and a speedy return to health thereafter.

2. An inflammatory form, characterised by a vivid, copious eruption on the 5th day, the accession of which was attended by *aggravation* of the previous febrile symptoms, along with considerable hoarseness and dyspnoea, and on the 3d day, coincident with the fading of the rash, the signs of acute bronchitis or dysentery arising; these diseases being cut short if seen early, but if fully formed before treatment was adopted, proving frequently fatal.

3. A typhoid or asthenic form, with scanty, livid, and sometimes quickly receding eruption; in severe cases proving fatal on 3d or 4th day of eruption; in milder cases a typhoid form of fever occurring, attended with great prostration, dyspnoea, scanty or suppressed secretion of urine, with progressive emaciation, terminating generally in death, from the 8th to the 18th day; in all but one the chest being unaffected primarily, but in many the fatal termination being ushered in by inflammatory affection of the lungs.

We now proceed to inquire into the mortality of this epidemic, and the various cognate causes which may be supposed to have had an influence upon it, as well as the morbid anatomy in those cases in which a *post-mortem* examination was permitted.

In connection with this point we proceed to consider,

1st. *The Influence of Season, Temperature, etc., upon the Mortality of this Epidemic.*

Under this head we purpose not merely to inquire into the mortality of this epidemic as influenced by these combined causes, but also the relative proportion of sequelæ at different periods, and *their* mortality at such times. In the following rather complex table we have arranged the mortality in different months, as well as the total number of sequelæ from which that mortality was derived.

Months.	Cases.	Sequelæ and Relative Mortality.								Total Mortality.	Per Centage.
		Toxæmia.	Mortality.	Respiratory System.	Mortality.	Digestive Syst.	Mortality.	Typhoid Fever.	Mortality.		
December, .	5	1	...	2
January, .	5	2	...	1
February, .	34	1	1	14	...	1	1	2.94
March, .	49	1	1	20	2	1	...	6	6	9	18.03
April, .	35	1	1	11	2	2	1	2	1	5	14.28
May, .	23	4	...	4
June, .	19	5	...	4	...	1	1	1	5.26

We find from the above table that the highest mortality occurred in March, and arose principally from respiratory sequelæ and typhoid fever, while in the same month the greatest number of cases occurred. In connection with this subject, a table illustrative of the temperature and prevailing winds in these months was prepared, but the evidence was of so negative a character as not to warrant its insertion; the only point worthy of consideration with reference to the high mortality in March, was the frequent occurrence of wet days.

To give the true value to the varying mortality in different months, we must also bear in mind that this epidemic lasted for 7 months, and that probably epidemics, like individual diseases, have a development, a maturity, and a decline. We find, then, that the month of March may be held to represent the maturity of the epidemic, and the period of its greatest virulence. In fine, it may be considered that season influenced mortality, but that this influence was strengthened by the maturity of the epidemic at the period of greatest mortality. We next proceed to consider,

2d. *The Influence of Age upon the Mortality.*—I have arranged the results furnished under this head in the following table:—

Ages.	Total Cases.	Mortality.	Per Centage.	Ages.	Total Cases.	Mortality.	Per Centage.
Under 1 year, .	12	Between 7 and 8,	6
Between 1 and 2,	24	5	20.83	" 8 and 9,	3
" 2 and 3,	49	7	14.28	" 9 and 10,	4
" 3 and 4,	22	2	9.09	" 10 and 11,	1
" 4 and 5,	22	1	4.54	" 12 and 13,
" 5 and 6,	18	" 13 and 14,	2	1	50
" 6 and 7,	6	" 23 and 24,	1

From this table it would appear that the epidemic was most fatal to children between 1 and 2 years of age, a time corresponding to that of dentition; the mortality steadily declines from this period, ceasing between the age of 4 and 5, and not commencing again until at 13, when we have another fatal case. These cases show the fact alluded to by Copland, etc., that measles is most fatal during the period of dentition. The numbers about the age of puberty are not sufficiently great to corroborate the opinion as to the danger of measles at this period, although there is presumptive evidence in its favour, seeing that from the age of 5, up to a period approaching puberty, no fatal cases occurred. In accordance with the established views of infantile pathology, we would, *a priori*, have been inclined to expect the fatal cases which occurred during the period of dentition to have arisen from sequelæ, involving the respiratory or diges-

tive systems, seeing that sympathetic disorders of these systems are common at such a period; still further, we would have expected the respiratory system to have been solely affected, as in winter and spring (the times in which these cases occurred) sympathetic disorder of the respiratory system is common; while in summer and autumn sympathetic disorders of the digestive system preponderate. The results obtained do not, however, corroborate our expectations, for we find that, of the 5 cases fatal between the age of 1 and 2 years, one arose from sudden recession of an ill-coloured, scanty eruption, two from typhoid fever, one from bronchitis, and one from croup. While of 7 cases between 2 and 3 years, when the sympathetic irritation of dentition is nearly or entirely gone, we find that 2 cases prove fatal from bronchitis, 4 from typhoid fever, and 1 from sudden recession of an ill-coloured eruption. The fatal cases between 3 and 4 being one of typhoid fever and one of dysentery; between 4 and 5 one of typhoid fever; that between 13 and 14 being a case of speedy death from recession of rash.

As it might be objected that a mere recital of the ages at which the fatal cases occurred, can afford no valid information as to the frequency of the sequelæ at different ages, we have in the following table arranged the sequelæ according to the ages at which they occurred.

Ages.	Total Cases.	Respiratory Sequelæ.	Digestive Sequelæ.	Toxæmia.	Typhoid Fever.
Under 1 year,	12	6	1
Between 1 and 2,	24	5	1	1	2
" 2 and 3,	49	16	5	1	4
" 3 and 4,	22	7	2	...	2
" 4 and 5,	22	10	1	...	1
" 5 and 6,	18	5	1
" 6 and 7,	6	2	1
" 7 and 8,	6	1
" 8 and 9,	3	2
" 9 and 10,	4	3	1
" 10 and 11,	1	...	1
" 12 and 13,
" 13 and 14,	2	...	1	1	...
" 23 and 24,	1

This table, then, shows us that, during the period of active dentition, namely, between 1 and 2 years, the number of cases of respiratory and digestive sequelæ is considerably less than between the age of 2 and 3 years, a time when, in the great majority of instances, the process of dentition is completed. From the facts exhibited by

this table, we would be inclined to consider that in this epidemic age had no influence in predisposing to particular forms of sequelæ; in a word, we conceive that the type of the disease determined the form of sequelæ, while from the high mortality exhibited between 1 and 2 years of age, the coincidence of dentition may have influenced their fatality.

3d. Sex, as Influencing Mortality.—Of these 170 cases, 75 were boys, and 95 girls; of the boys 3 died, or 4 per cent.; of the girls 13 died, or 13·68 per cent.

It would thus appear that the number of females attacked was greater than that of males, a fact which may be owing to the excess of females in the population, seeing that according to the last census there were 110 females to 100 males living.

The mortality of females is enormously greater than of males; a fact at variance with what is observed in epidemics of fever, in which, although the seizures are fewer than in males the mortality is much less. If we look to the births which take place in this country, we find that 105 boys are born to 100 girls, while the whole population living gives a preponderance to females; in what way, then, can we account for the enormous mortality of females in this epidemic on such data? The researches of Professor Simpson have proved that while the *whole* mortality, of children, shows an excess of males, still that it is limited to the first year of life, and arises in consequence principally of nervous diseases induced during labour by the size of the male head.

In this epidemic we find that during the first year of life, while the condition of male and female are unequal, that no deaths arise; yet that when we come to a time when the conditions of the two sexes are equal, the mortality is in excess on the female side, we infer, therefore, that in this epidemic sex had a decided influence on mortality; whether it is always so with measles we cannot say, but it is certainly worth the attention of future observers. If we look to the relative number of sequelæ affecting each sex, as a means of explaining these results, we find that of 57 cases of sequelæ of the respiratory system, only 23 were girls, but the whole mortality belongs to that sex. Of 15 cases of sequelæ of the digestive system, we find that, although the seizures were nearly equal, the only fatal case belonged to the female sex. Of 3 cases fatal from toxæmia, we find 2 to be girls. Of 9 cases of typhoid fever 6 are girls, the only one that recovered being a boy. The conclusion we would be inclined under these circumstances to come to is, that while the total number of sequelæ shows the male sex to be most frequently affected, yet, that the mortality of that sex is notably less in consequence of their superior strength in resisting or overcoming disease when established.

4th. Constitution of Patients' previous Diseases, and State of Health at Invasion of Disease as influencing Mortality.—Upon this subject we will not enter into detail, seeing that their elaboration would

prove neither interesting nor instructive; we will, therefore, only particularly inquire into the constitutional states, etc., of the fatal cases, with such references as may be necessary to understand their relation to the recoveries. Of eight cases in which typhoid fever proved fatal, six were stout and of previous good health; the seventh was a twin of feeble health, but with no constitutional taint; the eighth was a feeble child in consequence of labouring under a severe attack of pertussis at the time of seizure; the only successful case was a robust boy, but of no better apparent health than the six fatal cases. Of four cases fatal from sequelæ of the respiratory system, all were previously in good health; in none of these cases was there any obvious constitutional taint, for farther on we will show that in the post-mortem examinations tuberculosis was absent in all the sequelæ of the respiratory system. One child, previously of robust health, and no previous disease, died of dysentery. Three children died suddenly from toxæmia; in two the previous state of health and constitution were good, in the third the state of health was doubtful.

It would appear that the typhoid fever attacked those of a robust constitution generally. The respiratory and digestive sequelæ attacked also the robust and strong, and we accordingly find that the fatal cases belong to that category; the cases of toxæmia are too small to come to any conclusion upon as to the state of health. In predisposing to this form, let me here remark that as the general rule the fatal cases were the most robust, while of the successful cases a great, a very large majority, were feeble strumous creatures; in a word, the epidemic in its mortality bore no obvious relation to a previously debilitated state, but the reverse. Were we to describe the kind of children we most dreaded to take measles, we would specify those in robust health, who either took that form designated as inflammatory, and in consequence bronchial inflammation, of a severe character, or a typhoid fever attended with great depression.

5th. Ventilation—state of dwelling, etc., as influencing Mortality.

—The evidence of the influence of these combined causes is also not of a nature which we would have been inclined to expect. In the three cases of sudden recession of rash and blood poisoning, the houses were all ill ventilated, overcrowded, badly lighted, and in one very damp. In the four fatal cases of respiratory sequelæ the dwellings were in three well ventilated, etc., in the fourth ill ventilated and overcrowded. In the only fatal case of sequelæ of the digestive system the dwelling was well ventilated and not overcrowded. In the eight fatal cases of typhoid fever the houses were in six well ventilated, etc., and in two ill ventilated and overcrowded. Generally speaking, no very close connection between the state of the dwelling and its ventilation, as compared with the results or complications of the disease was observed, although in predisposing to the disease, and leading to its extension, their influence was undoubted. In those cases in which sudden recession

of the rash was followed by symptoms of blood poisoning, we would be inclined to consider the state of the dwellings to have borne no unimportant relation to the results observed, but in the other cases the conditions present during the progress of the disease did not seem to cause any particular form of sequelæ, or influence their results. With the above exception we therefore consider that the mortality of this epidemic did not seem due to any hygienic imperfections, but more to the form of the disease taken, with which these imperfections bore no apparent relation.

6th, Period at which Treatment was commenced, and attention of Parents to orders, as influencing Mortality.—Generally speaking, a direct influence was observed between the successful issue and the early period at which the patients were seen, the exceptions being the cases of toxæmia and typhoid fever, in which cases remedies seemed of little avail. Although, from the short time intervening between the first visit and the fatal event, in the cases of blood poisoning, no treatment could have time to develop its effects. Of the 3 cases fatal in this way, 1 died 2 hours after first visit, 1 in 4 hours, and 1 in 7 hours. Of the 4 fatal cases of sequelæ of the respiratory system, 2 were not seen until the evidence of organic change in the lung was present (collapse); in 1 conjoined with croup, which had gone on to exudation; the fourth case was seen from the commencement, but went on to collapse of the lung. In the first three cases in which organic changes were present before treatment was commenced, the remedies prescribed were regularly administered. In the fourth case, seen from the commencement, the remedies were at the first administered very irregularly, and latterly not at all, the mother being engaged all day in field work, while the child was left in charge of one not much its senior.

The only fatal case of dysentery was well attended to, and the treatment was early commenced.

In the 8 fatal cases of typhoid fever, treatment was early commenced; and in most cases the orders given were attended to. It thus appears that of these 16 fatal cases, 7 were not seen until either the time before death was too short to permit of the action of remedies, or the time past for their use, when disease had progressed to organic change, or the remedies prescribed not administered. In these days of therapeutical scepticism, it is, we imagine, a conclusive evidence of the effect of remedies promptly administered in acute diseases, to show that of fifty-four cases in which bronchitis supervened, in many severe, the three cases which proved fatal did so from the want of early treatment; for we find that of these fifty-four cases three were nearly similar in all respects but one, and that being the treatment.

We have now concluded our remarks upon the causes which influenced the mortality in this epidemic, a subject which, no doubt, will be thought by many to have been dealt with too minutely, still by the means adopted only could we have given the proper value to

certain circumstances, generally acknowledged to bear in no small degree upon such a result. To recapitulate the results of these observations, we find—1. That as regards *season*, the spring months were those in which most fatal cases occurred. 2. That in this epidemic temperature and prevailing winds had probably no influence upon the mortality, but that coincident with the highest mortality a moist state of the atmosphere was observed. 3. That age had no direct influence upon the mortality, but, that as during dentition the highest mortality occurred, that period is unfavourable from the standard of health being lowered. 4. That sex seems to have exerted a powerful influence upon the mortality—the mortality on the female side being enormously greater than on the male. 5. That constitution, previous diseases, etc., evinced no direct influence over the mortality, but may have indirectly predisposed to certain forms of the disease, attended with complications. 6. That generally the influence of ventilation, state of dwelling, etc., upon the mortality, was not at all conclusive. 7. That generally the time at which treatment was commenced, and the degree of attention given to orders, exercised a powerful influence in diminishing or increasing the mortality. Before entering upon the post-mortem appearances, we would here observe that it is not meant to deny the positive influence of certain causes above enumerated in disease, but to show that in this epidemic the influence of these causes was of a negative kind.

Post-Mortem Appearances.—Of the sixteen fatal cases, post-mortem examinations were only permitted in eight. Of the three cases fatal from sudden recession of the rash, one was examined—

1. A stout girl, æt. 2, of previous good health, was attacked with measles. The eruption was scanty and discoloured, suddenly disappearing on the morning of the third day, attended with dyspnoea, feeble pulse, and a drowsy semi-comatose state, proving fatal in seventeen hours after the recession of the rash. Post-mortem, twenty-four hours after death. Body stout, and good deal of subcutaneous fat. *Thorax*—pericardium moist, heart healthy, right ventricle contains dark fluid blood in moderate quantity, left contracted and empty. *Pleuræ* non-adherent, moist; some vesicles of air between pulmonary *pleuræ* and lungs in front; lungs generally congested, most so at back part; right lower lobe non-crepitant, infiltrated with blood non-granular; bronchi exude a frothy fluid; trachea congested. *Abdomen*—liver large, congested; kidneys and spleen also congested.

Of those in whom bronchitis supervened and proved fatal, three in number, two were examined:—

1. A female child, æt. 15 months, previously healthy, was seized with measles, followed by aggravation of the cough, and febrile symptoms on the third day, simultaneous with the fading of the rash. These symptoms were neglected for about fourteen days; at this period medical advice was had recourse to for the first time; the child was found to be labouring under extreme collapse of the lung and croupy breathing, and died a few minutes after. Post-mortem thirty-six hours after death. Body pale, subcutaneous fat in tolerable quantity. *Thorax*—pericardium contains about a drachm of straw-coloured serum; heart valves normal; right ventricle distended, with dark coloured fluid blood; left ventricle contracted and empty. *Pleuræ* non-ad-

herent, moist; lungs both imperfectly crepitant throughout—in the substance of both lungs, numerous lobules of carnified tissue are observed; both lower lobes are condensed by bloody infiltration; bronchial tubes filled with purulent matter, tenacious and in greatest quantity in the smaller tubes; trachea congested; larynx, no deposit on free surface, but greatly thickened from a deposit in the submucous tissue of a serous character. *Abdomen*—liver, spleen, kidneys, normal.

2. A female child, *æt.* 2, previously healthy, was seized with measles, followed on the third day by an aggravation of the cough and fever; when first seen these symptoms had been allowed to go on, and the child seemed labouring under collapse of the lung, attended with dyspnoea and laryngeal cough; died suddenly, fourteen days from appearance of eruption. Post-mortem thirty hours after death. Body pale, emaciated. Thorax—pericardium moist; heart valves healthy; right ventricle dilated with semi-fluid dark blood; left, contracted, empty. Pleuræ non-adherent, moist; left lung, upper lobe, cedematous; lower, non-crepitant, whole lobe nearly composed of carnified lobules, with here and there a small portion of crepitant lung tissue; right lung, a large emphysematous bulla, size of a marble, is found between lung tissue and pulmonary pleuræ, at base in front; lower lobe condensed, the lobules generally have the usual appearance of carnified lung, but others are lighter coloured and imperfectly granular (lobular pneumonia)? Bronchi—all exude a tenacious purulent fluid, which is most abundant in the smaller bronchi. *Abdomen*—liver, spleen, kidneys, healthy.

The case which proved fatal from the supervention of croup was examined:—

1. A healthy stout female child, *æt.* 14 months, was seized with measles, which commenced to recede on the third day, attended by increase of the febrile symptoms, cough, and the physical signs of acute general bronchitis, which yielded, but left a laryngeal cough; in three days croup commenced, running on rapidly to death in twenty-four hours after the symptoms attracted notice. Post-mortem forty hours after death. Body pale, not emaciated; blueness of nails of hands and feet; a good deal of subcutaneous fat. Thorax—pericardium moist; heart valves normal; right side gorged with semi-decolorised clots; left empty and contracted. Pleuræ moist, non-adherent; lungs, left upper lobe crepitant, slightly cedematous; lower lobe sparingly crepitant, numerous carnified lobules, with cedematous crepitant tissue between; right upper lobe crepitant, slightly cedematous; middle lobe small, nodulated externally (like as is seen in advanced cirrhosis of liver), hard, non-crepitant. This lobe is made up of these nodules, which seem fibrous on section, and contain no cavity; between them are pieces of non-crepitant lung tissue, which become crepitant by inflation, the fibrous bodies remaining unaffected; they apparently represent an extreme degree of collapse; lower lobe imperfectly crepitant, carnified in patches; *larynx and trachea*, rimæ plugged by a fibrous mass extending fully an inch down trachea, non-adherent, of the thickness and shape of an ordinary goose-quill; trachea congested to its bifurcation, no lymph on its surface; at the bifurcation a yellowish and very tenacious fluid is found proceeding downwards to the smaller tubes of both lungs, which are completely filled. *Abdomen*—liver, spleen, and kidneys, healthy.

The diseases of the digestive system include one fatal case of dysentery, which was examined:—

1. A stout healthy female child, *æt.* 3, seized with measles, followed on the 3d day (synchronous with recession of the rash) by symptoms of acute dysentery. On 15th day of its duration *cancrem oris* supervened, destroying part of the upper lip;—the two upper incisors, and about half of the hard palate, with com-

inencing death of the bone beneath ; died exhausted three days after its super-vention, or 17th day from commencement of dysentery ; no chest complication. Post-mortem thirty hours after death. Body pale, thin, little subcutaneous fat. Thorax—pericardium moist ; heart valves normal ; right side distended, with semi-fluid blood ; left ventricle contracted and empty. Pleuræ non-adherent, moist ; lungs, *left*, marked emphysema of edge of upper lobe in front, vesicular and interlobular—the latter preponderating. Upper lobe imperfectly crepitant, with carnified portions in centre about half an inch square ; lower lobe sparingly crepitant, congested, at back part, numerous collapsed lobules. *Right*, upper lobe crepitant, with vesicular emphysema at apex in front, and atrophy of edges ; middle lobe œdematous ; lower lobe condensed, with blood, no granulations. Bronchi generally exude a pale frothy fluid on pressure. *Abdomen*, stomach, tissue soft, contains a little milk and bread ; not congested. Whole *intestine*, from stomach to within a foot of rectal end, normal, and stained throughout by bile ; about twelve inches from anus two or three ulcers, about the size of a split pea, are found, they had only destroyed the mucous membrane ; for the last six inches of its course the whole rectum is covered by a deposit of lymph of honey-comb appearance, which is easily stripped off from the congested mucous membrane beneath. Liver, spleen, and kidneys, normal.

Of the fatal cases of typhoid fever, eight in number, four were examined after death :—

1. A stout female child, æt. 1½, was attacked by an imperfect eruption of measles, which commenced to recede quickly on the third day, attended by a typhoid form of fever and great dyspnoea ; this state continued for seven days, no physical signs of chest disease having in the interval occurred, but on the evening of the seventh day a fine moist rale was heard in the lower and back part of both sides, and she died early on the eighth day of the typhoid state. Post-mortem thirty-five hours after death. Body pale, not emaciated ; a good deal of subcutaneous fat. Thorax—pericardium contains about a drachm of straw-coloured serum ; heart valves normal ; right side distended, with semi-fluid dark blood. Pleuræ—A few patches of lymph on diaphragmatic surface of right pulmonary and costal surfaces of pleuræ ; *right lung*—all this lung, except a portion of about two square inches at apex, is condensed ; on section the colour of the lung is a dirty straw, with granulations : the upper lobe is not wholly in this state, there being patches of one and two square inches, with congested lung tissue between ; the other lobes are uniformly granular ; *left*, upper lobe crepitant, œdematous ; lower, condensed, red, and granular on section. Bronchi, especially the smaller, exude purulent mucus on pressure. *Abdomen*—liver, spleen, and kidneys, congested ; otherwise normal.

2. A stout healthy female child, æt. 2½, was seized with an imperfect eruption of measles, and on the third day fell into a low typhoid state, attended with dyspnoea, but no physical signs of chest affection on the 7th day of the typhoid state ; both cornea observed to become dim, with no previous inflammatory signs beyond slight injection of the palpebral conjunctiva ; this state was succeeded next day by ulceration between cornea and sclerotic ; ultimately both eyes sloughed, leaving a small fleshy looking mass at bottom of each orbit ; died of exhaustion on tenth day of fever, or thirteenth day from appearance of eruption. Post-mortem forty-six hours after death—Body pale, emaciated ; little subcutaneous fat ; eyelids sunk ; cornea, etc., gone. Head—membranes pale, no deposit. Brain—pale, firm ; lateral ventricles contain a little clear serum ; optic commissure, and nerves firm, but, like the brain, very anæmic. Thorax—pericardium contains about half an ounce straw-coloured serum ; heart valves normal ; right side distended, with dark fluid blood ; left auricle contains a fibrinous coagulum ; ventricle contracted, empty. Pleuræ moist, no adhesion ; lungs, left upper lobe imperfectly crepitant, contains at

apex two yellowish cheesy masses, each about half an inch square, with defined borders; lower lobe condensed, with blood and carnified lobules; *right*, upper, and middle lobes crepitant, lower imperfectly crepitant, containing numerous crude miliary tubercles. *Abdomen*—liver, spleen, and kidneys normal.

3. A stout female child, *æt.* 2, had been labouring under well marked pertussis for several weeks, with well-formed hoop and accompanying bronchitis; the paroxysms were becoming less frequent and severe, when it was seized with measles, which came out imperfectly, receding suddenly in twenty-four hours, again appearing imperfectly three days after, and permanently disappearing in twenty-four hours. She fell into a low typhoid state, with slight dyspnoea, and died exhausted on nineteenth day of typhoid state, the chest signs being, during that period, slight inspiratory roughness, with no marked rale. Post-mortem forty-eight hours after death—Body pale, emaciated; little subcutaneous fat. Thorax—pericardium moist; heart valves normal, right side filled with semi-decoloured clots; left contracted, empty; pleurae, no adhesions, moist; *lungs*, left upper lobe crepitant; lower lobe condensed, tough, carnified; *right*, upper lobe, superior half, crepitant, inferior half, non-crepitant, externally presenting a nodulated appearance, exactly similar to that observed in cirrhosis of the liver in an advanced stage; the middle lobe is atrophied, and seems made up of these nodules, which, on being cut into, are firm, fibrous-looking, and with no apparent cavities; they are about the size of a small marble, and have an imperfectly crepitant tissue between; lower lobe non-crepitant, carnified; smaller bronchi exude a tenacious yellow fluid. *Abdomen*—liver, and spleen, normal; kidneys congested, lobulated.

4. A stout healthy female child, *æt.* 2, was seized with an imperfect eruption of measles, which receded quickly on the third day, followed by a typhoid state, accompanied with dyspnoea, slight croupal symptoms, and no chest signs beyond inspiratory roughness, and for a few days before death dorsal impairment of percussion, with resistance. Died exhausted, on nineteenth day, of fever, a small abscess of cornea commencing. Post-mortem forty-eight hours after death—Body pale, emaciated, little subcutaneous fat. Thorax—pericardium moist; heart, valves normal; right side gorged with semi-decoloured clots; left, contracted, empty. Pleurae—recent adhesions of pleura in front of left lung, soft, white, and easily detached from opposed surfaces; *lungs*, left upper lobe crepitant, normal; lower, condensed, carnified; *right*, upper lobe, crepitant; middle, condensed, carnified; lower lobe, imperfectly crepitant, containing some lobules of fawn-colour and granular, with healthy crepitant tissue between; larynx and trachea, pale, no exudation. *Abdomen*—liver pale, spleen and kidneys normal.

To recapitulate the results of these post-mortem examinations, we find,—1st. That in the case of toxæmia, from sudden recession of the rash, the only changes observed were extreme congestion of the viscera, and a fluid state of the blood.

2d. That in the cases of respiratory sequelæ examined, the evidences of marked collapse were present in all, and coexistent with a state favourable for its development; but that emphysema was generally absent, contrary to what is found in the adult, and probably so in consequence of the conditions necessary for its production, being absent owing to the enfeebled state the patients were in, and the yielding state of the parieties of the chest.

3d. That in the fatal case of digestive sequelæ—a case of dysentery—we have the unusual circumstance of the formation of a false membrane upon the interior of the intestine, and also evidence of the occurrence of marked collapse of the lung in a child who never

had bronchial disease, but who had been lying in a semi-lifeless state for several days, thus proving the accuracy of the opinion of Dr West, that collapse may occur in such circumstances without bronchitis.

4th. That in the four cases fatal from typhoid fever which were examined, the parenchyma of the lungs was primarily affected in two; and in two secondarily, in the one case labouring under pertussis, the lesions found seemed more the result of it than of measles.

ARTICLE V.—*Postscript to a Paper in the January Number of this Journal, confirming the discovery by Keber of a Remarkable Body Penetrating into the Ovum of the Fresh Water Mussel.* By MARTIN BARRY, M.D., F.R.S.

HAD this discovery by Keber extended no further than that of the penetration into the interior of the ovum of a body having always the same appearance, form, and size, and this by a funnel-shaped canal evidently provided for the purpose,—had Keber's discovery gone no further than even this, there would have been sufficient in it to induce the thought with every one deserving to be called an embryologist, "What else can this be than a spermatozoon?" But when to that discovery he has it in his power to add, This body which I saw, not only at the very mouth of the micropyle, but in countless instances at all degrees of penetration through that canal, and in the yelk itself,—this body which has always the same general appearance, colour, form, and size,—and after getting into the ovum divides into many parts,—this body *I know to be a spermatozoon, for I have traced the spermatozoon of the same animal from its earliest origin through all stages of development, and become perfectly familiar with its appearance, colour, form, and size*: I say, when a man has it in his power, as Keber has, to add all this, he speaks with authority which no man has a right to question, until he shows that Keber misinterpreted what he saw; for at mere denials a man sure of his facts can afford to laugh and "bide the time," as some of us have done before him.

Let observers mark well what Keber says of *the size* of the ova in which they should find a funnel-shaped micropyle, and the entering or entered spermatozoon, namely, ova of $\frac{1}{40}$ " to $\frac{1}{30}$ ". And let them notice his experience of the season of the year—*September*—in which ova of this size are most frequent. By attending to these two things they will not have to look long before seeing a confirmation in several parts of the field of view; for when once seen, the objects in question are so easily recognized that the observer wonders how it was that they were not seen sooner. If they neglect attention to the size of ova, they may perhaps see a globule of albumen, and think that Keber

took this for a spermatozoon; instead of which he pointed out particularly, in his first work,¹ the nature of the same. All that Keber asks for is, that observers should *rigorously test* his observations. I never read an account of a discovery in which such testing was shown to be more anxiously desired. Only let me warn observers not to publish denials until they are quite sure that they have been looking at *the thing which Keber pointed out*; otherwise they will make themselves ridiculous by "a beating of the air," and wish that what they wrote could be expunged. I have seen smaller ova of the animal in question in which, as Keber says, no micropyle existed. Again, I have seen larger ova in which no remains of the micropyle could be discerned, while in ova of the size above mentioned, I think I never failed to find it when the ovum lay in a manner which made the seeing of it possible. And rarely has it happened that in ova of about the said size, I did not find the remarkable body in question either entering or already entered. He who has not seen all this, and traced the spermatozoon from its earliest origin through all stages of its development to a body not distinguishable from that which penetrates into the interior of the ovum, if he respects his own reputation, let him pause before denying the discovery of Keber.

Keber terms the spermatozoa *cells*. I have been in the habit of terming their large extremity a *nucleus*. No doubt, however, if it be a nucleus it is vesicular, as other nuclei become. And really it is no easy matter to point out where the *nucleus* ends, and where the *cell* begins; that is to say, it would not be easy to point out this in a manner that would be satisfactory to every eye. There is a fact, however, noticed by myself, of which I omitted to make mention, and which it may be worth while to make known here, as a fact which is in perfect keeping with what I saw to follow the penetration of the spermatozoon into the ovum of the rabbit. My fact is this: that the many parts into which Keber saw the said large extremity to divide, *are nuclei themselves*.

Second Postscript, confirming Keber's view that the Penetrating Body is a Spermatozoon.

I have lately (in March 1855) resumed my examinations of unio, directing my attention chiefly to the *testis*. It is now in my power to state, with Dr Keber, that *I am quite incapable of seeing any difference either in size, oblong form, or behaviour towards light, between the body or anterior end of the mature spermatozoon of unio, and the body which he has figured entering its ovum.*

M. BARRY.

¹ *De Spermatozoorum Introitu in Ovula*. Königsberg, 1853.

Part Second.

REVIEWS.

L'Huile de Foie de Morue Envisagée sous tous les Rapports Comme moyen thérapeutique. Par L. J. DE JONGH, Docteur de médecin à la Haye.

Cod Liver Oil regarded in all its relations as a therapeutic agent. By L. J. de JONGH, Doctor of Medicine at the Hague. Paris, 1853. 8vo, pp. 262.

De l'Huile de Foie de Morue et de son Usage en Médecine. Par M. Ed. TAUFFLIEB, Docteur en Médecine, à Barr (Bas-Rhin).

On Cod Liver Oil and its Use in Medicine. By M. Ed. TAUFFLIEB, Doctor of Medicine at Barr. Paris, 1853. 8vo, pp. 95.

The Pathology of the Broncho-Pulmonary Mucous Membrane. By C. BLACK, M.D., etc. Edinburgh, 1855. Part II.

SINCE the year 1841, when Professor Bennett first introduced cod-liver oil into the medical practice of this country as a valuable means of removing scrofulous and tubercular diseases, its therapeutical and commercial importance has gradually increased to an extent seldom witnessed in the history of the materia medica. Previous to the publication of his monograph, indeed, it had been employed here and there, and especially by Kay and the elder Bardsley of Manchester, as a remedy in rheumatism. But even for that disease it had been allowed to fall into unmerited neglect, so that when Dr Bennett, after watching its good effects in some German hospitals, first wrote, almost the entire profession were unacquainted with its value, while druggists were in a complete state of ignorance as to its properties, and knew not even where to obtain it. The amount retailed by one of the first druggists of Edinburgh in 1841, was one gallon, and that altogether to tanners and shoemakers, for the purpose of softening leather, whilst during the last year the same druggist has disposed of no less than 600 gallons! So little disposed were medical practitioners in general to employ this substance, that it was with the greatest difficulty the proposer could induce even his personal friends to give it a trial. Two exceptions to this, indeed, deserve honourable mention, viz., Dr Andrew Wood and the late Dr Robert Spittal, the first of whom tried it largely in several public institutions for children, and at once observed its good effects; while the latter was the first hospital physician who administered it, and satisfied himself of its value in the phthisical cases of the Royal Infirmary. At this period cod-liver oil was the subject of

almost universal ridicule—its very name could not be uttered without exciting smiles and jests, and, if alluded to in a class-room, a broad grin was observed on the face of every student. The leading journal of those days commenced its critical notice of Dr Bennett's monograph as follows: "We are a little surprised that any one whose aim is the promotion of medical science, which we believe to be that of the author of this treatise, not mere personal notoriety, should take such pains in introducing to the notice of his brethren any single therapeutic agent." Then, during four pages which had for their object no censure, but simply to persuade the professional public that the oil was useless or inert; it concludes with "expressing our fears that the *oleum jecoris aselli* will prove far from equal to the expectations which Dr Bennett's treatise is likely to excite in its favour."¹

Notwithstanding every species of discouragement, however, Dr Bennett persevered in recommending it to the favourable consideration of his medical brethren, and in his Lectures on the Practice of Physic, and especially in the Poly-Clinic which he established at the Royal Dispensary, he continued to demonstrate its usefulness. The correspondence and trouble he undertook answering inquiries with regard to the oil, and the efforts he made to improve its manufacture in Scotland, though not likely ever to be generally appreciated, were at least eminently successful. Gradually its importance came to be recognised, for inasmuch as phthisis and other grave diseases were avowedly incurable, there could be no harm in trying a new remedy. Once arrived at this point its permanent adoption was secured, for we have never yet met with a man possessing any power of observation whatever, that after a certain time did not recognise its analeptic properties. In the autumn of 1847, Dr Bennett published an appendix to his work, since which time its employment in Scotland became general. In 1849 all his facts were confirmed by Dr C. J. B. Williams of London, and in 1851 the medical officers of the Brompton Consumptive Hospital, published a report giving the most decided opinion as to its good effects in phthisical and other scrofulous diseases. Since then the demand for cod-liver oil has become as great in England as it had previously been in Scotland, and now the consumption of it may be said to be enormous, its manufacture and sale at present constituting a most important branch of commerce. Such is the history of the introduction of cod-liver oil into the medical practice of Great Britain.

But when, in 1849, the good effects of cod-liver oil were, after a trial of eight years, generally acknowledged, the theory of its action became a subject of lively discussion. The theory contended for by Dr Bennett is founded upon the fact discovered by Ascherson, that oil and albumen, when brought into contact, always occasion a membranous coagulation of the latter, and that when

¹ See Brit. and For. Med. Rev., January 1842.

rubbed together they produce an emulsion. Introduced at a time when histological knowledge was only entering into the domain of science, the importance of this fact could scarcely be supposed to impress itself favourably on the minds of medical men. That nutrition was in any way connected with so mechanical a process as the making an emulsion out of oil and albumen, seemed too absurd to be entertained for a moment; and even those who, at length, could no longer shut their eyes to its good effects in practice, continued to ridicule without mercy the histological theory which had been advanced to explain its *modus operandi*. The following good humoured distich and note, brought out at an annual dinner of one of our medical institutions, and since printed for private circulation, indicates the sort of objections entertained of this doctrine.

"There Williams writes that Bennett has no merit,
To prove that oil and eggs our tissues cause;
I own that for myself I ne'er could bear it,
To think that human blood was salad sauce."

How to Make a Structure.—If we place a drop of oil and another of albumen on a slip of glass, and allow the one to flow over the other, a pellicle will be observed to have formed. This, examined microscopically, presents the appearance of a membrane sometimes puckered and thrown into elegant folds. . . . If now we unite the two drops by means of friction, we form an emulsion.—*Dr Bennett on the Structural Relation of Oil and Albumen, Monthly Journal, Sept. 1847.*

How to make Salad Sauce.—Put into a large basin the yolks only of two fine and very fresh eggs, carefully freed from the germs, with a little salt and cayenne, stir them well together, then add about a teaspoonful of the purest salad oil, and work the mixture round with a wooden spoon until it appears like cream.—*Modern Cookery by Eliza Acton, p. 113.*

Yet although it was for a long time difficult for those who had not followed the progress of physiological science to comprehend how the mixture of oil and albumen in the stomach and intestines could tend to the formation of healthy blood, a consideration of the constitution of the chyle appears to us to explain this matter without any difficulty. Thus, chyle is an emulsion formed by the union of oil and albumen derived from the food; and no one can doubt that, out of that chyle or fatty emulsion blood is formed. When, then, after giving the oil to emaciated persons, we observe that they become stronger, more muscular, and fatter, it requires little reasoning to prove that we must have added something to the chyle, through the chyle to the blood, and through the blood to the tissues. Again, when we examine chyle microscopically, we find it to be composed of exceedingly minute particles, or to consist of a molecular basis, which is soluble in æther, and therefore fatty, and that as it flows from the intestines towards the blood, through the mesenteric glands, and, lastly, through the thoracic duct, blood corpuscles may be seen to form in it. Hence, the molecular is a primary and evolving element, out of which the blood corpuscles and the blood itself are evi-

dently formed. This view, modified as to one or more unimportant details, is the one which is in most accordance with physiology, and is most generally adopted.

This theory still finds many objectors, the latest of whom is Dr Black, who says of it—

"It may be urged that, were fat a 'primary and evolving element,' as considered by Dr Bennett, it would not be diminished in quantity as it flows along the lacteals; that nature would not thus all but destroy the very resources whence the nuclei of the tissues are to be drawn; that it would exist in greater proportion in the blood; that the quantity found in that fluid is manifestly insufficient to form with albumen the necessary nuclei for the proper nutrition of the tissues; that it would form a constituent part of healthy muscular fibre, and of other structures having a cellular basis of formation; and that, when found as a constituent part of such tissues, it would not be, as it invariably is, associated with disease. It seems, therefore, more consonant with facts to regard fat, absolutely considered as such, as taking no part in the formation and nutrition of tissues, than to look upon it as a 'primary and evolving element' of such tissues."—P. 146.

Here Dr Black has evidently misunderstood what is meant by "primary and evolving element." In a note he tells us that he supposes it to mean an element which enters directly into the formation of tissues. But this, so far from being the primary, is really the ultimate element of histogenesis. By the primary element histologists comprehend the first morphological appearance which presents itself, and this is everywhere a mass of minute molecules and granules which, by uniting together, constitute primary nuclei, around which primary cells are formed. All these elements, however, are but temporary, and undergo successive elaborations, evolutions, and reformations, before they directly enter the tissues. The argument advanced by Dr Black, namely, that fat is not a primary evolving element, because it diminishes as it flows along the lacteals, is exactly that which every histologist employs to show how, by its transformations, it becomes prepared to form tissue. That oil may serve to form albumen, and that albumen may be transformed into oil, nobody can have any doubt who watches in an ovum the development of an embryo. Indeed, although we are willing to pay every attention to Dr Black's objections, and to give him credit for sincerity in expressing his opinions, it is impossible to overlook the fact, that he is not familiar with the great progress recently made in histology and embryology. He has been led away too far by the hypothesis of germinal centres, and, from what he has advanced, does not appear to us sufficiently to appreciate what may be called the molecular theory of morphology.

Passing, however, from these speculations, it may be asked, how is it that, if cod-liver oil operates in virtue of its oily or fatty matter, that any other oil or fat does not answer the same purpose? Now the fact is, under ordinary circumstances, the stomach and alimentary canal are capable of reducing all kinds of fat to the required fluidity and purity necessary for the formation of chyle. But there are a

certain class of cases where the digestive powers are evidently impaired, and cannot do so. We shall not at present enter into the theoretical views by which some have endeavoured to account for the peculiar form of dyspepsia in these cases, because we should thereby be led away too far. We may safely assume that there is such indigestion, and in the vast majority of cases a remarkable antipathy to eating all kinds of fatty, or, as it is called, rich food. It has indeed been asserted—and we find it is so by Dr Black—that tubercle frequently occurs without the slightest symptom of a dyspeptic character being present. If by this be understood that individuals who are not habitually underfed (as often occurs among the lower classes) become tubercular, and waste away, whilst the appetite remains good and digestion is perfect, we will venture to say the assertion is altogether opposed to experience. The statements of patients, indeed, would often lead a superficial examiner to conclude that their appetite and digestive powers are everything that could be wished; but we need scarcely say that the hopeful and flattering accounts given by consumptives of themselves are not likely to deceive the sagacious physician, still less to be received by him as an argument for or against any particular theory. Our own notion, therefore, is, that in giving animal oils, we introduce an essential element of nutrition, already so prepared that it is rendered easily assimilable. Of this we are convinced, that it is in proportion to its capacity of being digested, and of forming good chyle, that its good effects are rendered obvious.

But why should cod-liver oil be more assimilable than any other kind of oil? This is a difficult question to answer. The vegetable oils, indeed, are more or less purgative, and have been largely tried, without producing any benefit. Many other animal oils have been employed, and, it is said, with some effect—for instance, neat's foot oil. We have given scate, shark, and dog-fish oil—for samples of which we hereby thank the donors—all with good effect; and we believe that these oils are little, if at all inferior to cod-liver oil. We have also given various kinds of genuine cod oil, which have been kindly forwarded to us from various parts of this country, and from abroad, and have especially to thank Mr Archibald of Newfoundland, Mr Fox of Scarborough, and Mr Hansard of London, for specimens recently forwarded to us.

We have no difficulty whatever in speaking of the rival claims of the different manufacturers of this substance, because we believe—in accordance with the views previously stated—that they are all good, and are medically of equal value. Yet these oils vary greatly in their physical properties. Here we cannot in justice forbear mentioning that the Messrs Parker, oil manufacturers in Leith Walk, have for many years—indeed long before Dr C. J. B. Williams spoke of the advantages of giving a pure oil—manufactured it as pure and tasteless as has been done by anybody. It is unnecessary for them to make their oil known by advertisement, for the simple

reason that every drop they can produce is eagerly purchased by the druggists of Edinburgh. They were the first, as they are still among the best and cheapest producers of this substance. To meet the increased demand in England, however, an excellent, pure, and almost tasteless oil has been brought into the market, and that of Messrs Archibald and Fox have in these respects certainly never been surpassed. Those persons, therefore, who find, from experience, that they can take with greater ease those light and comparatively tasteless oils, may with great confidence employ any of them.

The notion, however, that a pale oil is superior to the brown kind, is most erroneous. Throughout Germany and the northern kingdoms of Europe the brown oil is preferred, and the assertion so readily made, that its more nauseous smell and taste prevent its introduction into practice, or interfere with its assimilation, is entirely opposed to the most extended series of facts. We can declare, from the result of no small experience, that not unfrequently the brown oil is retained on the stomach, when the light oil is not, and that children more especially, contrary to what may be supposed, in many cases prefer the brown to the more tasteless kinds. Many describe the brown oil as stimulating and acting like a cordial. The conclusion we have arrived at then, with regard to the superiority of one or other kind of oil, amounts to this, that that is the best which is most easily taken, retained on the stomach, and digested, and when we have to do with a capricious organ, experiment only can decide which that shall be.

It is only right, however, to give Dr de Jongh's view of the action of the oil, which is as follows:—

“Our analyses have shown that cod-liver oil is a very compound remedy. We find in it fatty matters, biliary principles, iodine, phosphoric acid, phosphorus, butyric acid, gaduine, and several inorganic salts. To which of these principles, many of which are recognised as very active remedies, does cod-liver oil owe its salutary action in rheumatismal and scrofulous affections? Is it the iodine, to the fatty principle, to phosphorus, to other matters, or to the action of all these principles united? If we consider that, in the diseases in which cod-liver oil exhibits an incontestible value, the digestion is augmented, the nutrition ameliorated, the secretions excited, the function of the lymphatic system revived, and lastly, that the ganglionic system is affected, we shall be easily convinced that it is not the biliary principles, not the fatty matters, not iodine, or any other single one which can meet all these indications. Hence why we attribute the efficacy of cod-liver oil, as much to the combined action of all these principles as to that of many among them.”—P. 255.

This is certainly an ingenious way of avoiding a difficulty. Dr de Jongh avows his want of power to resolve the question of how one or more of these principles act, and evidently regards cod-liver oil as a mixture made up of certain precious ingredients, each of which has its destined, though undiscovered object, while the whole of them unite in producing a given effect. Hence, only the oil that contains all these ingredients is a good oil, and as they are present in the brown kind as determined by analysis, this is the oil to be preferred.

Now, without denying that, therapeutically, there may be something in this mode of reasoning, which perhaps may explain why the stomach of one person agrees with one oil, and another with a different one; it is not necessary to suppose that the different organs and glands of the economy are affected by the various substances of which the oil is composed. Physiology tells us that if we can succeed in stimulating the first stages of nutrition in scrofulous cases, the subsequent ones will be necessarily forced into renewed action. Make good chyle, and good blood and tissues will follow. In this case especially, *c'est le premier pas qui coute*. But be this as it may, the introduction of cod-liver oil into medical practice is now universally recognised to have been productive of great good, and in the works of MM. Taufflieb and De Jongh will be found an excellent resumé of the class of cases, with examples in which it has been found most beneficial. Although we have only alluded to Dr Black's observations in reference to the mode of action of oil, we can recommend his *brochure* as a very excellent one on the subject of which it treats. The figures in the plate he has given will be recognised by every histologist as being well executed and true to nature.

Epilepsy and other Affections of the Nervous System which are marked by Tremor, Convulsion, or Spasm; their Pathology and Treatment. By CHARLES BLAND RADCLIFFE, M.D., Assistant Physician to the Westminster Hospital, etc. London, 1854. 8vo, pp. 144.

Eutherapeia: or, an Examination of the Principles of Medical Science, with Researches in the Nervous System. By ROBERT GARNER, Surgeon to the North Staffordshire Infirmary, etc. London, 1855. 8vo, pp. 282.

On the Pathology of Delirium Tremens; and its Treatment without Stimulants or Opiates. By ALEXANDER PEDDIE, M.D., Edin. Edinburgh, 1854. 8vo, pp. 51.

(Continued from p. 245.)

By epilepsy we understand a disease of the nervous system occurring in paroxysms, in which consciousness is suspended, whilst the muscles are thrown into spasms more or less general. The abolition of cerebral, and the excitation of motor functions have always rendered the pathology of the disease a matter of obscurity, and its paroxysmal character, in the intervals of which, all the functions of the body frequently proceed without change, have tended still further to perplex the scientific inquirer. The work of Dr Radcliffe has for its object the elucidation of the epileptic phenomena, and the improvement of their medical management. Hitherto it

has been supposed that sometimes the disease may depend upon an increase, and at others upon a diminution of the vital powers of the system. According to Dr Radcliffe, it is *always* connected with debility, and always requires a tonic and stimulating treatment. In arriving at this conclusion, he has been led into a lengthened physiological argument which displays great ingenuity and very extensive knowledge of the physiology and pathology of the nervous system, a branch of inquiry, indeed, which all those who have cultivated it, know has been pursued with great zeal and success by Dr Radcliffe. A rather copious analysis of his work, therefore, is required to do his argument justice, as well as to place the modern views of epilepsy fully before our readers:—

In the first place the author examines muscular contraction as manifested in ordinary muscle, in the coats of vessels, and in the heart.

The contraction which is manifested in ordinary muscle cannot he thinks, be regarded, as the result of any kind of stimulation, for the following reasons:—An involuntary muscle is much more sparingly supplied with nerves than a voluntary muscle. An involuntary muscle is much more disposed to contract than a voluntary muscle. In other words, that muscle is most disposed to contract which is least supplied with nervous influence; and hence it follows, or appears to follow, that nervous influence is not necessary to contraction.

An involuntary muscle has far fewer blood-vessels than a voluntary muscle, and hence blood does not seem to be necessary to contraction, for the muscle which receives the least quantity of blood is most disposed to contract. The muscles of a hibernating animal are more prone to contract during the hibernating than during the active state, and yet the circulation at this time is so low as to be barely consistent with life. The muscles of a reptile are more prone to contract than the muscles of a mammal, and yet these muscles are distinguished chiefly by their paleness, that is, by their want of blood. *Rigor mortis* occurs when the blood is stagnant and dead, and passes off repeatedly (as has been shown by the experiments of M. Brown-Séquard) when new blood is injected. All these facts are opposed to the idea that the blood is necessary to contraction.

Some of the investigations of MM. Dubois Reymond and Matteucci are equally opposed to the idea that muscular contraction is stimulated by electricity. These investigations show:—That there is a current of natural electricity in a muscle when at *rest*. That the evidences of this current *disappear* during contraction. That contraction is immediately provoked by the passage of a current of artificial electricity which opposes and neutralizes the natural current; but that contraction is not provoked by the artificial current, when it coincides with and intensifies the natural current, until the circuit is broken and the former suspended.

Nor is it by any means certain that muscular contraction is stimu-

lated by contact. Instead of exciting the stomach to contract, the food accumulates, and the stomach expands, until the appetite is satisfied, and contraction happens when the stimulus connected with the molecular changes of digestion is at an end. Instead of exciting the uterus to contract, the germ increases in size, and the womb expands proportionately, and contraction happens when (apparently) the stimulus of increasing growth is at an end. It is not even certain that a needle stimulates contraction. The muscle does not always contract under these circumstances; and when it does, the contraction *may* possibly be due to the discharge of electricity previously present in the muscle. The experiments of M. Dubois Reymond prove the existence of such a discharge; and the analogy between the structure of muscle and of the electrical organ of the torpedo, and between the circumstances attending the production of contraction on the one hand and of discharge on the other, are in favour of this supposition. The facts, moreover, which have just been mentioned respecting the action of nervous influence, blood, and electricity, are opposed to the idea that the contraction is stimulated by the needle.

It is the same with regard to other agencies. It is cold (which is the privation of heat), and not heat, which favours contraction. It is darkness, and not light, which favours contraction in the irritable cushions of the sensitive plant; and it is the same in the iris, for it is more easy to suppose that the iris expands under the stimulus of light, and that the pupil is closed in this manner, than that it is drawn out by the contraction of sphincter fibres, which have no existence. This explanation is supported by the authority of Bichât; it equally accounts for the phenomena; and it harmonises with the known influence of light upon the sensitive plant. Again, it is carbonic acid, and not oxygen, which favours contraction in the muscular fibres of the air-passages. Oxygen, indeed, seems to provoke the very opposite of contraction, for under its influence the air-passages dilate and fill with air. Again, other non-stimulating agencies, such as opium or strychnia, favour contraction; and other stimulating agencies, such as alcohol or æther, oppose contraction.

It appears, therefore, that muscular contraction, as manifested in ordinary muscle, is due to the withdrawal of the vital or physical stimulation which was previously present in the muscle, and not to the impartation of any new stimulation. It further appears that there is no reason why this contraction may not be due to common molecular attraction—that is, to the law of gravitation. It is, indeed, quite possible that the *semi-gaseous* constitution of the muscle may allow the particles of the muscle to recede or approach to a much greater degree under the presence or absence of heat or any other stimulus, than is allowed by the physical constitution of a metal or any fixed solid. It is quite possible that this should be the case, for so unstable are the affinities of the muscular particles, that (for the most part) these affinities cease to operate immediately after

death, and the particles become resolved into their constituent gases.

A similar conclusion arises from a consideration of muscular contraction, as manifested in the coats of vessels. "Joy flushes the skin and fear blanches it; in other words, the superficial capillaries expand when the nervous energy is exuberant, and shrink when it is deficient. When the blood is rich and stimulating, as in plethora, the vessels are red and full; when it is poor and watery, as in anæmia, they are shrunk and empty." When the hand is held to the fire, it becomes flushed; when exposed to cold it becomes pale. These phenomena appear to be utterly inconsistent with the idea that the muscular contraction of the vessel is caused by nervous influence, by blood, or by heat; and other phenomena are not less so. Arguing from the remarkable expansion which is caused by heat in the dartos and in the ordinary subcutaneous cellular web, it is also supposed that the heat and other stimuli acting upon the vessels must cause a greater degree of expansion in the coats of the vessel (which contain a good deal of cellular tissue) than in the fluid contents of the vessel (which consist chiefly of water); that vacua must thus be left between the coats and the contents; and that movements must result from the blood passing to fill these vacua, which movements are altogether independent of the heart. If, therefore, the hand be held to the fire, the vacua thus resulting from the excess of expansion in the vessels must necessitate a flow of blood to the part.

The muscular contraction of the heart is equally inexplicable on the supposition that it is the result of stimulation.

"The fact that the heart remains distended with blood during a full half of the time occupied in its rhythm, is a strong argument that the blood does not excite the ventricular systole. The histories of plethora and anæmia are to the same effect. In plethora the pulse is full and slow; in anæmia, empty and quick. In the one case, the heart fills to distension with rich blood, and the systole is deferred; in the other case, the heart takes in a small quantity of poor, unstimulating blood, and expels it immediately. The facts are the very opposites of what they ought to be if the blood excited contraction, for then there should be a small quick pulse in plethora, and a full slow pulse in anæmia. But they are just what they ought to be, if the blood provokes the heart to dilatation by its stimulant properties, for then the heart ought to dilate most, and the dilatation to continue longest, when the blood is rich and warm, as in plethora."—P. 33.

Upon attending more particularly to the phenomena of the heart's action, it appears still more improbable that the ventricular systole is caused by stimulation of any kind—and of the blood particularly. At the systole the oxygenated arterial blood rushes through the coronary arteries into the coats of the heart; there it remains until it has given up its arterial and stimulating properties; and then the systole returns. This is the simple fact. The seeming probability is, therefore, that the diastole is stimulated by the blood, and not the systole; and this probability is increased by the circumstance that it affords a clue to the rhythm of the heart.

Thus, according to the author, contraction of ordinary muscles, of the coats of vessels, and of the heart, is everywhere manifested under the same circumstances, and this contraction is due to the unresisted molecular attraction of the muscle upon the suspension of that stimulation which is synonymous with life. In this way muscular contraction, instead of being a phenomenon peculiar to vitality, is an effect of the grand law of gravitation. In this way, the three grand and hitherto inexplicable phenomena of physiology—muscular contraction, the movement of the blood in vessels, independently of the heart and the action of the heart—receive a single and physical explanation.

The pathology of the disorders in which muscular contraction is in excess—"epilepsy and other affections of the nervous system which are marked by tremor, convulsion, or spasm," admits of being explained in far fewer words than the physiological premises, though the statement of all the facts upon which the pathology is based occupies a much wider space in the volume itself. The topics successively considered are epilepsy, affections allied to epilepsy, periodicity, and treatment.

In epilepsy, the condition of the circulation is habitually one of great depression. The plethora of the butcher is never met with, and any vascular fulness, if such exists, is mere venous congestion. This depression is aggravated before the fit; and during the fit the condition is either one of asphyxia or syncope. If inflammation, or true fever, chance to be developed, so surely are the convulsions of epilepsy banished for the time. These conclusions are warranted by all the facts of the case. With this condition of the circulation an active condition of the nervous system is incompatible, and this is in accordance with the actual symptoms. Sense and intellect are completely obliterated during the fit, and at all times they are under a cloud, or if the torpor is occasionally broken by maniacal or other excitement, the patient is for the time relieved from his fits. The condition of the muscles is always wanting in tone. The several causes inducing the fits are always exhausting, and not exciting, in their character. In a word, there is every reason to believe that the muscles of the epileptic contract excessively (as might be expected from the premises) because they are less stimulated than they ought to be, and not for a contrary reason.

In affections allied to epilepsy, whether these be marked by tremor, convulsion, or spasm, the same conclusions are arrived at. The condition of the circulation during the paroxysm is still one of asphyxia or syncope, or one tending to asphyxia or syncope. Inflammation and true fever are utterly uncongenial with any form of tremor, convulsion, or spasm. Thus, rigor precedes fever, and again succeeds it as subsultus, but it never accompanies fever. Convulsion often takes the place of rigor, or subsultus, but it never occurs in the intermediate hot stage. The spasm of whooping-cough disappears if pneumonia or bronchitis are developed, and returns again

when the inflammation is over. In every instance the rule is that the muscular disturbance is coincident with the opposite of vascular excitement—asphyxia, or syncope, or a condition approaching thereunto.

As in epilepsy this condition necessitates a corresponding inactivity in the functions of the nervous system, and this necessity is fully corroborated both by the symptoms during life and the appearances after death. If there has been inflammation of the brain or spinal cord, the tremor, convulsion, or spasm, are found to be before or after, but never during this inflammation. The condition of the muscles is also wanting in tone, and the causes inducing the maladies are never of an exciting character.

Thus, in affections allied to epilepsy, as in epilepsy itself, and in ordinary muscular contraction, the muscles contract, not because they are stimulated, but because they are not stimulated.

The phenomena of periodicity are also thought to furnish evidence of the same kind. The plant exhibits plainer and more numerous evidences of periodicity than the animal, and it does this, it is argued, because it has less of that innate life which enables the animal to be partially independent of the vivifying influences of the heavenly bodies. If man exhibits more evidences of periodicity than he ought to do, it follows therefore that he has lost some of that innate life which is the badge of distinction between him and the plant; and hence the periodicity of epilepsy or of any cognate disorder, is merely a proof that the epileptic or his congener is less vitalized—less stimulated than he ought to be.

If, then, these diseases depend upon the want of that stimulation which naturally belongs to the muscles, it follows as a necessary consequence—what indeed may almost be said to have been proved by experience—that bleeding, purging, and all lowering measures are not calculated to do good, and that the only hopes of benefit must be placed, not upon tonics merely, but upon stimulants. This is the conclusion which is verified by the experience of the author, whose practical remarks, as well indeed as the entire work, we cordially recommend to the attentive consideration of our readers.

The work by Mr Garner, entitled “*Eutherapeia*,” is a popular resumé of the general principles of medicine, and its circulation, in this age of charlatanism, is very likely to be beneficial. We were induced to consider it here from observing on the title page that it contained researches in the nervous system. These researches consist in the personal examination, and representation in outline, of the nervous system of several of the inferior animals, but we have not discovered in the text any great novelty either in fact or generalisation.

With regard to the able essay of Dr Peddie on *Delirium Tremens*,

it originally appeared in our pages, and our readers have been enabled to judge of it for themselves. Its subject-matter is of the highest practical importance, and the additional cases and remarks which the author has introduced into his separate publication add considerably to its original value.

On Stricture of the Urethra and Fistula in Perineo. By JAMES SYME, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh, etc., etc., etc. Second Edition. 1855.

IN the former edition of this work, published in November 1849, the author communicated, as he now tells us, his "early and, of course, crude observations," on the treatment of certain forms of stricture of the urethra and fistula in perineo, by a new method which had originated with himself, viz., division of the affected part of the urethra by external incision upon an instrument previously carried through it. It was shown that the operation had afforded relief in several severe cases of stricture, which had resisted the ordinary means of treatment, and that it had hitherto proved almost free from danger in the hands of its inventor. But that the cure would prove permanent was left to be inferred from one single case, viz., that which led to the adoption of this plan of treatment: in all the others spoken of as cured, the operation had been so recently performed as to forbid any deductions from them on this point. And although the first case was a very striking example both of the failure of the means previously in use, and of the success of the new method, after the employment of which the patient continued to enjoy "the most perfect health," without use of the bougie, after the lapse of "several years;" yet, one such case was no sufficient guarantee for the general permanence of the cure, and it was felt that further experience of this operation was required to enable Mr Syme to establish the high claims he had asserted for it.

He has, however, made good use of the extensive opportunities afforded by his practice during the five years that have since elapsed, and the facts which he now places before us are such as to give a totally different character to the work, which, in the present edition, entirely re-written with the exception of the preface, is no longer a mere attempt to "explain and recommend" a particular method of treatment, but assumes the form of a systematic practical treatise on stricture of the urethra.

After giving, in his usual powerful style, a vivid description of "the evils originating from this fruitful source of misery," Mr Syme proceeds to divide strictures with reference to their symptoms and treatment, into—1. Imaginary; 2. Slight; 3. Confirmed; 4. Irritable; and 5. Contractile:—

"Imaginary strictures constitute a large proportion of the cases that occur in practice. They generally depend upon an erroneous idea of the patient, sug-

gested by some irritation of the urinary organs, or debility in the expulsive power of the bladder, and encouraged either through unskilfulness of the surgeon, who attributes the pain and difficulty resulting from his awkward use of instruments to contraction of the passage, or through the culpable delusions of rapacious quacks whose appropriate field is the remedy of diseases that have no real existence. Such cases explain the good effects attributed to various modes of treatment, which, if the stricture were real, would prove useless, impracticable, or injurious."—P. 10.

"It is a remarkable fact that strictures frequently pass into the most confirmed state without the patient being aware of their existence;" and "not unfrequently the first intimation of there being something wrong is afforded by the formation of a perineal abscess," regarding the pathology of which the author expresses the opinions he has been long known to entertain, and which he supports, as in the former edition, by arguments which appear to us incontrovertible.

"The truth seems to be, that the irritation of the stricture, or the means employed for its remedy, occasions inflammation in the textures adjacent to the urethra, which, sooner or later leading to suppuration, gives rise to an abscess separated from the canal of the urethra merely by its lining membrane, but thickly covered externally by the fascia and integuments of the perineum. In obedience to the law of progressive absorption induced by the pressure of purulent matter, spontaneous evacuation is nevertheless much more apt to take place outwardly than inwardly, although the resistance of the fascia tends to render the passage tortuous and directed towards the hip, scrotum, or anus, instead of the nearest point, which of course coincides with the raphe. In no long time after the matter is thus discharged, or through an incision anticipating this event, ulcerative absorption establishes an opening in the thin denuded portion of mucous membrane that constituted the urethra wall of the abscess, and then the urine escaping in more or less quantity, renders the fistula in perineo complete."—P. 13.

Slight strictures, and the majority of the confirmed class, yield to dilatation, which may be effected "through the use of various means, such as retaining a succession of catheters in the bladder, or introducing bougies frequently for lengthened periods. There is, however, no occasion to place the patient under any such restraint, since the object in view may be more simply, safely, and effectually accomplished by passing bougies with intervals of three or four days, withdrawing them immediately after their introduction." We rejoice in the reiteration of this doctrine, which the author has taught for many years, and are only astonished that the practice of surgeons does not more generally testify to its truth. Nothing can be more reasonable than the momentary introduction of instruments, when the mode of action of dilatation is rightly understood; and this is placed by our author in its true light, not as a mere mechanical process for stretching the contracted part—like "the finger of a glove," but as a means of exciting just sufficient action to cause the absorption of the material effused into the urethral coat:—

"Any sort of stretching which exceeds the degree requisite for this purpose, so far from doing good, will, therefore, probably increase the evil by reinducing the irritation productive of contraction; and mere distension of the canal,

by the permanent retention of catheters, must not be confounded with the real widening which results from a restoration of the urethral coats to their natural state of capacity and tenuity.

"To effect dilatation with this view, the best means are unquestionably metallic bougies, and those made of Berlin silver seem decidedly preferable to any other, as they take a fine polish, are not liable to rust, and being hollow, are guided more lightly than the plated steel instruments used in London, while their moderate expense does not place them beyond the reach of ordinary employment."—Pp. 14, 15.

For our own part, we could never see why silver catheters were preferable to bougies for the smallest sizes; on the contrary, it is in small instruments that the greater firmness of bougies is of most importance, while at the same time, it may be remarked, that bougies, besides being cheaper, are more durable, and, what is of no small importance, more easily kept clean than catheters.

"Pain and bleeding, when bougies are passed, are certain indications of the procedure being faulty. The bougie should be rather suspended than held between the tips of the fore and middle fingers, and gently urged on by the thumb, while the fingers of the left hand rest on the perineum to assist in determining the proper course of the instrument. Stretching of the penis and forcible clutching of the bougie are sure signs of awkwardness and incompetency in this department of surgery.

"It is unnecessary to say anything as to the preposterous plan of taking casts of the stricture by pressing upon it the extremity of a soft plaster bougie, since any one at all conversant with the subject must be satisfied that such a procedure can produce no result better than deception of either the practitioner or the patient. . . . However often the introduction of instruments may be required, the greatest care should be taken to avoid all attempts to gain an advance by force in opposition to unfavourable circumstances.

"Yet however carefully and skilfully this process may be conducted, disagreeable consequences, such as rigors, are apt to result from it, and the risk of such occurrences will of course be greater, when there is any impropriety on the part of either the patient or the surgeon. . . . But whatever may be the difficulties and danger of the simple dilating process, they are trivial when compared with those attending the use of caustic; which, even if it should happen to be correctly applied to the seat of contraction, must expose the patient to the risk, not only of immediate irritation, but also of subsequent inconvenience from cicatrization of the ulcerated part."—Pp. 15-19.

With regard to the treatment by internal incision, Mr Syme states, that besides being uncertain and dangerous in the first instance, "so far from lessening the disposition to contract, the internal incision rather increases this morbid tendency, so as seriously to aggravate, instead of alleviating the disease;" and this he infers partly from his own experience, but more from extensive opportunities which he appears to have had of ascertaining the results of that of other surgeons. He comments, with deserved severity, on the strange proposal of M. Reybard to "rip open the canal with a certainty no less unerring than relentless," by a lancet blade pointing forwards instead of backwards, as in Mr Stafford's instrument, and contained in a sheath so large as to be capable of introduction only into strictures such as would be readily amenable to dilatation;

and he gives, in an appendix, a reclamation addressed by himself to the Imperial Academy of Medicine of Paris, who in 1852 awarded the Argenteuil prize to M. Reybard, on the recommendation of a committee, whose Report mentioned Mr Syme's operation as one not to be compared with that of M. Reybard.

The Boutonnière operation is based on the supposition of the existence of impermeable strictures, in which Mr Syme does not believe.

But there remain certain forms of stricture that will not yield to dilatation, and require some other treatment, and these

"May be distinguished as unyielding, irritable, and contractile. In the unyielding form dilatation, though it may be carried on to some extent, is sooner or later arrested by resistance of the tough texture at the seat of contraction; and if attempts are made to remove this obstacle by forcible distension, the most serious consequences are apt to ensue. In the irritable condition, while all the symptoms of stricture are presented in an extreme degree of severity, the gentlest introduction of instruments is sure to produce great aggravation, not only at the time, but for days afterwards; so that the patient is distracted between desire to obtain relief, and dread of the effects resulting from means employed with this view. In the contractile, or spasmodic stricture, as it is usually called, no difficulty is experienced in dilating the canal to its proper capacity; but unhappily with little benefit—the patient still making water laboriously, painfully, and frequently—by drops, or in a dribbling stream, which is liable to complete obstruction, through the influence of any local or constitutional disturbance affecting the urinary organs.

"For the treatment of stricture in these three forms, whether existing singly or combined together, the means of remedy hitherto employed have proved quite unavailing; and the patients thus afflicted are deserving to be regarded as a great discredit to surgery. If poor, they frequent hospitals until dismissed by desire from despair of relief, or are declared incurable, or fall victims to practice more zealous than discreet. If rich, they run the gauntlet of European skill—having bougies introduced—caustic applied—internal incisions inflicted; and so on, until with broken health, disappointed hopes, and perhaps empty purses, they retire in dreary seclusion from society to carry on a dangerous and ineffectual system of palliation through means of the various apparatus collected in the course of their wanderings. That this is not an imaginary or overdrawn picture, must be admitted by every practitioner who possesses any considerable field of observation; and will also appear from the cases to be found in any large hospital."—Pp. 25, 26.

In these cases the method devised by Mr Syme is to be employed:—

"The peculiar and fundamental principle of which is to divide the stricture completely by an external incision—not at random, as by the procedure formerly in use—but with certainty and accuracy, through the assistance of a grooved director passed fairly through the contracted part of the canal. This obvious distinction has been strangely overlooked, and the operation which I have proposed has been held responsible for the disastrous effects of the very methods which it was intended to supersede, so that individuals, societies, and journals, have paraded the deadly statistics of groping in the perineum without a guide, or opening the urethra behind the stricture, as arguments against adopting a proposal which afforded perfect security from the dangers of these proceedings. It is much to be regretted, that through such misconceptions, the relief placed within their reach has been withheld from many unfortunate sufferers; and I venture to express the hope, that any impression so produced, will no longer be permitted to oppose the progress of improvement in this department of surgery.

"Cutting into the perineum without the assistance of a precise guide, exposes to the serious danger of opening the urethra on the wrong side of the stricture, of breaking through the deep fascia, and of wounding the artery of the bulb, so as to incur the risk of urinary extravasation and hæmorrhage, while pressure being the only means available to suppress the latter, must greatly tend to promote the former evil. It is therefore no wonder that this procedure has been looked upon as a forlorn hope, warrantable only in cases of impermeable stricture. But while admitting, as I have already done, that in some rare cases the urethra may be actually obliterated, I maintain that no *stricture* is impermeable, and that if a drop of urine is able to escape, a director of sufficiently small size may be introduced; and in support of this position I appeal to the fact, that although patients alleged to labour under impermeable contractions, have come to me for relief from the most distant parts of Scotland, England, and Ireland, from the Colonies, and from America, I have never, either publicly or privately, been unable to pass an instrument since I became satisfied that there was no true impermeability.

"Some advocates of impermeability, indeed, allege that those who deny the existence of this condition effect a passage by force; but as the stricture is tougher than the sound urethra, and as, therefore, any passage accomplished by force must necessarily be a false one, which would aggravate the patient's case instead of remedying it, the satisfactory result of treatment affords a most complete refutation of such statements. As already said, it is far from my intention to allege, that the introduction of instruments, may always be accomplished with ease. In general, I have succeeded at the first attempt; but in many cases, have had to wait days, or even weeks, before the passage could be hit. Indeed, on three occasions—one in private and two in public—I found it necessary to open the urethra anteriorly to the stricture, so as to obtain the assistance of a finger placed in the canal, to guide the point of the instrument."—Pp. 33-36.

One of the two cases that occurred in public is given in detail, as Case XIV., where Mr Syme, after mentioning that he had been for two months unable to pass a bougie through a stricture reported impermeable, gives the following account of the plan he adopted:—

"As I had little doubt, however, that the difficulty arose from the form of the urethra where the false passage entered it, rather than from mere tightness of the stricture, I resolved to lay open the urethra on a director in front of the stricture, and then endeavour to guide the director through the stricture by means of my forefinger introduced into the wound. For I had found, in a former case, that the tip of the index finger being inserted into the part of the urethra in front of the stricture as into a thimble, afforded the means of guiding on an instrument through the stricture with unexpected facility. Accordingly, on the 31st August, the patient being under chloroform, I measured with a large bougie the distance of the stricture from the external orifice, and having introduced a director rather larger than No. 1 bougie for the same distance into the urethra, I pushed it in as far as it would go, and being thus sure that the end of the director was in the false passage, I made an incision in the middle line of the perineum, and laid open on the director the contiguous parts of the urethra and false passage. Having then introduced my finger into the wound, I succeeded in guiding the director through the stricture, and divided it in the usual manner by running the knife along the groove. I had now no difficulty in passing a full-sized catheter into the bladder."—Pp. 94-96.

This case appears to throw an entirely new light upon the subject of relieving retention of urine by operation when the catheter cannot be passed in the ordinary way. And although Mr Syme tells us that he has never yet found puncture of the bladder necessary for retention of urine depending upon stricture, he must admit that if a case like the above had come to him with urgent retention, before he had hit

upon the expedient there made use of, he must have had recourse to puncture of the bladder. For though the difficulty in that case arose from complication, with a false passage, rather than from tightness of the stricture, yet it was not on that account the less insuperable in the ordinary way. The great obstacle to the management of strictures at the bulb unquestionably is the difficulty of guiding small instruments with precision in that part of the canal, and the means now furnished by Mr Syme for facilitating this process, appears to us a most valuable addition to surgery; and we need scarcely point out the contrast between the results of this method, and those of "groping in the perineum without a guide."

"It is the preposterous system of 'tunnelling,' as it has been called—or attempting, by long continued pressure, to pass large bougies or catheters through a tight contraction—that gives rise to the greatest difficulty in introducing instruments, since there is thus formed a cul de sac beyond the stricture, but nearly in the proper direction of the canal, so that if small instruments are afterwards used, false passages are apt to be formed at the bottom, while the true one exists at the side of the excavation."—Pp. 36, 37.

For a description of the mode of performing the operation of division of a stricture by external incision, we must refer the reader to the work, where he will find a representation of Mr Syme's new staff, which, by giving accurate information as to the seat of the stricture, "has wonderfully facilitated the process, and also rendered it much more sure of being effectual."

"The only sources of danger that can be attributed to the operation, are bleeding and extravasation of urine; and in order to estimate the importance due to them, it is necessary that the true position of strictures should be ascertained." Here Mr Syme makes the important announcement that "the seat of contraction may be positively limited to that portion of the urethra which extends from the bulb to the orifice," and "the ground upon which he makes this statement is that in all his experience (viz., in 108 cases), he never found it necessary to cut farther back than the bulbous portion, for the conveyance of a full-sized instrument into the bladder," although in many of the cases the stricture was previously believed to be in the membranous position.

"It being then assumed as a fundamental principle, that incisions for the remedy of stricture do not require to be carried farther back than the bulb of the urethra, it follows that there is no occasion for cutting through the deep fascia of the perineum or extension of the triangular ligament, and consequently, that if the urine should become extravasated, its diffusion must be limited to the scrotum and other external parts. But if a large catheter be retained in the bladder, there is no risk of any such occurrence, and therefore this source of danger may be put entirely aside."—Pp. 42, 43.

Serious hæmorrhage can proceed only from the arteries of the bulb, and a figure is given showing the lateral position of these vessels, and making it evident that they "cannot be wounded by any incision in the middle line."

"By these considerations in regard to the security from extravasations of urine and hemorrhage," says the author, "I was originally led to expect that the operation would prove nearly, if not entirely, free from danger; and this anticipation has not been disappointed. Having declined no case presented for treatment, and operated at all ages, from 77 downwards, as well as under every variety of complication from long existence, alleged impermeability, and the false passages of previous mismanagement, I have now performed the operation 108 times, with only 2 fatal results that can be ascribed to it."—Pp. 44, 45.

To some persons these exceptions will probably have the effect of proving the rule, for though, for our own part, we never doubted Mr Syme's good faith, there has been in some quarters a wonderful unwillingness to admit the truth of his statements regarding the safety of the operation. Both the deaths are thought to have occurred from pyæmia; but one of the patients appears to have been in a state hardly suitable for operative interference of any kind. The other case is by no means a clear one; and we cannot but regret exceedingly that permission was not obtained to examine the body, considering that the result was attributed to a fall upon the knee and head in a fainting fit, and that "symptoms of cerebral excitement presented themselves and assumed an alarming character," so late as four weeks after the operation. We cannot but think some cerebral lesion more likely to have been the cause of death than pyæmia. Be this as it may, the mortality from this operation has certainly been very slight in Mr Syme's hands, and this we confess we have no difficulty in understanding; no surgeon would speak of the extraction of a calculus from the spongy part of the urethra by an incision in the middle line as a dangerous operation, and admitting, as we must, that the disease is limited to that part of the canal, why should a medium incision through a stricture be fraught with greater danger, provided always that the operator be skilful and divide the indurated textures upon a proper guide?

One instance of hemorrhage occurred "in the 98th case," but this was under very "peculiar circumstances," for which we must refer the reader to the work; and Mr Syme "ventures to express the hope, that as the appearance of one swallow is admitted not to make a summer, one instance of bleeding in a hundred operations will not be regarded as good ground for apprehending hemorrhage." So far from there being any risk of the wound remaining fistulous, it may be stated that of all the means hitherto devised for the remedy of fistula in perineo, a free division of the contracted part of the canal is the most effectual, as might be expected from the state of matters concerned."

"While the operation may thus be regarded as exempt from the risk of hemorrhage, urinary extravasations, and fistulous effects, it is here proper to remark, that there are not unfrequently symptoms of an alarming character, and which under other circumstances would justly excite the most serious apprehension. These are rigors occurring alone or associated with bilious vomiting, suppression of urine, or delirium. They generally present themselves during the first two days, and are seldom met with beyond the third. In the great majority of cases they pass off in a few hours without the slightest disagreeable

consequences ; but on rare occasions, just as in the feverish attacks attending the introduction of a bougie, leave some local derangement, such as a swelled testicle or abscess of the scrotum. There is no treatment required on the occasion of these attacks, and if the surgeon has had sufficient experience to feel confident that there is no real danger, he will be able to administer the only practicable relief by assuring the patient and his friends that the state of nervous irritation will quickly subside."—Pp. 52, 53.

With regard to the effects of the operation, we must again leave the author to speak for himself:—

"The good effects of the operation may be divided into immediate and remote. The first extending to a period of some months' duration, and the latter existing through the remainder of the patient's life. Now, whatever may have been the condition of the stricture, whether irritable, contractile, or obstinate, and whatever may have been the severity or duration of the symptoms, complete relief in the first instance has invariably resulted from every operation that I have performed. Instead of the slow and frequently interrupted progress of improvement which usually attends the use of bougies, all the distressing symptoms quickly disappear, not unfrequently without an hour's delay, and at all events in the course of a few days. If the urine has been thick and loaded with mucus, it becomes clear and limpid. The frequent and distressing calls to micturition are succeeded by a state of blissful repose. The most obstinate and unyielding contraction admits with ease instruments of the largest size ; and the most irritable stricture which could not previously be touched without the production of spasms and ague, permits the passage of bougies without the slightest uneasiness either immediate or consecutive. The urine, instead of being passed by drops or escaping incontinently, flows freely in a copious stream, and the patient, however much worn down by suffering, speedily regaining his appetite and strength, appears to his friends like a new edition of himself. The relief thus obtained having proved no less permanent than complete in many cases of the most hopeless character, I think the fair presumption is, that when relapses do take place, there must be some reason for such exceptions from the general rule, and that the duty of practitioners consequently is, not to search out these failures as objections to a proposal for the remedy of contractions confessedly incurable through other means, but rather to study the subject in a candid spirit of inquiry, with the view of ascertaining the circumstances essential to success."—Pp. 53, 54.

In case XVI. will be found a description of the post-mortem appearances of the urethra of a patient who died of aneurism, having been operated on two years previously for a tight stricture at the bulb, accompanied with induration of the perineum:—

"A white depressed line of cicatrix, on the inner surface of the urethra, extending, in the mesial line, half an inch forward from the extremity of the bulb, corresponded exactly with the line of cicatrix in the integuments and intermediate textures. The urethra in this part was rather wider than natural, from having a slight funnel-shaped depression on the lower surface, and, before being opened, easily admitted a No. 12 bougie. In other respects the coats of the urethra presented a perfectly natural aspect, and the spongy tissue of the bulb, as well as the neighbouring parts, were free from induration.

"No one," observes Mr Syme, "looking at the urethra, which remained thus sound and ample at the part where it had been so long and tightly contracted, although two years had elapsed since the operation without any means being used to prevent relapse, could entertain a reasonable doubt as to the recovery proving permanent, however long the patient might have lived ; and such a result should stimulate our exertions to discover, so that they may be avoided, the errors of performance to which any recurrence of the symptoms

hat may have happened ought to be attributed, rather than to the principle of the operation."

Most of the cases given in the work, while examples of the different forms assumed by the disease, tend to prove the permanence of the cure; and with regard to this point we would particularly refer the reader to the first case, alluded to in an early part of this notice as having appeared in the first edition. "Thirteen years have now elapsed," yet the patient, who "has never required the bougie, in every respect enjoys the most perfect health."

In proceeding to consider the causes of the relapse that has occurred in some cases, Mr Syme complains that he has been left almost alone in the investigation of this subject, and that "little, if any, additional information has proceeded from other sources" during the last five years.

"That the rapacious quacks who have so long made stricture of the urethra profitable source of gain should oppose the introduction of an efficient method of rescuing the victims of their extortion from the toils of an incurable malady, is not at all surprising; and that members of the profession who have not acquired the art of passing instruments safely through urethral obstructions should view with coldness a proposal essentially requiring for its successful application the utmost familiarity with operative manipulation, is quite consistent with what was to be expected. But that the leading members of the profession should have displayed such apathy in regard to a matter so deeply affecting the interest of their patients, could hardly have been anticipated. . . . The most obvious and certain cause of relapses would appear to be adhesion by the first intention between the edges of the incision made through the strictured art, which must restore the state of matters that existed previously to the operation."—Pp. 55, 56.

And that this cause does sometimes come into operation will be strikingly illustrated by a case of stricture at the very orifice of the urethra, in which the condition of the part could be accurately observed.

"If the stricture is tight, the presence of a full-sized catheter will go far to prevent approximation of the cut edges; but if it be of that dilatable kind which requires the operation merely to remedy a spasmodic or resilient tendency, the risk of reunion will be considerably greater; and it is accordingly in such cases that I have found the disease most apt to return. At the expense of somewhat prolonging the process of recovery, it will therefore be prudent, with the view of promoting its permanency, to allow the urine in the first instance to pass through the wound, which may be easily done by introducing the finger occasionally, so as to feel the surface of the catheter."—Pp. 58, 59.

"Another cause of relapse," Mr Syme has "no doubt, is making too limited an incision through the stricture; since the canal is frequently, if not always, contracted on each side of it into a conical form—so that if the whole of this part is not divided, the portion remaining may reproduce the symptoms." "As in all wounds and injuries of the urethra contraction of the canal is apt to occur, if a full-sized instrument is not occasionally introduced during the process of healing, this precaution should not be omitted," particularly during the early period of the treatment after the operation.

"Independently of these causes, the tendency to contraction may be maintained by a source of irritation in some other part, as by a stone in the bladder," which is well known to exercise this influence; and,

"If the irritation of a stone in the bladder be sufficient to maintain the contractile disposition, it seems reasonable to conclude that an irritating disease in some other portion of the urinary organs, or even in those of the digestive function which are so intimately associated with them, may produce a similar effect; and, therefore, while all such adverse influences should be carefully recognised, and, if they permit, removed, it is obvious that cases may occur in which permanent relief cannot be afforded, in consequence of the seat or connection of the opposing irritation. But surely the possible existence of these exceptional conditions would not constitute a good ground for rejecting a mode of treatment, which, at the worst, can only fail to do good, and in the great majority of cases, proves an effectual remedy for one of the most distressing, and, under all other means of relief, the most hopeless, diseases to which the human body is liable."—Pp. 62, 63.

In taking a general view of this work, which we believe will ever be standard on the subject, we are forcibly struck with the simplicity which it introduces into the treatment of stricture. Instead of bougies kept in from half an hour to several hours, according to the condition of the patient, a succession of catheters retained in the bladder, a series of tubes passed over one another, and various other dilating means, caustics of different kinds, internal incision by different methods, the Boutonnière, etc., among which the surgeon might well be at a loss to choose, Mr Syme restricts the treatment to dilatation by his gentle method, and, when this fails, division of the contracted part by external incision, which we agree with him in regarding as simple, safe, and effectual.

We cannot take leave of our author without expressing our admiration of the masterly manner in which he has handled the subject of stricture of the urethra, and our conviction, that this book will add as much as any of his previous works to his deservedly high reputation.

Part Third.

PERISCOPE.

MIDWIFERY.

DR MASON'S CASE OF RUPTURE OF THE UTERUS DURING LABOUR, IN WHICH GASTROTOMY WAS SUCCESSFULLY PERFORMED.

DR MASON read before the Philadelphia College of Physicians (Sept. 6, 1854) the following interesting account of a case of rupture of the uterus, in which the operation of gastrotomy was successfully performed by Dr John Neill:—

Monday, 24th July, called to Mrs John M'Devitt, South above Twentieth Street, in labour with her sixth child; reached her at 9 P.M.; had been in active

about for about an hour; of a florid complexion, somewhat fleshy, large muscular development, with all the appearance of possessing a constitution of more than ordinary strength and vigour.

Upon examination, found the os uteri about half open, membranes presenting unbroken, the head to be felt high up above the superior strait; the pains were good, but by no means violent, with distinct remissions of five or six minutes; left her, and returning in about an hour, found the os uteri fully open, pains somewhat stronger, but still the presenting part did not descend. I now ruptured the membranes in the expectation that, as there was neither contraction nor rigidity, the head would come down into the pelvis without further delay. In this I was disappointed, and it was evident that the head had some difficulty in entering the superior strait. Still it advanced a little, and I thought I could detect the anterior fontanel looking towards the left acetabulum, giving the fourth position of Baudelocque. I was however by no means certain on this point, but resolved to wait. I made a visit in the next street, and returned to the patient in less than half an hour; the pains were now much stronger, and I thought that the head had advanced slightly. At this time Mrs M. was obliged to get up, for the purpose of relieving her bowels, and I went down stairs, still without the slightest anxiety as to the result of the labour, for the spirits were good, the countenance cheerful, the woman well formed and vigorous, and a state of active labour had not existed for more than two hours and a half. While at stool, the patient had two pains; during the latter she suddenly complained of intense agony, with a burning sensation in the right side; the woman hurried her to bed, and called me into the room; I found her on her back in great torture, which she assured me was no longer her pain of labour, but that something had gone wrong inside of her.

I examined her pulse and found it but little altered; this, added to the circumstance of there being neither vomiting nor cold clammy skin, nor anything like an approach to syncope, made me hope that matters were not so bad as I had at first apprehended; but after administering some forty drops of laudanum, using hot fomentations, and waiting for some time, finding that the uterine contractions were completely suspended, that the presenting part had receded, and that there was a sanguineous discharge, though not profuse, from the vagina, I felt convinced that the uterus was ruptured.

Before, however, proposing any operation, I called upon Dr Hollingsworth for his advice and assistance. He immediately came in the kindest manner, and, after careful investigation, the diagnosis was distinctly made out. The placenta had *not* passed into the cavity of the abdomen, for it could be distinctly felt with the cord passing from it. The head of the child could be detected through the abdominal parietes occupying the lower part of the abdomen in the right side, near the inguinal region, but no portion of it remained in the uterus.

Under these desperate circumstances, we deliberated with sad forebodings on the treatment to be adopted in order, if possible, not to lessen the little chance of life remaining. I am aware that all the best authorities recommend introducing the hand through the torn womb, into the bowels of the victim, seizing the feet, and first dragging the infant back into the womb, and from thence, *per vias naturales*, into the world. They say that it gives the child a better chance. It may be so; I am not prepared to dispute that point, for I thank God I have had no experience in such a procedure; but this I do know, that the description of the operation has always filled me with unutterable horror. Another thing, which I am bound to confess, though it may appear very unprofessional and unnatural to some, is, that I never thought of the child or its life, or anything about it, except to wish, that as it had pleased God to place it here, it would please him, in his infinite mercy, to assist me in getting it away without tearing the poor devoted woman to pieces, and entailing upon myself the terrible conviction, that I had made almost certain death a certainty.

After due consideration, however, we determined to explain the nature of the necessary operation by turning to the patient, and propose it as a dernier resort. This we did, but she absolutely refused to submit to it; and, from the hydrocephalic condition of the head, afterwards ascertained, we had reason to be thankful that she did so. At this time there was no vomiting, the expression of her countenance was good, the pulse firm, and the skin natural. The pain in the abdomen, at first very severe, had now much abated; and after administering a powerful dose of morphia we left her, determining to see her in the morning, and then be guided by circumstances.

Next morning, the 25th, when Dr Hollingsworth and myself visited her, we found her much better than we anticipated; pulse firm and strong, countenance bright, and mind unclouded. Longer to leave her undelivered was out of the question; professional duty and common humanity alike demanded that an effort, however desperate, should be made to save her. We therefore determined to propose gastrotomy, as that operation, in our opinion, afforded her the best chance. To this, encouraged by feeling better than she anticipated, she at last consented; and, after consulting Dr Neill, who undertook the performance of the operation, it was determined on.

Operation by Dr Neill.—The patient was placed upon a stout table covered with blankets, her shoulders and head supported by pillows; and as a preliminary step, about four ounces of ether were administered by inhalation. The incision was made in the linea alba, commencing about two inches below the umbilicus, and extending towards the pubis full six inches. The moment the opening was made, large quantities of mingled blood and clots escaped, the omentum seeming saturated with blood, and both the visceral and parietal peritoneum being deeply stained. A dead child's back presented, its head lying low down towards the right groin, its feet to the left. It was immediately removed, and found to be hydrocephalic; the bi-parietal diameter of the head measuring, I should suppose, six inches; the occipito frontal probably seven. Its entire weight I should judge to be not less than ten pounds.

The rent in the uterus appeared to be enormous, and perfectly uncontracted, for the operator passed both hands through it, right down into the organ, and, as it were, scooped up the placenta, with all the coagula within his reach. Upon the removal of his hands, the womb instantly contracted to about the size of a man's fist. The blood, fluid as well as coagulated, was then removed from the cavity of the abdomen as far as practicable, disturbing the viscera as little as possible. The incision was then closed by five sutures, and afterwards by long adhesive straps, leaving an opening at the lower part of the wound to favour the escape of fluids; a compress and binder completed the arrangement.

The patient's strength was less exhausted than could have been anticipated; spirits good; pulse 120, firm and equal. The time occupied by the whole operation did not, I should think, exceed five minutes. In half an hour she was placed in bed, and an enema of laudanum administered; grain doses of opium were directed to be given by the mouth every three hours, in order to keep her, if possible, in a perfect state of repose, and prevent any action of the abdominal viscera; at the same time the system was supported by nourishing fluids, beef tea, etc. Shortly after the operation, the patient began to vomit a greenish watery fluid, which continued several hours, but was checked by the exhibition of small quantities of brandy with ice; the opium treatment seemed to agree with her, for she slept, and complained of but little pain.

On the morning of the 26th, I visited her in company with Dr Hollingsworth. Found her tolerably easy; mind cheerful; tongue clean and moist; pulse 120; the abdomen was tympanitic, and very much distended; the breathing much embarrassed by the accumulation of gas. Ordered her to continue the opium pills, and to have an injection containing turpentine. On making my evening visit I learned that the bowels had been slightly opened, and that she had passed large quantities of flatus, by which the tympanitic distension of the abdomen was much lessened; breathing easy and natural;

pulse rapid and weak. Directed brandy to be continued with the opium. 27th and 28th.—Continued in much the same condition—occasionally vomiting. On the 29th, Dr Neill visited her with Dr Hollingsworth and myself. Removed the stitches from the wound, which was healthy and closing extremely well; she was now ordered milk punch *ad libitum*. At this time there was a very copious, dark, offensive discharge from the vagina, which was kept continually syringed with warm water and soap. The bowels had been moved once copiously; pulse 120; tongue moist but slightly furred. The patient looked to be hopeful and strong, that we began to feel encouraged.

On the morning of the 30th, I understood that she had passed a restless night. Looked very much worse; the lips were pale; countenance dejected; pulse, 130; vomiting of green matter without effort—in fact a regurgitation of the fluids contained in the stomach. I began to lose hope. Still her mind never wavered, day nor night, and when spoken to she replied quickly and clearly, but without anything like an unnatural elevation, a condition which I have sometimes observed in bad cases of uterine phlebitis. The discharge from the vagina was less copious and less offensive. When I saw her in the evening, she was labouring under the worst possible symptoms; so much so, indeed, that I thought it possible she might die before the morning. Her pulse was from 135 to 145, very weak; her feet and legs were cold, also the lower part of the belly; her wrist and arms to the shoulders in the same condition, and bedewed with a clammy sweat.

I confess I regarded her as moribund, and the priest in attendance told her that she was dying, and must make her peace with God; the poor woman replied that she would make her peace with God most willingly, but that the reverend father was wrong, that she was not dying yet—she did not feel like lying. And as it proved, she was right, for the next morning, the 31st, I was agreeably surprised to find that her skin had regained its natural temperature, that her strength had improved, and that she was altogether better than on the previous day. There was no pain on pressure of the abdomen, but still there was considerable tympanitis, and the pulse continued at 135.

On the 1st of August, the vomiting continued, but only occasionally. On the 2d, vomiting had ceased entirely. I watched with great anxiety for a diminution in the frequency of the pulse, as indicating some favourable change, but as yet in vain. Notwithstanding the steady pursuance of the opiate treatment, the patient's bowels, on the 1st, were largely opened, three or four times, she complained of great pain, before each evacuation; opiate enemata checked this, and on the 2d she had but one stool, perfectly natural in colour, and of the consistence ordinarily produced by a dose of castor oil.

August 3. Pulse somewhat slower, about 120; dressed the wound in the abdomen; did not think it looked quite so well; some discharge from one of the suture openings; was suffering from great uneasiness of the bowels, they having been opened several times; before each movement, considerable pain was complained of, somewhat resembling the tormina of dysentery; colour perfectly natural. In the evening there again appeared great coldness of the extremities. Ordered an enema of starch and laudanum, with hot bricks to the legs and feet, brandy and milk to be given freely.

On the 4th, found the patient warm; pulse 130; tongue foul; complaining of great pain in the bowels, which had been moved several times during the night; the breathing high and laboured. Both Dr Neill and myself thought her prospect of recovery worse than usual. Dr Hollingsworth had left town, and I was obliged to be absent from the city for some hours; Dr Neill, therefore, undertook to see her for me. In the evening I found her symptoms the same as in the morning. Ordered her a large teaspoonful of laudanum, as an injection, and desired the attendant to give her all the nourishment she could take with a continuance of the brandy and milk.

Saw her, with Dr Neill, on the morning of the 5th. Breathing decidedly improved; countenance and spirits better, though the pulse was weak and con-

tinued at 130; tongue clean; dressed the abdominal wound, which looked much healthier; though there was considerable discharge from another of the suture wounds; had passed a comfortable night, slept well, and had had no pain or trouble with her bowels; but there had been a considerable discharge from the womb, described by the nurse as being of a clear red colour, and devoid of smell. At half-past nine in the evening, saw her again. Condition unchanged; pulse the same; uterine discharge still copious; bowels opened once.

On the 6th, found her much improved; pulse 120; tongue clean; expression of face natural; heat of skin almost natural, with very little thirst; the abdominal wound nearly healed. In the evening, found her easy, but showing more weakness. This I attributed to the uterine discharge, which continued copious.

Morning of the 7th, stronger and better; pulse 110; discharge from the womb much lessened; had slept soundly all night. At ten o'clock, evening of the same day, great change had taken place; pulse 100, firm and steady; uterine discharge nearly suppressed; had taken her food regularly, and with appetite; bowels had been opened once naturally during the day.

From this time she improved so rapidly that, on the 15th, she came down stairs; on the 24th, just a month from the time of the rupture, was at the wash-tub; and on the 2d of September, I met her on the street, when she told me she felt as well as she did before the accident.—*American Journal of the Medical Sciences*, p. 278.

HECQUEREL AND BODIER ON THE TREATMENT OF VAGINITIS.

The above physicians recently made a series of experiments, in the *Hôpital de Lourcine*, on various proposed methods for the treatment of this affection. The following are the general results of their observations:—

1st. *Solution of Nitrate of Silver.*

This was used in solutions of various strengths. One was used of 100 grammes of the salt to 100 grammes of water. This was found to be a painful method of treatment, which generally caused a smart exacerbation of the complaint, and on account of the long period which necessarily intervened between each application, required a long period to effect a complete cure. It was tried in 13 cases; 6 of these were cured after an average length of treatment of 45 days, and an average of 9 applications; 4 patients left the hospital, preferring the disease to a prolonged trial of a painful remedy; and in 3 cases the treatment totally failed.

A weaker solution was also tried, viz., 16 gr. to 100 grammes of water. Nine applications of this cured a child aged 8 years in 45 days; and eleven applications produced a cure in a child, *set.* 11, in 25 days. In a woman with acute vaginitis it was tried for 37 days without any result.

2d. *Solid Nitrate of Silver.*—The application of the solid caustic to the inflamed vaginal mucus membrane was found infinitely preferable to its solutions. But this was not without its disadvantages, for its use was attended by considerable pain, which, however, was not so severe as that occasioned by the concentrated solution. The exacerbation occasioned by it was brief in its duration. Out of 21 cases 16 were cured with an average number of 6 or 7 applications; and in 5 its use was found ineffectual.

3d. *Tincture of Iodine.*—This was ascertained to be less efficacious than the nitrate of silver, in the treatment of vaginitis, although it was far less painful, could be applied more frequently, and occasioned less violent and prolonged irritation. Eleven cases of moderate intensity were treated by means of it, and of these 6 were cured in the average period of from 32 to 33 days by 11 or 12 applications. This mode of treatment is recommended by the authors as preferable to all the others in simple vaginal leucorrhœa existing without inflammation of the mucus membrane. Five cases of this sort which were thus

treated, were all cured in periods varying from 3 to 22 days, and on an average, by, from 2 to 7 applications.

4th. Concentrated Solution of Tannin.—A strong solution of tannin, in distilled water (100 grammes of each), applied directly to the inflamed vaginal mucus membrane, was found to be the best of all the modes of treatment. The solution thus obtained was a viscous liquid,¹ and its application caused no pain or uneasiness. Twenty-eight patients treated by means of it were cured in an average period of 26 days, and after 6 or 7 applications.

Messrs Becquerel and Rodier recommend this latter mode of treatment as the best for the affection, being as speedy, and less painful than that by means of solid nitras argenti.—*E' Union Médicale*, 18th January.

TWINS BORN AT AN INTERVAL OF FORTY DAYS.

A healthy primiparous woman, æt. 24, who had always menstruated regularly, gave birth, after a short and natural labour, to a completely developed, but rather puny child, which died of catarrh when it was eight days old. The placenta came away naturally, an hour after the birth of the child. Some hours afterwards she attended to household duties. The abdomen was only partially diminished in size; active foetal movements were felt by the patient; and there was no lochial discharge, no lacteal secretion, and no milk fever.

Nothing particular occurred until the fortieth day after the birth of the child, when another infant was born, which, though feeble like the first, had evidently reached the full period of its development. The lochial discharge, and secretion of milk then became established.—*Gaz. de Hôpitalux*, 5th Dec. 1854.

TOXICOLOGY.

LEAD POISONING; THE ARTIZANS WHO ARE MOST EXPOSED TO IT; AND SOME OF THE REMEDIES RECENTLY PROPOSED FOR ITS CURE.

In *Henke's Zeitschrift für die Staatsarznei Kunde*, (Hft. I. 1854), Dr Bierbaum, has recently published a lengthy paper on this subject, which contains much valuable information on the relative prevalence of this affection among certain kinds of artizans. There are two great classes of men very liable to the lead disease, viz.,

1st. Those who are engaged in obtaining the metal, and in manufacturing its preparations.

2d. Those who use lead and its compounds for industrial and artistic purposes.

Among the first class are, *a*, the miners who dig the ore, and the foundrymen who fuse it thereafter; both are very often martyrs to the affection. *b*, Also those engaged in the fabrication of leaden articles. Tanquerel des Planches, found that 406 out of 1213 cases of lead poisoning occurred in individuals thus employed. *c*, Those who work among salts of lead, as cinnabar, litharge, etc.

In the second class are found, *a*, Artists. They used to be affected more in former times than they are now, as they were then obliged to grind their own colours in a dry state, and thus they unavoidably inhaled the dust. *b*, Heraldic and decorative painters, are not very subject to the disease. Among 1213 cases, 33 were decorative painters. (Tanquerel des Planches), *c*, Varnishers of carriages and metal are very often thus affected. *d*, Manufacturers of pottery and earthenware, and porcelain, are also liable on account of the salts of lead used for the process of enamelling. *e*, Artizans in glass works are not very liable to the disease, although the oxide of lead is used to facilitate the vitrifying process. *f*, Manufacturers of cards and fancy paper are also not much affected, al-

¹ Tannin and gallic acid are peculiarly soluble in glycerine. A solution of either in this fluid would form an admirable and elegant preparation for use in such cases.—*Translator*.

though they use the preparations of lead very extensively. *g*, Typefounders, compositors, and printers, as also, *h*, tinsmiths, etc. are very little subject to it. *i*, Lapidaries are not very much affected by lead colic, for although the stones are subjected to the friction of a leaden wheel, the particles of the metal liberated thereby are not inhaled, as the stones are covered with water and emery powder during the process. Tanquerel des Planches found 35 cases among 1213 of lead disease occurring among lapidaries. *j*, Copper and bronze founders are occasionally affected on account of the lead which is mixed with these metals. *k*, Workmen in shot manufactories contract lead disease occasionally. Des Planches found 11 cases out of 1213 among those employed in such works.

CHLOROFORM AND IODIDE OF POTASSIUM FOR THE TREATMENT OF LEAD POISONING.

I. Chloroform.—This remedy is recommended by M. Aran, Physician to the *Hôpital St Antoine*, in Paris. He has just published a somewhat lengthy paper on its use, in which he gives some very interesting illustrative cases. One of these occurred in a house painter, æt. 31, who was affected with lead colic for the second time, after having followed his trade for twenty years. There was obstinate constipation, bilious vomiting, pain in the epigastrium, tormina, scanty urine, and a very slow pulse—only 48. A compress, on which nearly a drachm of chloroform had been poured, was placed on the umbilical region, and retained there for several minutes. In spite of the sensation of heat and burning occasioned, immediate relief was experienced by the patient. In addition to this a sulphureous bath was ordered; a mixture containing 30 drops of chloroform was administered; and a domestic enema was prescribed, to be followed by a small clyster, with 20 drops of the anæsthetic in it. This treatment was continued for a week with great benefit, the chloroform being given morning and evening; and 16 days after admission the patient was discharged cured. Rather an interesting incident occurred in this man's case—he swallowed one day, by mistake, nearly an ounce (30 grammes) of chloroform. Symptoms of narcotic poisoning appeared, but the patient recovered satisfactorily. Several other cases are given in which the same treatment proved very efficacious.

M. Aran says that the compress saturated with the chloroform should be covered with dry compresses, and that it should be allowed to remain in contact with the skin from 2 to 5 minutes. After the preliminary large doses the following mixture should be used, *R chloroformi* gtt. 20-50; *gum. tragacanth* 5j; *syr. simplic.* 3j; *aque* 3iij;—*misce.* Dose:—a tea-spoonful frequently. He gives the following formula for the chloroform enema:—*R chloroformi* gtt. 20-50; *gum. tragacanth* 5ij; *ov. vitelli.* j; *aque* 3iv. *misce.*

The patient should be desired to endeavour to retain this enema as long as possible.—*L'Union Méd.*, 4th, 6th, and 13th Jan. 1855.

II. Iodide of Potassium.—It has been asserted by MM. Natalis Guillot and Melsens that the administration of the iodide of potassium is the best means for ridding the system of lead and mercury, inasmuch as the insoluble compounds which the salts of these metals form with organic substances, are all soluble in the iodide of potassium, which is a salt very easily and rapidly eliminated from the animal economy. Mialhe denied these assertions, which were founded on experiments made on dogs. M. Malherbe, chief physician to the Hôtel-Dieu at Nantes, has recently tried the drug in some cases of lead-poisoning, with great benefit. He never gave more than 15 grains daily, but often much less. His experience of the remedy has not been sufficiently extensive to enable him to arrive at definite conclusions; but he thinks, from the trials he has made of it, that the following deductions are warrantable:—

1st, In lead poisoning elimination of the metal takes place naturally by means of the urinary organs, which is probably due to the influence of the alkaline chlorides contained in the animal fluids, as has been thought by M. Mialhe. But this eliminative process is not sufficiently rapid to produce a spontaneous cure of lead poisoning.

2d, This elimination of lead by the urine is rendered much more active by the use of the iodide of potassium; the excretion of the phosphates by this channel is also considerably increased; and these salts yield an insoluble residue which contains the greater part of the eliminated lead.

3d, Although lead is not naturally eliminated by the saliva, the iodide of potassium seems capable of occasioning its discharge through this medium.

Malherbe gives the following rules for the rational treatment of lead poisoning, in which he has arranged the remedies according to their relative importance, and not in the order in which they should be administered, which must necessarily vary in different cases. The indications are:—

1st, To eliminate the lead contained in the economy by means of iodide of potassium, which should be administered methodically, and should be given as long as the urine and the saliva give the reactions of the metal.

2d, To cleanse the cutaneous surface by means of sulphureous and soap and water baths; and to purify the mucous surfaces by the internal use of the preparations of sulphur and by purgatives. The latter medicines, by inducing abundant bilious evacuations, doubtless eliminate a part of the lead contained in the liver, but they cannot reach those portions of the metal which are combined with the tissues of other organs.

3d, To calm hyperæsthesia and nervous symptoms in general (as epilepsy, delirium, convulsions, coma), by narcotics, and especially by belladonna, which, in addition to its sedative properties, is possessed of a cathartic action which often renders the employment of purgatives unnecessary.

4th, To combat paralysis by means of strychnia and electricity.—*Gaz. des Hôpitaux*, 21st Dec. 1854.

DR FRAZER ON THE RECOGNITION OF VEGETABLE POISONS BY THE AID OF THE MICROSCOPE.

Considerable practical difficulties exist in the medical investigation of fatal instances of poisoning, and the detection of the deleterious substance which has been employed, even when it chances to be one of our well-known ordinary chemical agents which are most easily recognized, is a problem that requires considerable skill in the details of analytic chemistry, and often slow and tiresome operative manipulation; still, thanks to the present state of chemical knowledge, it is a question which may be solved by any one who exercises adequate care and enjoys an average extent of practical dexterity, and at the same time, with so much certainty, that its accomplishment removes the possibility of error, leaving not the shadow of a doubt on the mind as to the exact nature of the poison itself.

It is, however, a very different matter when vegetable poisons have been the source of mischief; owing their activity to varied proximate principles, many of which are as yet obscurely known and difficult to recognize, even when isolated, by the aid of chemical agents, they are almost completely beyond the reach of our recognition if absorbed into the system, and we are, under such circumstances, usually obliged to rest satisfied with whatever accidental information can be obtained from circumstantial evidence, or to confess that whilst negative results demonstrate the absence of chemical substances, our positive information extends no farther than of stating that some vegetable possessing dangerous properties has been the cause of death.

Now we meet, although not very frequently, with instances of the latter form of poisoning, in which, usually as the result of accident, some of our ordinary indigenous plants have caused deleterious, and perhaps, fatal effects; such for instance, as the *cenanthe crocata* (which seems pre-eminently to deserve the rank of the most dangerous of our native flora), or the other poisonous umbelliferæ; or of the solanaceæ, as belladonna, which appears to cause more frequent accidents in England than with us, probably from its greater abundance; or the ranunculaceæ, as aconite, etc.; and in all these, and similar poisons, our test tubes and reagents appear to be as yet almost powerless in

detecting their presence. In such cases I would propose, that, aided by the history and symptoms, an aid of which we always avail ourselves in other forms of poisoning, the microscope be employed in their investigation; and the most certain way, I believe, to accomplish this result, especially for those not very intimately acquainted with the peculiar differential characters of the plants, is to compare whatever vegetable fragments may be ejected by vomiting during life, or found in the body after death, with some recent specimens of those vegetables which are most suspected to have been the cause of the accident. I have satisfied myself in this manner that the *leaves*, especially of aconite, henbane, foxglove, belladonna, and several other of our indigenous poisonous plants, can be easily recognized, and that they present distinctive characters adequate to establish ample ground for their discrimination.

The point in such an investigation, which we require to determine in the first instance, is identically similar to the first step in deciding on the nature of a botanical specimen; if the specimen consists altogether of cellular tissue, it is to be classed as one of the "cellulares," and possibly may prove to be some of the poisonous fungi; should it, however, yield us distinct evidence of vascular tissue, thus demonstrating its more exalted place in the botanist's systematic arrangements, in that case we will have as our next duty, supposing it is a fragment of leaf which we are examining, to decide on the nature of the venation, which at once points out whether it constituted a portion of an exogenous or of an endogenous plant, the latter having the well-known parallel venation, and the former presenting an equalled distinctive reticulated arrangement. Having advanced so far, we then have four other points, at least, for aiding our further identification of its source.

1st. The presence or absence of hairs, their relative abundance on the upper or under surface of the leaf, and their shape, composition, and arrangement.

2d. The appearance of the epiderm on the upper surface of the leaf, the form of the cells of which it is composed, the existence or non-existence of stomata, and if they are present, their shape, size, and disposition in the epiderm.

3d. Similar observations on the epiderm upon the under surface of the leaf.

4th. The disposition of the parenchyma of the leaf, the development of various crystalline matter (raphides) in this tissue, and their form when they exist.

An agreement in these four points, with corresponding appearances in a recent specimen, would, I conceive, afford more than an equivalent to the degree of moral certainty which is now derived from a chemical analysis of a mineral poison, and I can readily understand, with the scientific aids now at our disposal, that just as the chemist is able to place securely in a sealed tube, and exhibit before the court sublimate of arsenic and mercury as undoubted evidences of his analytic skill, so the microscopic observer might produce, to corroborate his testimony, accurate drawings of the fragments of a poisonous plant, printed by solar light, as photographs, or more slowly obtained with the aid of a pencil and camera. I have myself made, during my investigations on this subject, a number of rough sketches in the latter manner, of the appearances presented by several of our ordinary vegetable poisons, but they are so easily identified by the method I have attempted to describe that I need not dwell further upon them.—*Dublin Hospital Gazette*.

THERAPEUTICAL NOTES.

I. GALLIC ACID IN PYROSIS.—Dr Bayes says that in pyrosis, where this disease is unaccompanied by extensive ulceration, or organic malignant disease of the stomach, or by disease of the liver, the most marked benefit will follow the use of the remedy. Gallic acid, here, not only checks the secretion with a certainty and rapidity he has never seen follow the administration of any other remedy, but it gives general tone to the stomach, increases the appetite, and (what I very little expected when I first used it) in many cases removes consti-

patation. This I can only account for on the supposition that the relaxed atonic state of the stomach which favours pyrosis is continued throughout the alimentary canal, the constipation in these cases arising from want of power in the muscular coats of the intestines to expel the feces. This want of tonicity is remedied by gallic acid.—*Association Med. Journ.* 4

II. COD-LIVER OIL WITH QUINA.—Mr Bastick gives the following account of his mode of preparing this medicine:—The oleum morrhue cum quina is simply a perfect solution of quinine in cod-liver oil. The quantity of quinine may be varied according to the wish of the prescriber, although it is generally employed in the proportion of two grains to each ounce of the oil. This preparation is best made in the following manner: The requisite quantity of disulphate of quinine is dissolved in distilled water, with the aid of a little dilute sulphuric acid. The quinine is precipitated from its solution by means of an alkaline carbonate; the precipitate is treated with boiling alcohol; the resulting alcoholic solution, after being filtered, is evaporated to dryness. The residue, which is pure quinine, is then added to the cod-liver oil, and the mixture is heated in a water-bath until solution is completely effected, which is known by the oil becoming perfectly transparent.—*Lancet.*

III. LIEBIG'S ALBUMINOUS SOUP.—This soup may justly be considered a valuable therapeutic agent; it is prepared in the following manner:—Take half a pound of fresh meat, beef or fowl; wash it; then place it in a pint of distilled water, containing four drops of hydrochloric acid and about thirty grains of common salt, and leave it to macerate during one hour. After this, turn out the whole mass and strain through a linen cloth, repeating the process for the first portions of the soup, which are never clear; when the liquid has all passed through, wash the meat with distilled water to obtain a pint of soup. A cupful of this will be a nourishing dose for an invalid; it should be kept in a cool place.—*Bulletin Therapeutique.*

VI. COLLODION IN HYDROCELE.—Dr Malik has published a case, in which a child was born with hydrocele of the left tunica vaginalis. Diuretics and local frictions were tried without success. Compression, by means of adhesive plaster, was resorted to, but irritation of the skin was produced without any diminution of the tumour. It was resolved to try collodion. The repeated application of this substance appeared to cause considerable pain; the child cried a deal, and slept ill; but there was no fever, or functional disturbance induced. In a few days notable diminution of the tumour was observed, so the application of the collodion was continued. The child gradually became habituated to the constriction occasioned by the collodion, and was not much annoyed by it. At the end of a month, the little patient was completely cured, no trace of a hydrocele remaining.—*Prag. Vierteljahrsch. Bd. 38.* X

V. APPLICATION OF THE NITRATE OF SILVER FOR THE CURE OF PROLAPSUS ANI, under the care of Mr Lloyd in St Bartholomew's hospital. The plan is to smear the whole surface of the protruded bowel with solid caustic, and then return it. The application is repeated once in a week or fortnight, as may be requisite. Mr Lloyd states that he rarely found it necessary to employ it more than three or four times; and further, that although the plan had been one invariable resort with him, for a long series of years, that he had never known any untoward consequences to result. In cases in which the protruded bowel has become swollen, and is difficult of reduction, the effect of the caustic is surprising. In one such case, the mass could be easily seen to diminish in size under its influence. Mr Lloyd does not limit the use of this remedy solely to prolapsus, but adopts it also in cases of hæmorrhoidal congestion, and thickening of the mucous membrane about the verge of the anus.—*Med. Times.*

VI. PHOSPHATE OF LIME.—Dr Kuchenmeister recommends the following for-
NEW SERIES.—NO. IV. APRIL 1855.

mula in cases in which phosphate of lime is indicated:—*Calcis phosphat.*, ʒij.; *Calcis carbon.* ʒj.; *Sacch. lactis*, ʒij.; *M. ʒss. bis terve in die.* Instead of the milk sugar, lactate of iron may be substituted, if iron be required. The especial use of the carbonate of lime appears to be that carbonic acid is liberated by the acid of the stomach, and dissolves the phosphate. Lactic acid also is formed from the sugar, or is set free from the lactate of iron, and dissolves the phosphate. The most ready way of absorption is, however, when the phosphate is given with food, especially with milk, with which it forms a soluble combination.—*Schmidt's Jahrb.*

VII. UVA URSI INSTEAD OF ERGOT OF RYE.—Mr Harris relates cases in which he has employed a strong decoction of uva ursi in accouchments, where the ergot of rye would ordinarily have been employed, and found its employment followed by vigorous pains, which soon caused the expulsion of both fœtus and placenta. Mr Harris prefers this medicine to ergot of rye, inasmuch as it does not cause such strong contractions as the latter, which are so very painful to the mother and dangerous to the child.—*Virginia Medical Journal.*

Part Fourth.

MEDICAL NEWS.

EDINBURGH OBSTETRICAL SOCIETY.

SESSION XIII.

RECENT VIEWS IN EMBRYOLOGY.

Dr Simpson made a lengthened communication on the more recent views on embryology, and exhibited a very beautiful and extensive series of illustrative casts, models, and drawings.

CASE OF FATAL ACCIDENTAL HEMORRHAGE.

Dr Simpson exhibited the uterus and contents removed from a woman who had died, pregnant with twins at about the sixth month, from hemorrhage.

Dr Gordon, in whose practice the case occurred, mentioned the particulars of the case, and stated that during all her previous pregnancies, the patient had been subject to floodings.

Dr Thomson corroborated *Dr Gordon's* statement.

Dr Simpson made some remarks on the preparation, and exhibited a drawing of the parts as first observed.

ON THE STATE OF THE FŒTAL PULSE AS AN INDICATION FOR ARTIFICIAL DELIVERY. BY PROFESSOR SIMPSON.

Dr Simpson made some remarks on the indications afforded by the stethoscope for expediting delivery, and specially directed the attention of the Society to the fact, that while danger was usually indicated to the mother during labour, by the increased rapidity of her pulse, the death of the child was most frequently threatened when the fœtal pulse became slower and slower. It was known that in cases where, during labour, pressure was exercised upon the cord, the pulsations of the fœtal heart became feebler; and were at length suspended by the continuous pressure. This was most probably the way in which the fœtus perished, during severe and prolonged labours; the aeration of the blood by the placenta being imperfect, or entirely suspended. There were, however, cases in which danger was indicated to the child, by the fœtal pulse becoming much more rapid than ordinary, reaching 150 or 160 beats in the minute, and

at the same time very irregular. Dr S. believed the danger in these cases did not result from pressure on the umbilical cord, as in the cases where the pulsations became slower and slower, but from pressure or some source of irritation acting on the brain.

PRESENTATION OF CORD.

A conversation took place on this subject, and cases illustrating the different and most successful modes of treatment were referred to by various members.

Dr Keiller stated that he had lately witnessed a case of cord presenting with the head, and that he had succeeded in reducing the loop, and supporting it above the brim for two hours, by introducing his hand into the vagina, and which, he stated, the free use of chloroform enabled him to do. The child however, was still-born, apparently from a secondary compression by the shoulders or other parts, after the head had passed through the brim. Dr K. mentioned another case somewhat similar to the above, but in which the child was saved, the cord being reduced by the hand after having failed with the catheter. Dr Keith having also tried the method of reduction by the catheter in this case.

ON THE MAMMARY TREATMENT OF AMENORRHEAL CASES. BY DR KEILLER.

Dr Keiller directed the attention of the society to the treatment of amenorrhea by mammary irritation, and referred to several cases which he thought he had recently cured by this method. Dr K. remarked that our knowledge of the active sympathetic relations existing between the mammary glands in the female and the other more directly sexual organs, first gave rise to the idea that an irritation set up in the former might be advantageously communicated to the latter, and this idea of stimulating the uterine system into increased action, by irritating those parts of the body which, although apparently unconnected, are yet intimately related, has not only been long entertained but often acted upon, with the view not only of stimulating the uterine organs in amenorrheal cases, but of exciting premature labour and checking hæmorrhage, by sympathetically inducing the uterus to contract. The modes of applying this indirect kind of irritation to the uterine organs are either superficial or deep, or both combined (which latter method was that adopted by Dr K.). The superficial stimulation consisted in the application of mammary excitation, embrocations, and especially of sinapisms frequently repeated. The deeper irritation being that derived from the daily use of the common breast-pump, by which we endeavour to increase the action or excite the latent function of the glandular tissue of the mamma. Dr K. had, he believed, witnessed the best effects from this combined mammary treatment in a number of cases which had recently presented themselves for treatment in his ward in the Infirmary. The following case was particularly referred to by Dr K., as affording a good illustration of what has been termed amenorrheal ulcer, i.e., an open sore either intimately connected with, or entirely dependant on, protracted absence of the menses:—

CASE OF AMENORRHEAL ULCER.

Euphemia W., æt. 20, was admitted into the Royal Infirmary on the 2d June. She had never menstruated. The left ankle joint was discoloured, swollen, and presented a considerable sized ulcer over the internal malleolus, which she stated had first broken out when she was 13 years of age, and had existed to a greater or less extent ever since. The history which she gave of her varied state of health, and the relative condition of the ulcer, was corroborated during her stay in the hospital, and this, along with the result of the case, proved the vicarious character of the ulcer. After remaining five months under treatment, during which time various efforts were made to heal the sore on the ankle, but which always had the effect of aggravating the amenorrheal symptoms, the mammary treatment was tried after the usual manner, when she became unwell for the first time in November; soon after

which the sore on the foot began to heal, without being accompanied by the usual constitutional symptoms, and she soon afterwards left the hospital in apparently good health, and the ankle perfectly restored.

MORBID PREPARATIONS.

Morbid preparations were then exhibited, viz., 1st, *A polypus of a singular character removed from the cervix uteri by excision*, by Dr Simpson; 2d, *A specimen of a decidual membrane*, by Dr Simpson; 3d, *Uterus (and appendages) showing vesicular polypi of cervix*, by Dr Keiller; 4th, *Organs of generation of a female (found dead under suspicious circumstances) in which a complete circular hymen existed*, by Dr Keiller. 5th, *The uterus of a lioness that had died three days after parturition, of peritonitis, showing the condition of the inner surface of the impregnated horn to be similar, so far as had been as yet observed by the naked eye and by the microscope, to the other horn which was unimpregnated*, shown by Dr Keiller.

LIQUOR AMNII—ABNORMAL QUANTITY. BY DR JAMES A. SIDLEY.

Case 1.—Mrs B., Stevenlaw's Close, pregnant of her third child, when I saw her in December 1852, with Dr Aiken. She said she was six months gone; at that time she was as large as other women at full time. The os uteri was open to about the size of a shilling; the foetal pulse could not be heard although examined several times. On the 1st January 1853, I was sent for and found her in labour; the abdomen was extremely large, but not pendulous, and the parietes so thin that distinct fluctuation could be felt. I left her, and was again sent for on the morning of the 2d, about five o'clock, when I found the os uteri nearly fully dilated, and the pains very rapid and strong, but not the slightest effect produced on the bag of waters, which was tightly stretched across the os. Believing that there was superabundance of liquor amnii, and over distension of uterus, I gave her a dose of ergot, had her brought to the edge of the bed, and ruptured the membranes with a quill, when about *four gallons and a half* of liquor amnii were caught in basons, and a great quantity besides flowed on the bed and over the floor. The child, evidently about the seventh month, was soon born, but dead; had been so for some days; placenta came away of itself. Uterus contracted well, and no hemorrhage followed. Woman made a good recovery.

Case 2.—Mrs M'K., Richmond Street, abdomen pendulous, pregnant of third child, at full time. Said she was much larger than on previous occasions, and thought she would have twins.

Nov. 19, 1854, 10 a.m.—Labour was tedious during first stage from indurated os from ulcerations; but got great benefit from v. s. and Tart. ant. and a bandage. The os uteri at length was fully dilated, but after that the pains made no progress; membranes were never pressed down. I had her brought to the edge of the bed, and caught in basons *three gallons* of liquor amnii, a good quantity flowing on the bed and floor, the child was born about two hours after; woman and child did well.

UNUSUAL FETAL DEFORMITY—RETENTION OF PLACENTA. COMMUNICATED BY JAMES GERRARD, M.R.C.S., BUCKIE.

"The following case, which came lately under my care while practising in another part of the country, may not be without interest, as presenting a variety of foetal deformity, seldom, I believe, met with. In Sept. 1851, I was hurriedly sent for, to visit Mrs S., a sickly, unhealthy woman æt. 37, in labour with her sixth child, the message being that the child was born, but that "all was not right," and, as the woman was in great danger, urging me to hurry on as fast as possible. On reaching her house, some three miles from my own, I was glad to find matters not so bad as had been described; the child, I was told, had been expelled some two hours previously, and had manifested no signs

of life, the placenta was not removed, the hemorrhage was little more than moderate, there had been a large excess of liquor amnii, a binder had been applied, and altogether the patient felt comfortable. On applying my hand to the abdomen, the uterus was felt contracted but not firm, the os uteri I found almost compressing the cord, wholly preventing me from detecting any portion of the placenta. Considering the case one of irregular contraction, and the symptoms not demanding immediate interference, I judged it the safest course to administer an opiate, and await the result. Meantime the foetus was shown to me; it was full grown, the surface cold and collapsed, the cord properly separated and tied, the upper portion of the body was well developed, the lower extremities were joined into one by a continuation of the common integument, proceeding from the pubis downwards; the soles of the feet were in close apposition, the phalanges of the toes alone being apart from each other, and from those of the opposite foot; there was thus properly no perineum, no anus, the only appearance of sexual organ being a small portion of reflected cuticle in the centre of the pubis, not unlike a rudimental penis. The bones of both sides of this double and single extremity could be easily defined through the structures, and seemed well developed; the double thigh could be flexed on the pelvis, but the leg was firmly fixed in the extended position. I very much regretted being unable to examine the parts within this enclosed perineum, an inspection having been resolutely objected to. I am quite aware that in such circumstances, parties think it no sin to be not over-zealous about the preservation of life, but I had no reason to suspect, much less to attribute neglect in this instance; I had every confidence in the party who was pressed into the service as midwife, and the appearance of the foetus induced me at once to suppose that life had only been uterine. So great had been the excess of liquor amnii as to suggest to the attendants, in the absence of an inspection, the existence of flooding, which goes to confirm the fact noticed by most authorities on midwifery, that where this secretion is in excess, death or disease of the foetus may be anticipated. A fixed state of the knee with permanently extended leg must be ill adapted for accommodating the extremity to the maternal passages during the exit of the shoulders and breech, but in this instance, there seemed to be no difficulty, nor did the patient at the time detect anything unusual in her case. In cases of retained placenta from irregular contraction, some authors wholly condemn, while others give a sort of qualified toleration to the use of opium. No general rule will hold good in every case; in my hands opium has repeatedly been of signal service in irregular contraction, both before and after the expulsion of the foetus, but I should not use it in every case of the kind. I must confess my general confidence in its power to subdue irregular contraction; the dose administered to Mrs S., (tinct. opii. qtt. xxxv.), had the expected effect; in little more than half-an-hour after, I could feel the placenta through the soft and yielding os uteri. I continued to introduce my hand in the cone form recommended in such cases, until I could grasp the placenta, which I then found to be still attached to the uterus by a small portion of its extent, but by compressing it, and slightly twisting it together, I soon succeeded in removing it. The woman made a good recovery, and at the usual time was up and attending to her household duties."

IMPROVED NIPPLE TRACTOR.

Dr Graham Weir exhibited an improved nipple tractor, and which, he stated, he had found extremely useful in forming the nipple in cases of retraction. It consisted of a small vulcanised Indian rubber bag fixed upon a small glass nipple shield, but modified so as to fit the nipple and allow of its being raised without including more of the breast.

UTERINE BOUGIE.

A modification of the uterine bougie was exhibited by *Dr James Sidey*, consisting in the separation of bulb from stem, with the view of its more easy and less painful introduction.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXXIV. FIFTH MEETING.

Wednesday, February 7th, 1855.—WILLIAM SELLER, M.D., President,
in the Chair.

CASE OF CANCER IN VARIOUS ORGANS. BY W. T. GAIRDNER, M.D.—The patient, an old man, had died in the House of Refuge, with no marked affection of any important organ. He had a large tumour in the neck, evidently a cancerous enlargement of the glands in that region. The microscopic examination fully warranted that diagnosis. In the liver there were a few cancerous nodules; and, what was interesting, one of them had been entirely converted into calcareous matter. Near the apex of one of the lungs there was a deposit, which, however, under the microscope, yielded no characteristic corpuscles, but a number of nucleolar bodies, which might be either cancerous or tubercular. The question remained, and it was one which Dr Gairdner could not solve, whether the deposit in question was a tubercular one, passing into a state of obsolescence, or whether this was an instance of the two morbid changes progressing simultaneously.

CASE OF IRREGULARITY OF VESSELS AT ROOT OF NECK. BY JAMES SPENCE, Esq.—The preparation exhibited the right subclavian coming off from the left side and back part of the arch of the aorta: from this origin it passed towards the right side immediately in front of the bodies of the vertebræ and posterior to the œsophagus and trachea—the œsophagus being displaced towards the right side. *Mr Spence* made a few remarks as to the bearings of the case on surgical practice, and as to the altered position of the inferior laryngeal nerve generally noticed in such cases.

CASE OF EXTENSIVE REMOVAL OF LOWER JAW. BY JAMES SPENCE, Esq.—The preparation was obtained from a patient on whom Mr Spence had performed the operation of disarticulation of one side of the lower jaw from within half an inch of the symphysis about thirteen years ago. About five years prior to that time, the greater portion of the right side of the jaw had been removed by Professor Fergusson, now of King's College, London. A dense fibrous texture connected the small portion of the ascending ramus of the right side with the remaining portion near the symphysis, whilst on the left side a similar texture occupied the place of the disarticulated bone—on both sides affording firm attachment to the masseters and other muscles, so that the patient during life had considerable use of the mouth.

CASE OF GOITRE IN AN INFANT. BY J. Y. SIMPSON, M.D.—*Dr Simpson* regarded the pathological specimen (which was handed round) as one of great rarity, and at the same time as of interest, as it satisfactorily disposed of not a few of the current theories as to the production of the disease. Mr Syme, for example, was inclined to ascribe it to the fact that persons affected with the disease were compelled from their occupations to climb great heights, where the air was less dense, and who lived in valleys where there was an equal stagnation; others ascribed it to the use of snow-water, or of water impregnated with iodine. Now the specimen in question proved that goitre was not due to any of these influences. It had been obtained from a child which lived but a few hours after birth. When born, it was nearly asphyxiated, and it had only been kept alive by the presence of a catheter in the throat obviating the pressure of the mass. The brain was of small size, and was covered by a quantity of watery fluid, on the removal of which the brain was found to occupy only the posterior segment of the cranial cavity. The mother was an Italian by birth, and had never suffered from the disease. She had had seven dead children and three living. The cause of this mortality was diseased placentas; and latterly she had taken large quantities of chloride of potass with good

results. No other malformation was ever observed in her other children. In the present instance labour was brought on at the eighth month by the passage of a probe between the membranes, as the child's heart was observed to beat slowly. The enlargement of the neck had the same influence in throwing the head back as the arm of the child when placed under the chin, the labour in consequence was tedious.

Dr Keiller had met with a similar case nine months ago, and the child, he was happy to state, was still alive. The mother came from the neighbourhood of Cockermouth, where he (*Dr Keiller*) believed goitre was prevalent; she had at one time of her life been affected with the disease. The tumour in the child's neck had become smaller since birth.

Dr Simpson believed that there were different kinds of swellings met with in the necks of children. Large ranulae were occasionally present from an enlargement of the salivary glands. A beautiful specimen of which existed in the late *Dr Hamilton's* collection; the tumour was fully half the size of the child's head. He had himself seen two other cases which occurred in the old town of Edinburgh, both of which proved speedily fatal from the gradual enlargement of the ranulae.

ON SOME INTERESTING CASES AMONG THE WOUNDED FROM ALMA. BY JAMES JOHNSTON, M.D. (SURGEON TO THE HAMPSHIRE MILITIA).—The communication was contained in a letter to the Secretary, *Dr Warburton Begbie*, and consisted of a detailed account of cases of gun-shot injury, which had been drafted home to Portsmouth in the *Himalaya*, *Cambria*, and *Avon*.

CASE I. was one of wound of the epigastrium, received on the 20th September, just as the patient, a private in the 55th Regiment, had crossed the river. The shot was the size of an egg, and entered the epigastrium at the edge of the cartilage of the tenth rib on the right side of the thorax. The man rolled over and struggled on the ground, and while so doing the shot fell out at the wound, and was afterwards found in his trousers. There was no hemorrhage, but intense dyspnoea, which continued for thirty-six hours. He lay on the battlefield till the 22d, and partook only of liquids. He expectorated blood for a few days. The wound finally healed in November at Malta. The case continued to progress favourably up to February 1st, 1855, when *Dr Johnston* wrote that there had been sudden recurrence of the dyspnoea, with profuse discharge upwards and downwards of bilious matter, and that hectic had set in. The right lung is condensed inferiorly; and from the bilious symptoms, *Dr Johnston* regards it as probable that the shot had pierced the margin of the diaphragm, entered the right pleura, bruised if not wounded the lung and liver, and then, after rolling about the chest, had escaped by the entrance it had made.

CASE II. A musket bullet had penetrated the medullary canal of the tibia, and twelve days afterwards was successfully extracted. The bullet was peculiar, being cleanly cut through the centre for more than two-thirds of its diameter, for the purpose, it is believed, of increasing its destructive power by its possible division.

CASE III. was struck in the height of the action by a musket ball which entered to the right of the symphysis pubis, crossed the spermatic cord, and plunged deeply into the right thigh under *Poupart's* ligament. For the moment he was doubled up, and ten minutes after there was some bleeding. Great swelling took place in the abdomen, thigh, scrotum and right testis. He never had pain in passing urine or feces. Abscesses formed over the inguinal ring and underneath the scrotum; and fistulae still remained. The bullet had not been extracted. It appeared to be very deeply lodged under *Poupart's* ligament and gave little uneasiness.

CASE IV. was one of bullet wound of the leg with splintering of the tibia. It was received as the patient, a sergeant, after a stumble on the moist ground

on the Russian side of the river, was running to regain his comrades. Some pieces of bone were yet to come away.

CASE V. had his limb amputated below the knee on the field of battle for gunshot injury. He was conveyed to Malta; and the stump did well till on the receipt of a slight injury the entire flap sloughed. Since landing at Portsmouth, cicatrization had rapidly advanced.

CASE VI. was one of amputation of the arm in consequence of hæmorrhage from a sloughing wound of forearm.

CASE VII. was also a musket wound of the forearm. This man was in the act of pulling the trigger when he was struck. The ball entered the inner aspect of the right forearm below the elbow-joint, traversed the bend of the elbow, and passed out behind the head of the radius, which bone was much shattered. Hæmorrhage occurred; but the pulse was normal. The median nerve was injured. Pieces of bone had come away from time to time, and ulcers and sinuses at the points of entrance and exit of the ball, but under careful treatment since his arrival in England the arm had greatly improved, motion and sensation having perceptibly returned.

CASE VIII. was one of grape-shot injury of the leg. The patient was wounded towards the close of the action, just as he had gained the level on the top of the heights about one hundred yards distant from the batteries. The shot entered over the tubercle at the head of the tibia, and extensively shattered the bone, without however breaking it across. It passed out below the head of the fibula, which escaped injury. Hæmorrhage ensued, but was arrested by a tourniquet. In the progress of cure, enormous quantities of loose portions of the tibia had come away; but Dr Johnston hoped to preserve a useful limb.

CASE IX. was a curious one of injury to the abdomen; the bullet had passed into the pelvis, a little way above Poupart's ligament, without wounding the peritoneum. There was numbness of the right lower extremity; and profuse hæmorrhage recurred from time to time. Difficulty in passing urine was occasionally experienced, and pain in defæcation, with the passage of mucus and blood. The ball was never traced. The wound was healing kindly. Dr Johnston regarded it as possible that the ball had either perforated the rectum and been passed by stool, or that it was even now in the bladder.

CASE X. presented a gunshot injury of the leg, without any very remarkable features. It was received as the man was in the act of putting on a copper cap alongside of the batteries, and at the close of the action.

CASE XI. was wounded about three P.M., soon after the heights were gained, by a canister-shot, which struck him about the junction of the tenth rib with its cartilage on the left side, and made its exit nearly opposite to the transverse process of the last lumbar vertebra of the same side. He fell; and there was a gush of blood from the wound. The hæmorrhage continued from both wounds for two days. His breathing was unaffected, and he had no difficulty in urinating or defæcating. A portion of his trousers and a few small pieces of bone came from the posterior wound. The interesting point in this case was, that at Scutari fæces could be squeezed from the upper wound. Hitherto the case had done well.

CASE XII. was wounded at Inkermann, Nov. 5th, 1854, two hours after the action commenced, by a grape-shot which shattered the tibia. The case, beyond the suspicion of a portion of his drawers being still in the limb and protracting recovery, presented little novelty from the similar injuries recorded under cases viii. and x.

The poor men expressed a high opinion of the hospital arrangements at Scutari. The hospital was comfortable, and the supplies excellent. Wounds,

however, did not make satisfactory cures; as many instances had occurred of soldiers returning to the Crimea with their sores healed up to all appearance, which, on relanding, became again open, and resumed their former characters. No complaints were made of want of attention to their sufferings when wounded in the field. Due alacrity appeared to be used in the transport of all the serious cases, which were at once conveyed on board ship. Dr Johnston, in his letter, incidentally also gave a denial to the sweeping charge made by the *Times* of neglect of the wounded landed the other day at Portsmouth. All cases which required immediate medical and surgical aid were promptly attended to, and by four P.M. were all safely housed in a comfortable hospital.

It was unanimously agreed that the thanks of the Society be given to Dr Johnston for his highly interesting paper, and for his kind proposal on some future occasion to favour the Society with another. The President instructed the Secretary accordingly.

NOTES ON THE ADMINISTRATION OF GALLIC ACID, CHIEFLY IN BRIGHT'S DISEASE, AND IN HÆMOPTYSIS. BY W. T. GAIRDNER, M.D.—*Dr Gairdner* had tried gallic acid in the sweating of phthisis, and had found it equally powerless with other remedies in effecting good results-uniformly. In two cases of hæmaturia, of very obstinate character, it was not of the slightest service. In simple diarrhoea he had not found it comparable to the ordinary astringents. In hæmoptysis, although perfectly safe and possibly useful, Dr G. considered that the confident statements of its good effects, particularly in ordinary doses, have been based on insufficient grounds. In albuminuria it had totally failed in producing diminished albuminous secretion. Dr Gairdner concluded a narrative of some of his experiments with gallic acid in disease by a statement by no means favourable to its claims, either as a local or as a general astringent remedy. He had employed it in enormous doses with very little effect.

Dr Matthews Duncan had frequently employed gallic acid in pretty large doses in cases of profuse hæmorrhagia, or of hæmorrhage connected with the presence of fibrous tumours in the uterus; and had always been disappointed in its remedial effects. What he generally employed in preference, and with the best results, were the mineral acids, more particularly the sulphuric.

Dr Alexander Wood had long been convinced of the utter powerlessness of gallic acid in the treatment of hæmoptysis, and cited several cases in which he had ascertained its inefficiency. He considered that cases of copious and sudden hæmorrhage were not the ones which should be chosen for the purpose of testing the power of any astringent. There were too many elements of uncertainty to all of a satisfactory decision; and in fact we possessed no such astringent that ensured arrest of hæmorrhage under such circumstances. Where, however, we had the loss of blood in small quantities, and continuing for a length of time, we might with some confidence put an astringent to the test, and institute a comparison with its rival congeners.

Dr Andrew Wood mentioned that many years ago he had prescribed gallic acid in very large doses, and that to him such a mode of administration was no novelty. In hæmoptysis, he generally employed in preference small doses of ipecacuan. The *modus operandi* of this drug in such cases was not precisely known; but the remarkable effect produced by ipecacuan on the capillaries certainly pointed to an explanation. In the course of his experience, no harm resulted from the vomiting which occasionally followed. As regarded albuminuria, the records of his practice completely confirmed the statements of Dr W. Gairdner. In purpura, however, gallic acid was certainly attended with beneficial effects; and, in cases where turpentine and other remedies had been given in vain, he was satisfied that the acid had powerfully contributed to the cure.

Dr Simpson said, that the case which had first impressed his mind with the efficacy of gallic acid in the treatment of albuminuria was that of a brother practitioner who laboured under the disease, and was compelled to relinquish practice in consequence. Gallic acid was at length tried, and with the best results. He resumed practice some eight or ten years ago, and to this day remained comparatively well, and had no symptoms of albuminuria. The cure in this instance was very probably, however, a mere coincidence, as he (*Dr Simpson*) had never perceived the slightest benefit from the use of the remedy in any of his other cases. Besides, there were many sources of fallacy; as, in acute cases of albuminuria, the patient generally got speedily well; and, in the more chronic forms of the disease, it was long before the health was seriously interfered with. One patient he knew who suffered from Bright's disease so long ago as 1826; and *Dr Bright* himself had predicted a speedy and a fatal issue. The gentleman was still alive, and one of the strongest individuals in his native town, though the albuminuria remained as before. *Dr Gairdner* had exhibited the acid with alkalies—a combination which, according to the observations of *Liebig* on the effect of gallic acid on the blood, must destroy its efficacy as an astringent. With regard to its administration in cases of hæmorrhage, he had formerly brought the subject under the notice of the Society, and stated, as the result of his experience, that it was only occasionally successful. Some ten or twelve years ago, he had occasion to treat a case of severe menorrhagia where all kinds of medicines had been used, but without effect. From the recommendation given by *Sir Benjamin Brodie* in his chapter on *Hæmaturia*, he prescribed *Ruspini's* styptic, and relief speedily followed. The disease recurred, however; and the patent medicine was found to be too expensive. Acting on the hint of *Dr Anthony Todd Thomson*, he (*Dr Simpson*) gave gallic acid, and similar benefit was obtained. *Dr Gairdner* said that tannin had greater effect as an astringent in the treatment of hæmoptysis; but this it was difficult to reconcile with the observation of *Wöhler*, that tannin was converted into gallic acid in its passage through the circulation. *Dr Andrew Wood* had stated his experience of the efficacy of the acid in purpura; while it was well known that *Garrod* had as strongly recommended alkalies in that disease. Such statements he (*Dr Simpson*) thought could be reconciled by supposing that there might be several forms of purpura, each with their stronger affinity to particular remedies; and he (*Dr Simpson*) believed that the uncertainty in the treatment of menorrhagia admitted of a similar explanation.

Dr W. T. Gairdner, in reply, stated that he had only on one or two occasions employed the mixture of the acid with alkalies. In comparing the effects of gallic acid with those of tannin, he of course referred to their local action only; and, for the very reason that gallic acid had little local effect on the tissues, he did not regard it as a true astringent. The question which he had brought under the notice of the Society was, he held, of importance, as it was of great consequence to settle the claims of a remedy which was so much employed, and which, if it should prove worthless, was certainly attended with this evil consequence, that it supplanted remedies of possibly greater efficacy. Again, in the case of the poor, it was no small matter to be able to discard a medicine which was expensive, if it was at the same time inefficacious.

THE WAR.

MEDICAL EVIDENCE BEFORE THE SEBASTOPOL COMMITTEE.

The Earl of Cardigan was examined, and deposed to his landing at Scutari on May 23, and then proceeding to Devna. Whilst there, his men were very healthy till the cholera broke out; he only lost ten by it. A beautiful clear

river ran close by. The rations were good. He subsequently embarked with his men for the Crimea; the weather was very bad. A sad want of forage was experienced after the battle of Balaklava; about 360 horses of his brigade were killed in the charge at Balaklava; and, as a proof of bad arrangement, 100 more had to be shot, as they had no food! In November, the sickness of his division increased to a fearful extent; the sick soldiers had no fuel, and they had to lie on their second horse-blankets and cloaks. Two or three medical officers resigned. It was not at first believed, from Lord Aberdeen's peace policy, that the army were to remain in the Crimea for the winter. Nothing could be worse than the state of the harbour at Balaklava, in a sanitary point of view. The original number of his men was 1250, but there were not more than 300 remaining when he left the Crimea.

Mr Macdonald, who had distributed the *Times* fund, was next examined, chiefly as to the state of the two hospitals at Scutari—the General Hospital and the Barrack Hospital. A Turkish ship and an English transport were also engaged as convalescent hospitals. They were all under the care of Dr Menzies, who had a very inadequate staff until the arrival of nurses and medical men in November from England. The General Hospital was the best; it was quieter, and there were bedsteads for about half the patients; there was little hope, however, of patients recovering in the usual time. The men were very poorly clad; many had nothing but their coats and a single blanket. There were 2800 patients, but this was before the battle of Inkermann; after that engagement, the hospitals were greatly overcrowded. The purveying department was very bad; indeed, not worthy of the name. The first purveyor was a person upwards of seventy years of age, who was exhausted by a walk from one hospital to another, a distance of a quarter of a mile; he had two assistants and two boys—a most inefficient staff. The things he found deficient in the hospitals were tea, arrowroot, sago, sugar, port wine, and brandy. No proper means of conveyance were provided for the wounded; requisitions had to be made to the purveyor through the medical officer. The washing was frightfully deficient; even the hospital floors required it, and he gave 200 scrubbers for the purpose. The laundry had got filled with chopped straw; the washing department, of course, did not belong to the surgeons, but to the purveyors. Opium was very deficient, though Constantinople was the finest opium market in the world; it was the purveyor's duty to supply this also. The French orderly system of men for attending the ambulances was better than that of England; they were trained to it, and originally chosen for the work; but, in our army, directly the medical orderlies or hospital-sergeants knew their business, they were removed to some other employment, and thus gave the surgeons additional labour.

Mr Stafford visited the Scutari hospital in November, and remained several days; never met any opposition as to admission to see the sick. Wrote to the War Department at home to open the hospital now at Smyrna, which is looked upon as a most healthy place, except in June, July, and August. This witness gave a fearful account of the water-closets, but no blame was attachable to the surgeons.) He did not know whether Dr Menzies or Dr McGregor was head surgeon; the latter always evinced the greatest anxiety to promote the reform of the hospital. The wounded spoke in the highest terms of the medical officers, but there was a unanimous expression of condemnation of their treatment in the transport ships.

Mr Roebuck.—Did you see any of the poor fellows as they were landed?

Mr Stafford.—I did; and could not find words to describe their ghastly and filthy appearance; they were covered with lice, as thick as letters on a sheet of paper! The naval hospital was in better order. At Balaklava hospital, after going twice, I was turned out, on the ground that if everybody were admitted, it would put an end to all military arrangements. In this hospital there were fourteen men in one room, and eleven in another, on the bare floor, though there were bedsteads in the passage, but no one to fix them in their

places. There was no washing, but a great deal of cursing and swearing between the sick and orderlies—a sufficient index that all was confusion. Dr Hall was in command. As to coffee, the French roasted it, but received it as we did, quite green. They established a single place for roasting their coffee all at once, and then distributed it to their men fit for use.

Mr Stafford was asked several questions as to the value of peat charcoal as a disinfectant, and whether he thought, if used in the hospitals, it might not prove salutary as a deodoriser, but he seemed to have had no experience whatever of hospitals, except those he visited at Constantinople, which were the first he was tempted to enter. A certain Admiral Boxer, "a seaman of the old school," had all the arrangements under him, as to sending boats for the sick, and something similar was evident in the old-fashioned routine on shore. The men arrived perfectly *exhausted*, whatever other medical name it might get. Things at Scutari were in a state of utter confusion—the beds not numbered; no registers of patients. Once Lady Stratford brought a saucepan of chicken broth—not a cauldron, as some said. The men got baths also; they had bedsores to a terrible extent; the nurses brought them arrowroot and wine. There was a great want of pillows and shirts of every description. The witness was asked to state his personal experience of what he saw and did, and went over all this again; he was then inadvertently blamed by another member of the Committee, for saying so much about himself; he then admitted that the surgeons and official people were all just as busy as himself; but the "flood" of sick was so frightful and sudden, they were all confused. Fifty soldiers' wives were crammed into one small room in the Barrack Hospital, in a most shocking state.

Captain Percy (a private friend of Mr Sidney Herbert) who went out in charge of forty-seven nurses, was next examined, but his evidence was of no particular moment, as he had known nothing previously of hospitals. He noticed two very objectionable things—that the dead-house opened into one of the corridors, and bodies were exposed; and that the orderlies slept amongst the sick, exposed to contagion. He had heard that the hospital was left in a very bad state by the troops, on their way up to Varna. 10,000 iron bedsteads were lost!

Dr Andrew Smith, whose evidence was extended over two days, said he entered the military service in 1815. He had served in the 98th, 49th, and other regiments. Prior to his appointment as Director-General he was staff-surgeon of the first class. His superiors at present were the Commander-in-Chief, the Minister of War, and the Board of Ordnance; from each he received separate and independent directions (as he expressed it in another part of his evidence, the Medical Department lived as a "parasite" on three or four others). It was his duty to propose all medical appointments in the army. He had the general superintendence of all the garrison hospitals in the kingdom—Fort Pitt (Chatham), another at Dublin, Cork, etc. All medical stores were supplied on requisition to him in the first place. As to stores of clothing and medical comforts, formerly (1830) a purveyor-in-chief purchased everything; but since then that duty had devolved on the Ordnance Department. There was, in fact, now much confusion; the War Minister claimed an authority more properly belonging to the Medical Department; there was perpetual conflict of opinion and authority. As to the present expeditionary army at Sebastopol, he was first apprised of it in February last year. He was told by the Commander-in-Chief to provide necessaries of a medical kind for an army of observation, 10,000 men to proceed to Malta; an additional assistant-surgeon was accordingly appointed to each regiment; the staff was also increased. Hospital furniture, medical comforts, etc., were prepared in eight days; the bedding was supplied by the Ordnance. When asked if he saw these articles were duly provided and shipped, he said he had no authority to do so, but persons from his office were despatched daily to ascertain the progress made, but he had not the slightest power over the Ordnance or Commissariat; they

did as they pleased with the medicines. In March (the succeeding month) he had an intimation that an additional force of 25,000 men would be despatched to Turkey, when he took the same steps. As regarded the nature of the climate and local diseases of Turkey, Dr Smith represented to the Commander-in-Chief the necessity of sending out a corps of medical observation, the senior medical officers destined to accompany the army overland, together with a military engineer. The medical officers went accordingly, but no engineer officers. These medical officers reported as to the hygienic and sanitary conditions of the country beforehand, and the instructions were issued, founded on these reports. With regard to the establishment of army hospitals, Dr Smith said he trusted to the medical officers themselves; he did not consider it right to interfere beyond giving advice and suggestions; things were better managed when confided to the discretion of those on the spot, guided by the emergencies of the locality. The medical stores were knocking about from port to port, in place of being delivered at Scutari, but he had no possible control over such matters. He did not think it advisable to trust to the precarious drug-market of Constantinople, so all were sent together. As to purchasing articles on the spot, the purveyor had full power to purchase articles, if necessary; and from what appeared in the public press, there could not have been much difficulty in his doing so. Dr Smith had no authority to interfere in the improvement of the hospitals at Scutari; sufficient power was vested in the ambassador. Since the "war broke out," Dr Smith said he had been labouring day and night (fourteen hours every day). Some fault was found with Dr Lawson, but Lord Raglan's good nature was inclined to forgive him. Dr Smith thought the reports in the public press as to Scutari were very much exaggerated. All the mismanagement arose from the medical stores being left at Varna, and afterwards in the Crimea, everything depending on the Commissariat. Medical officers had some timidity in dealing with the purveyor, and much evil arose from this—not from carelessness about the soldiers. Dr Hall was the head of the entire medical arrangements; and sufficient supplies were despatched to the Crimea when the flank march was known. In answer to a question as to whether Dr Smith believed he had a sufficient staff of surgeons at the commencement of the war, he said he had not, and found the most unexpected and extraordinary difficulties in procuring surgeons. He could manage his office in half the time and with half the work, if he had not so many authorities interfering with the medical department. When he read the accounts in the daily press, he not only wrote to his surgeons and Dr Hall, but sent him the extracts. They were all over-coloured.

HOSPITAL SYSTEM IN THE EAST.—The *Times* ascribes the hospital disasters to Dr Smith and Lord Stratford de Redcliffe. It states that "It is difficult to dwell without most exasperated feelings on the conduct of men through whose cruel apathy so large a portion of our noble army has passed from the field to the hospital, and from the hospital to the grave. The bitterness of these feelings is increased by the reflection that the men who have done these things—the head of the medical department at home, and the ambassador to whom, in an evil hour, the express instructions of our ministers intrusted the care of our mutilated and suffering countrymen abroad—still retain their places uncensured and unquestioned. Against both these functionaries the case seems entirely perfect. To Dr Andrew Smith we must ascribe, in addition to his notorious personal incompetency for the highest place in such a department, the reign of terror among the medical officers which rendered them unwilling to accept assistance, hostile to all sympathisers, and ready, at the cost of thousands of lives, to conceal the faults of the department. Of Lord Stratford de Redcliffe truth compels us to speak in yet more severe terms of reprobation; placed in a most elevated station, intrusted with a boundless command of money for this special purpose, and having before him an inexhaustible field

Dr Beddoe briefly seconded this resolution, remarking that a very little pressure from without would probably compel the Admiralty to do justice in this matter; that the present juncture was peculiarly favourable for the movement, and that it behoved its supporters to speak now, or ever after hold their peace. Carried unanimously.

Mr D. B. Smith then brought forward the following resolution:—"That, in the opinion of this meeting, it would be greatly to be lamented if the students of Edinburgh should in any way favour the continuance of this abuse." He said he had only to look to the number of resignations by assistant-surgeons from the navy, to be convinced that, not one individual alone, but that many simultaneously, had felt that there was something egregiously and painfully at fault in the professional hygiene of that service, the result of which now was a want of the full complement of professional men in the navy. He reminded his fellow-students that they were bound, in justice to themselves, to the school in which they study, and to the public service, to arrive at no conclusions before having heard and duly considered all the bearings of this weighty matter. He judged the present temporary liberality at headquarters to be a pseudo-liberality, and one altogether misapplied. He would fain see, in lieu of it, the system, as a whole, entirely overhauled and amended. Was there really a want of assistant-surgeons? There would be found to be none if parsimony were only set aside, and a due recognition shown of the value of a surgeon, wherever he may serve. *Mr Smith* concluded by saying that the voice of the students of Middlesex and St Bartholomew's Hospital in London, the voice of the press, and the voice of the public, all united in calling loudly for reform in the regulations affecting the surgeon and assistant-surgeon in the navy.

Mr Lowe seconded the resolution, which was also carried.

Dr Broadbent moved—"That the acceptance of the appointments proposed to students by the Admiralty must tend to postpone reform, by providing substitutes for the want of qualified men, which has been produced by the evils of the present system." In proposing this resolution, *Dr Broadbent* dwelt principally on the certainty that gentlemen appointed to act as dressers would really be employed as assistant-surgeons, and he expressed his conviction that numbers of duly qualified men would willingly enter the service at such a juncture as this, if the Admiralty would but accede to their just and moderate demands.

Dr Kirk briefly seconded the resolution, and remarked that those who might now enter the navy as dressers, would have themselves to blame if, two or three years hence, when legally qualified, they found that service partially closed against them by the continuance of the present abuses.

Dr Nicolson, H.E.I.C.S., warned such students as had entertained any idea of accepting the proposals, that if they acted on that idea they would find themselves in a very equivocal and unpleasant position. The functions of dresser to the navy, properly so called, would probably be better left to such men as were employed in that capacity in the Indian navy. These were not gentlemen, but intelligent persons of a lower class, educated specially for the purpose, and competent to do the rough work under the direction of an assistant-surgeon.

This resolution being put from the chair, was carried unanimously.

Mr Leister proposed the fifth resolution:—"That this meeting, therefore, strongly deprecates this proposal, and earnestly calls upon the students of Edinburgh to sacrifice, on public grounds, any private advantages which it may seem to offer." He had peculiar satisfaction in proposing this resolution, because while its adoption might be thought to imply a sacrifice on the part of the students of Edinburgh of their own interests for the sake of the profession generally, yet he had no doubt it would be carried as unanimously as those which had gone before it. He had been told that public-spirited benevolence was not to be expected of medical students; that the pecuniary remuneration or personal professional advantage of any offer made to them would alone be

considered—he did not believe this; he thought that if a man was to be philanthropic in after life he would show evidence of this virtue during his studentship. And he believed that if the students of Edinburgh saw clearly that the acceptance of any offer made to them would have a most prejudicial influence on the medical profession at large, as well as on an important branch of the public service, they would think twice before embracing such an offer, however tempting. But, indeed, he was far from thinking that this one was so. The pay for the six months, including a bonus of L.25 to be given at the end of that time, would be nearly L.80; but this sum would be much encroached on by the reductions mentioned in the Admiralty regulations, so that even the money to be got would not be great, though this was by no means the most important consideration. Then, as to professional advantage, it was very uncertain whether the ship to which a dresser was appointed would come into action at all. It was not for him to repeat what had been said of the condition of assistant-surgeons in the navy, but he would remark that if their position was degraded, surely that of these dressers would be still more so. With no opportunity for study, and without any certainty of seeing active service, few indeed, he believed, would not bitterly regret that they had accepted these tempting dresserships. Then, on the other hand, what would the student give up for such an office? Why, he gave up his studies at the most important period. It had been said that the students were for the most part lazy fellows, who spent the whole six months from April to November in idleness. This he did not believe, and even if it were true, they were bound in considering a public question like the present to make provision, not for the idle and dissolute, but for those who were active in study, and of good moral character, as he believed the majority of Edinburgh students to be. But if such a student went out as a dresser to the Baltic, he would not only break in upon his course of study, and lose, perhaps for ever, valuable opportunities, such as the chance of obtaining a resident office in the Hospital, but would be in imminent danger of acquiring such habits in the cock-pit, as would make it necessary to place him for the future in the other class, the idle and dissolute. Much might be said on this subject, but he knew that nothing more was required to ensure unanimity.

Mr Locking seconded the resolution.

This resolution, like all the preceding, having been carried by acclamation, and thanks having been voted to the Chairman, the meeting separated.

[We understand that similar meetings have been held in Aberdeen, and at other medical schools, and we have little doubt that such firm demonstrations of opinion will produce the desired effect. Already captains in the navy have it in their power to grant the required accommodation, but it has been exercised only in a few instances. There is only one step more for the Admiralty to take, and that is to compel naval commanders to do that which they ought ere now to have done voluntarily, in virtue of the hint already so broadly given them.—Ed.]

DR GRANVILLE ON THE IMPERIAL RUSSIAN FAMILY.

DR GRANVILLE, author of "Travels to St Petersburg," etc., has published a letter which he had addressed to Lord Palmerston on the subject of the probable duration of the late Emperor Nicholas's life, so far back as July 6, 1853. Dr G. had communicated his views to the Premier on account of previous personal acquaintance; but his letter does not seem to have had the desired effect of causing the British Government to treat with the Czar as a person labouring under incipient derangement. There is no denying that the medical prediction has in this case proved true, but still Dr G. has to some extent shown himself to belong to that cautious class of prophets who do not make the pre-

diction public until the event has been realised. He should have published his letter whenever he saw that Lord Palmerston disregarded his advice.

"Confidential Letter to Viscount Palmerston, dated Kissingen, Bavaria, July 6, 1853.

* * * * *

"The health of the Czar is shaken. It has become so gradually for the last five years. He has been irritable, passionate, fanciful, more than usually superstitious, capricious, hasty, precipitate, and obstinate withal,—all from ill-health, unskillfully treated, and of late deteriorating into a degree of cerebral excitement, which, while it takes from him the power of steady reasoning, impels him to every extravagance, in the same manner as with his father in 1800; as with Alexander, in Poland, in 1820; as with Constantine, at Warsaw, in 1830; as with Michael, at St Petersburg, in 1848-9. Like them, his nature feels the fatal transmission of hereditary insanity, the natural consequence of an overlooked and progressive congestion of the brain. Like them, he is hurrying to his fate,—sudden death from congestive disease. The same period of life—between forty-five and sixty years of age—sees the career of this fated family cut short.

"Paul, at first violent and fanatical, a perfect lunatic at forty-five years of age, is despatched at forty-seven, in 1801.

"Alexander dies at Taganrog in December 1825, aged 48. For five years previously his temper and his mind had at times exhibited the parental malady by his capricious and wayward manner of treating the Polish provinces. He died of congestive fever of the brain, during which he knocked down his favourite physician, Sir James Wylie, who assured me of the fact at St Petersburg in 1828, because he wished to apply leeches to his temples.

"Constantine, eccentric always, tyrannical, cruel, dies at Warsaw suddenly, in July 1831, aged 52 years, after having caused rebellion in the country by his harsh treatment of the cadet officers. I saw and conversed with him on the parade, and in his palace, at Warsaw in December 1828. His looks and demeanour sufficiently denoted, to a medical man, what he was and what his fate would be. It has been said that he died of cholera; again, that he had been despatched like his father. The physician of the Polish military hospitals assured me, some years after, that he had died apoplectic and in a rage.

"Michael, after many years of suffering from the same complaints which afflict his only surviving brother—enlarged liver, deranged digestion, and fullness of blood in the head—became in 1848-9 intolerably irritable, violent, and tyrannical to his own officers of the artillery and engineers service, of which he was the supreme chief. In July 1849, he consulted me at St Petersburg. It was after he had passed in review the whole train of artillery which was leaving the capital for Hungary, at which review I was present and near him, and witnessed scenes of violent temper towards generals and aides-de-camps hardly equalled in a lunatic asylum. I found him as described above. I advised cupping, diet, non-exposure to the sun and to fatigue, the administration of suitable medicines, and the cessation from drinking steel mineral waters, of which he was fond ever since he had been at Kissingen. His physician, the younger Sir James Wylie (himself since suddenly dead) assented reluctantly, but did not carry my advice into execution. The Grand Duke, in the state he was, unrelieved by any medical measure or proper treatment, joined the army, rode out in the sun, and fell from his horse apoplectic, in September, 1849, aged 48.

"To complete this disastrous picture of the grandchildren of Catherine, their mother, Maria of Wurtemberg, a most exemplary princess, died apoplectic in November 1829, scarcely more than 65 years of age. The attack, mistaken for weakness, was treated with stimulants and bark by her physician, Ruhl, and bleeding was only had recourse to when the mistake was discovered—but too late to save. The meek and mild Elizabeth had but a short time before

ollowed her imperial partner, Alexander, to the grave, in the still fresh years of womanhood, 60 years of age.

"During my second sojourn in St Petersburg, in 1849, for a period of ten weeks What the opinion was of the Emperor's health—what acts of his came to my knowledge, which bespoke eccentricity—what were the sentiments of his physician, Dr Mandt, who, homœopathist as he is, and exercising most peremptory influence over his master, leaves him, nevertheless, unrelieved, except by mystical drops and globules—what transpired of political doctrines and opinions, or, in fine, what I gathered afterwards at Moscow on all co-equal points, must be left to your Lordship's conjecture—not difficult, after all I have divulged. To go further would be like a breach of trust, and of that I shall never be guilty.

"In all I have related there is nothing that had been committed to me as a privileged communication; while the imperative requirements of the moment, calling for its immediate divulgement, I hesitate not to make it, under the firmest conviction that my fears and anticipations will be surely realized.

"If so, then the method of dealing with an all-powerful sovereign so visited must differ from the more regular mode of transacting business between Government and Government. For this purpose it is—namely, to put her Majesty's ministers on their guard accordingly, that I have determined to place in your Lordship's hands the present professional information, which must be considered as so strictly confidential that I shall not sign it with my name.

"That I have selected your Lordship as the channel of my communication rather than the Minister of Foreign Affairs, to whom more properly it should have been addressed, will at once appear natural to your Lordship. In my capacity of—once, and for some years—your Lordship's physician (though not now honoured with that title), your Lordship has known me personally, and is convinced that what my pen commits to paper may be taken as coming from an honourable man, and your obedient servant."

N.B.—An acknowledgment of the receipt of this letter came by return of post, in Lord Palmerston's handwriting.

Memorandum.—At an interview with Lord Palmerston, February 23, 1854, on matters of a private nature, his Lordship was pleased to ask me before we separated whether I still adhered to my opinion and prediction. I replied, that before July 1855 (the Emperor would then be 59 years old), what I had anticipated would happen. "Let but a few reverses overtake the Emperor," I added, "and his death, like that of all his brothers, will be sudden." It has proved so. Alma, Inkermann, Balaklava, shook the mighty brain; Eupatoria completed the stroke, which has anticipated my prognosis only by a few weeks.

A. B. G.

AGASSIZ AND THE EDINBURGH CHAIR OF NATURAL HISTORY.

THE Chair of Natural History, vacant by the death of Edward Forbes, still remains without an occupant. The patronage is vested in the Crown; but as on the occasion of the two last vacancies the Town-Council of Edinburgh prompted the Government regarding the selection of a professor, so at the present juncture they have attempted a similar movement, and have thereby threatened, in this quarter at least, a collision between science and theology. We all know how a privilege which is allowed to be exercised unchallenged soon comes to be claimed as a right; and in like manner precedents are supposed to be formed by the fortuitous occurrence of certain incidents. Because the Town-Council of Edinburgh (of which collectively we desire to speak with all deference) has twice, under peculiar circumstances, taken upon itself to suggest to her Majesty's advisers who should fill the Natural History Chair in the Metropolitan University, it seems to have been concluded that they should ex-

ercise a similar liberty on a third, and probably on all subsequent occasions of vacancy. To this we demur. It might have been seemly to endorse the claims of a rising citizen in the person of Jameson; and it was a municipal compliment to Forbes, after it was known that he was to be appointed, that our civic rulers should step forward and intimate that the man whom the Queen had delighted to honour was also the man who would have been the choice of the patrons, had the election rested with them. But the present crisis does not furnish an analogous case. We have no indigenous claimant to professorial honours; and in respect to exotic aspirants, the Crown has emitted no sign. The Council therefore has, in our humble opinion, stepped beyond its province, and has interfered with the patronage of Downing Street in a way that the dignitaries of the Royal Exchange would have resented, had Downing Street used a corresponding freedom with them. At a recent meeting of the Council it was proposed that Government should be memorialized to present M. Agassiz to the chair—a movement which the result has proved to be unfortunate in more respects than one. In the first place, no steps appear to have been taken to ascertain if M. Agassiz would, at the call of the British Government, backed by the Edinburgh Town-Council, be willing to leave his adopted Transatlantic home. Indeed, the probability is that he would not accept a call from Scotland. And secondly, and still worse, no preliminary sounding was made as to whether even the Council itself would be unanimous in an invitation to Agassiz to become a member of our university. Two or three councillors, at least, hold that he is heretical in his opinions as to the descent of the human race, and the opposition of this minority led, at the first discussion, to the suspicious decision that further inquiry was necessary, and at the second, that the motion should be withdrawn altogether. For this conclusion Agassiz has to thank the proverbial discretion of friends.

Without entering minutely into the circumstances, we submit that Agassiz is entitled to be fairly dealt with. If he were one of a class of scoffers such as the Encyclopædists whom Voltaire gathered round him at Ferney, and who would have attacked any doctrine of the Scriptures simply because it was declared in the Scriptures, there might be a preliminary case against him; but it is another thing when a naturalist understood to be devoutly disposed towards religion, fairly and dispassionately prosecutes his scientific inquiries and arrives at a finding which, as we are informed, he does not submit as incompatible with the Bible, but simply as opposed to the popular interpretation of certain passages in the Bible. That such a course is legitimate, the history of astronomy and of geology amply testify; and that it does not clash with certain schools of theology will be seen from the writings of Archbishop Whately. In treating of this subject, that able divine informs us that—

“Some persons have imagined that we are bound to take our notions of astronomy, and of all other physical sciences, from the Bible. And accordingly, when astronomers discovered, and proved, that the earth turns round on its axis, and that the sun does not move round the earth, some cried out against this as profane, because Scripture speaks of the sun’s rising and setting. And this probably led some astronomers to reject the Bible, because they were taught that if they received *that* as a divine revelation, they must disbelieve truths which they had demonstrated.

“So, also, some have thought themselves bound to believe, if they receive Scripture at all, that the earth, and all the plants and animals that ever existed on it, must have been created within six days, of exactly the same length as our present days. And this, even before the sun, by which we measure our days, is recorded to have been created. Hence, the discoveries made by geologists, which seem to prove that the earth and various races of animals must have existed a very long time before man existed, have been represented as completely inconsistent with any belief in Scripture.

“It would be unsuitable to such a work as this to discuss the various objections (some of them more or less plausible, and others very weak) that have

been brought—on grounds of science, or supposed science—against the Mosaic accounts of the Creation—of the state of the early world—and of the Flood, and to bring forward the several answers that have been given to those objections. But it is important to lay down the PRINCIPLE on which either the Bible or any other writing or speech ought to be studied and understood : namely, with a reference to the object proposed by the writer or speaker.

“ For example ; suppose you bid any one proceed in a straight line from one place to another, and to take care to arrive before the sun goes down. He will rightly and fully understand you, in reference to the practical object which alone you had in view. Now, you perhaps know very well that there cannot really be a straight line on the surface of the earth, which is a sphere [globe] ; and that the sun does not really go down, only, our portion of the earth is turned away from it. But whether the other person knows all this or not, matters nothing at all with reference to your present object ; which was not to teach him mathematics or astronomy, but to make him conform to your directions, which are equally intelligible to the learned and the unlearned.

“ Now the object of the Scripture revelation is to teach men, not astronomy or geology, or any other physical science, but religion. Its design was to inform men, not in what manner the world was made, but who made it ; and to lead them to worship Him, the Creator of the heavens and the earth, instead of worshipping his creatures, the heavens and earth themselves, as gods ; which is what the ancient heathen actually did.

“ Although, therefore, Scripture gives very scanty and imperfect information respecting the earth and the heavenly bodies, and speaks of them in the language and according to the notions, of the people of a rude age, still it fully effects the object for which it was given, when it teaches that the heavens and the earth are not gods to be worshipped, but that “ God created the heavens and the earth,” and that it is He who made the various tribes of animals, and also man.

“ But as for astronomy and geology and other sciences, men were left—when once sufficiently civilized to be capable of improving themselves—to make discoveries in them by the exercise of their own faculties.”—*Whatley on Scientific Difficulties connected with Scripture.*

Dr Whatley speaks on the general question of biblical interpretation in its bearings on scientific inquiry. On the special point of the unity of the human race, Mr Hugh Miller holds (see after quotation) that Agassiz may be scientifically correct and yet theologically wrong. But before quoting Mr Miller, we must refer to the more recent views of Agassiz himself (originally they were propounded in the *Revue Suisse* for 1845), as these are contained in Nott and Gliddon’s “Types of Mankind.” The object of the naturalist is to show that there is a “close connection between the geographical distribution of animals and the natural boundaries of the different races of man,” a connection which he maintains “cannot be explained by any theory of the origin of life which claims to cover the whole of this difficult problem.” “I do not pretend,” he continues, “to present such a theory now, but would simply illustrate the facts as they are, to lay the foundation of a more extensive work, to be published at some future time.” He groups man into eight realms, the Arctic, Asiatic, European, American, African, East Indian, Australian, and Polynesian, and points out how this geographical distribution of certain animals coincides with the human inhabitants of the “realms” specified. A few passages from the sketch may convey an outline of Agassiz’ theory.

AGASSIZ’ THEORY.

“The boundaries within which the different natural combinations of animals are known to be circumscribed upon the surface of our earth, coincide with the natural range of distinct types of man. Such natural combinations of animals circumscribed within definite boundaries are called *faunæ*, whatever be their home—land, sea, or river. Among the animals which compose the fauna

of a country, we find types belonging exclusively there, and not occurring elsewhere; such are, for example, the ornithorynchus of New Holland, the sloths of America, the hippopotamus of Africa, and the walruses of the arctics: others, which have only a small number of representatives beyond the fauna which they specially characterize, as, for instance, the marsupials of New Holland, of which America has a few species, such as the opossum; and again others which have a wider range, such as the bears, of which there are distinct species in Europe, Asia, or America, or the mice and bats, which are to be found all over the world, except in the arctics. That fauna will, therefore, be most easily characterized which possesses the largest number of distinct types, proper to itself, and of which the other animals have little analogy with those of neighbouring regions, as, for example, the fauna of New Holland.

"The inhabitants of fresh waters furnish also excellent characters for the circumscription of faunæ. The fishes, and other fluviatile animals from the larger hydrographic basins, differ no less from each other than the mammalia, the birds, the reptiles, and the insects of the countries which these rivers water. Nevertheless, some authors have attempted to separate the fresh water animals from those of the land and sea, and to establish distinct divisions for them, under the name of fluviatile faunæ. But the inhabitants of the rivers and lakes are too intimately connected with those of their shores to allow of a rigorous distinction of this kind. Rivers never establish a separation between terrestrial faunæ. For the same reason, the faunæ of the inland seas cannot be completely isolated from the terrestrial ones, and we shall see hereafter that the animals of southern Europe are not bound by the Mediterranean, but are found on the southern shore of that sea, as far as the Atlas. We shall, therefore, distinguish our zoological regions according to the combination of species which they enclose, rather than according to the element in which we find them.

"If the grand divisions of the animal kingdom are primordial and independent of climate, this is not the case with regard to the ultimate local circumscription of species: these are, on the contrary, intimately connected with the conditions of temperature, soil, and vegetation. A remarkable instance of this distribution of animals with reference to climate may be observed in the arctic fauna, which contains a great number of species common to the three continents converging towards the North Pole, and which presents a striking uniformity, when compared with the diversity of the temperate and tropical faunæ of those same continents.

"The arctic fauna extends to the utmost limits of the cold and barren regions of the North. But from the moment that forests appear, and a more propitious soil permits a larger development of animal life and of vegetation, we see the fauna and flora, not only diversified according to the continents on which they exist, but we observe also striking distinctions between different parts of the same continent; thus, in the old world, the animals vary, not only from the polar circle to the equator, but also in the opposite direction—those of the western coast of Europe are not the same as those of the basin of the Caspian Sea, or of the eastern coast of Asia, nor are those of the eastern coast of America the same as those of the western.

"The first fauna, the limits of which we would determine with precision, is the arctic. It offers, as we have just seen, the same aspects in three parts of the world, which converge towards the North Pole. The uniform distribution of the animals by which it is inhabited forms its most striking character, and gives rise to a sameness of general features which is not found in any other region. Though the air-breathing species are not numerous here, the large number of individuals compensates for this deficiency, and among the marine animals we find an astonishing profusion and variety of forms.

"In this respect the vegetable and animal kingdoms differ entirely from each other, and the measure by which we estimate the former is quite false as applied to the latter. Plants become stunted in their growth or disappear be-

fore the rigours of the climate, while, on the contrary, all classes of the animal kingdom have representatives, more or less numerous, in the arctic fauna.

"Neither can they be said to diminish in size under these influences; for, if the arctic representatives of certain classes, particularly the insects, are smaller than the analogous types in the tropics, we must not forget, on the other hand, that the whales and larger cetacea have here their most genial home, and make amends, by their more powerful structure, for the inferiority of other classes. Also, if the animals of the North are less striking in external ornament—if their colours are less brilliant—yet we cannot say that they are more uniform, for though their tints are not so bright, they are none the less varied in their distribution and arrangement.

"The limits of the arctic fauna are very easily traced. We must include therein all animals living beyond the line where forests cease, and inhabiting countries entirely barren. Those which feed upon flesh seek fishes, hares, or lemmings, a rodent of the size of our rat. Those which live on vegetable substances are not numerous. Some gramineous plants, mosses, and lichens, serve as pasture to the ruminants and rodents, while the seeds of a few flowering plants, and of the dwarf birches, afford nourishment to the little granivorous birds, such as linnets and buntings. The species belonging to the sea-shore feed upon marine animals, which live, themselves, upon each other, or upon marine plants.

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"Though I still hesitate to assign to each [of the three distinct races within the boundaries of the East Indian realm] an independent origin (perhaps rather from the difficulty of divesting myself of the opinions universally received than from any intrinsic evidence), I must, in presence of these facts, insist, at least, upon the probability of such an independence of origin of all nations; or, at least, of the independent origin of a primitive stock for each, with which at some future period migrating or conquering tribes have more or less completely amalgamated, as in the case of mixed nationalities.

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"Before closing these remarks I should add, that one of the greatest difficulties naturalists have met with, in the study of the human races, has been the want of a standard of comparison by which to estimate the value and importance of the diversities observed between the different nations of the world. But (since it is idle to make assertions upon the character of these differences without a distinct understanding respecting the meaning of the words constantly used in reference to the subject), it may be proper to ask here, What is a species, what a variety, and what is meant by the unity or the diversity of the races?

"In order not to enter upon debateable ground in answering the first of these questions, let us begin by considering it with reference to the animal kingdom; and, without alluding to any controverted point, limit ourselves to animals well known among us. We would thus remember that, with universal consent, the horse and ass are considered as two distinct species of the same genus, to which belong several other distinct species known to naturalists under the names of zebra, quagga, dauw, etc. The buffalo and the bull are also distinct species of another genus, embracing several other foreign species. The black bear, the white bear, the grizzly bear, give another example of three different species of the same genus, etc. etc. We might select many other examples among our common quadrupeds, or among birds, reptiles, fishes, etc., but these will be sufficient for our purpose. In the genus horse we have two domesticated species, the common horse and the donkey; in the genus bull, one domesticated species and the wild buffalo; the three species of bear mentioned are only found in the wild state. The ground upon which these animals are considered as distinct species is simply the fact, that, since they have been known to man, they have always preserved the same characteristics. To make specific difference

or identity depend upon genetic succession, is begging the principle and taking for granted what in reality is under discussion. It is true that animals of the same species are fertile among themselves, and that their fecundity is an easy test of this natural relation; but this character is not exclusive, since we know that the horse and the ass, the buffalo and our cattle, like many other animals, may be crossed; we are, therefore, not justified, in doubtful cases, in considering the fertility of two animals as decisive of their specific identity. Moreover, generation is not the only way in which certain animals may multiply, as there are entire classes in which the larger number of individuals do not originate from eggs. Any definition of species in which the question of generation is introduced is, therefore, objectionable. The assumption, that the fertility of cross-breeds is necessarily limited to one or two generations, does not alter the case; since, in many instances, it is not proved beyond dispute. It is, however, *beyond all question* that individuals of *distinct* species may, in certain cases, be productive with one another, as well as with their own kind. It is equally certain that their offspring is a half-breed; that is to say, a being partaking of the peculiarities of the two parents, and not identical with either. The only definition of species meeting all these difficulties is that of Dr Morton, who characterizes them as *primordial organic forms*. Species are thus distinct forms of organic life, the origin of which is lost in the primitive establishment of the state of things now existing, and varieties are such modifications of the species as may return to the typical form, under temporary influences. Accepting this definition with the qualifications just mentioned respecting hybridity, I am prepared to show that the differences existing between the races of men are of the same kind as the differences observed between the different families, genera, and species of monkeys or other animals; and that these different species of animals differ in the same degree one from the other as the races of men—nay, the differences between distinct races are often greater than those distinguishing species of animals one from the other. The chimpanzee and gorilla do not differ more one from the other than the Mandingo and the Guinea Negro: they together do not differ more from the orang than the Malay or white man differs from the Negro. In proof of this assertion, I need only refer the reader to the description of the anthropoid monkeys published by Prof. Owen and by Dr J. Wyman, and to such descriptions of the races of men as notice more important peculiarities than the mere differences in the colour of the skin. It is, however, but fair to exonerate these authors from the responsibility of any deduction I would draw from a renewed examination of the same facts, differing from theirs; for I maintain distinctly that the differences observed among the races of men are of the same kind and even greater than those upon which the anthropoid monkeys are considered as distinct species.

"Again, nobody can deny that the offspring of different races is always a half-breed, as between animals of different species, and not a child like either its mother or its father. These conclusions in no way conflict with the idea of the unity of mankind, which is as close as that of the members of any well-marked type of animals; and whosoever will consult history must remain satisfied, that the moral question of brotherhood among men is not any more affected by these views than the direct obligations between immediate blood relations. Unity is determinative by a typical structure, and by the similarity of natural abilities and propensities; and, unless we deny the typical relations of the cat tribe, for instance, we must admit that unity is not only compatible with diversity of origin, but that it is the universal law of nature.

"This coincidence, between the circumscription of the races of man and the natural limits of different zoological provinces characterized by peculiar distinct species of animals, is one of the most important and unexpected features in the Natural History of Mankind, which the study of the geographical distribution of all the organized beings, now existing upon earth, has disclosed to us. It is a fact which cannot fail to throw light, at some future time, upon the very origin of the differences existing among men, since it shows that

man's physical nature is modified by the same laws as that of animals, and that any general results obtained from the animal kingdom regarding the organic differences of its various types must also apply to man.

"Now, there are only two alternatives before us at present :—

"1st. Either mankind originated from a common stock, and all the different races with their peculiarities, in their present distribution, are to be ascribed to subsequent changes—an assumption for which there is no evidence whatever, and which leads at once to the admission that the diversity among animals is not an original one, nor their distribution determined by a general plan, established in the beginning of the Creation ;—or,

"2d. We must acknowledge that the diversity among animals is a fact determined by the will of the Creator, and their geographical distribution part of the general plan which unites all organized beings into one great organic conception : whence it follows that what are called human races, down to their specialization as nations, are distinct primordial forms of the type of man.

"The consequences of the first alternative, which is contrary to all the modern results of science, run inevitably into the Lamarckian development theory, so well known in this country through the work entitled 'Vestiges of Creation ;' though its premises are generally adopted by those who would shrink from the conclusions to which they necessarily lead.

"Whatever be the meaning of the coincidence alluded to above, it must in future remain an important element in ethnographical studies ; and no theory of the distribution of the races of man, and of their migrations, can be satisfactory hereafter, which does not account for that fact.

"We may, however, draw already an important inference from this investigation, which cannot fail to have its influence upon the farther study of the human races : namely, that the laws which regulate the diversity of animals, and their distribution upon earth, apply equally to man, *within the same limits and in the same degree* ; and that all our liberty and moral responsibility, however spontaneous, are yet instinctively directed by the All-wise and Omnipotent, to fulfil the great harmonies established in nature."—*Gliddon and Nott*.

Mr Hugh Miller, in reviewing "Smyth's History of the Human Races," thus alludes to the Agassizian theory :—

HUGH MILLER ON AGASSIZ.

"The analogies *may* be on the side of the naturalist—as M. Agassiz says they are—and he may be quite right in holding, that varieties of the race so extreme as that of the negro on the one side, and the blue-eyed, fair-haired, diaphanous Goth on the other, could not have originated *naturally* in a species possessed of a common origin, during the brief period limited by authentic history on the one hand, and the first beginnings of a family so recent as that of *man* on the other. But, though he may possibly be right as a naturalist—though we think that matter admits of being tried, for it is far from settled—he may be none the less wrong on that account as a theologian. His inferences may be right and legitimate in themselves, and yet the main deduction founded upon them be false in fact. Let us illustrate. There is nothing more certain than that the human species is of comparatively recent origin. All geological science testifies that man is but of yesterday ; and the profound yet exquisitely simple argument of Sir Isaac Newton, as reported by Mr Conduit, bears with singular effect on the same truth. Almost all the great discoveries and inventions, argued the philosopher, are of comparatively recent origin : perhaps the only great invention or discovery that occurs in the fabulous ages of history is the invention of letters ; all the others—such as the mariner's compass, printing, gunpowder, the telescope, the discovery of the New World and Southern Africa, and of the true position and relations of the

earth in the solar system—lie within the province of the authentic annalist; which, man being the inquisitive, constructive creature that he is, would not be the case were the species of any very high antiquity. We have seen since the death of Sir Isaac, steam, gas, and electricity, introduced as new forces into the world; the race, in consequence, has, in less than a century and a half, grown greatly in knowledge and in power; and, by the rapid rate of the increase, we argue with the philosopher, that it can by no means be very ancient;—had it been on the earth twenty, fifty, or a hundred thousand years ago, steam, gas, and electricity, would have been discovered hundreds of ages since, and it would at this date have no such room to grow. And the only very ancient history which has a claim to be authentic—that of Moses—confirms, we find, the shrewd inference. Now, with this fact of the recent origin of the race on the one hand, and the other fact, that the many various languages of the race so differ, that there are some of them which have scarce a dozen of words in common, a linguist, who confined himself to the consideration of natural causes, would be quite justified in arguing that these languages could not possibly have changed to be what they are, from any such tongue, in the some five or six thousand years to which he finds himself restricted by history, geology, and the inferences of Sir Isaac. It takes many centuries thoroughly to change a language, even in the present state of things, in which divers languages exist, and in which commerce, and conquest, and the demands of literature, are ever incorporating the vocables of one people with those of another. After the lapse of nearly three thousand years, the language of modern Greece is essentially that in which Homer wrote; and by much the larger part of the words in which we ourselves express our ideas, are those which Alfred employed when he propounded his scheme of legislative assemblies and of trial by jury. And were there but one language on earth changes in words or structure would of necessity operate incalculably more slowly. Nor would it be illogical for the linguist to argue, that if, some five or six thousand years ago, the race, then in their extreme infancy, had not a common language, they could not have originated as one family, but as several, and so his conclusion would in effect be that of the American naturalists. But who does not see that, though *right* as a linguist, he would be *wrong* as a theologian—wrong in fact? Reasoning on but the common and the natural, he would have failed to take into account, in his calculation, one main element—the element of miracle, as manifested in the confusion of tongues at Babel; and his ultimate finding would in consequence be wholly erroneous. Now, it is perhaps equally possible for the naturalist to hold that two such extreme varieties of the human family as the negro and the Goth could not have originated from common parents in the course of a few centuries—and certainly the negro does appear in history not many centuries after the Flood. He had assumed his deep black hue six hundred years before the Christian era, when Jeremiah used his well-known illustration, “Can the Ethiopian,” etc.; and the negro head and features appear among the sculptures and paintings of Egypt several centuries earlier. Nay, negro skulls of a very high antiquity have been found among the mummies of the same ancient kingdom. But though, with distinguished naturalists on the other side, we would not venture authoritatively to determine that a variety so extreme could have originated in the ordinary course of nature in so brief a period, just as we would hesitate to determine that a new language could originate naturally in other than a very extended term, we would found little indeed upon such a circumstance, in the face of a general tradition that the negroid form and physiognomy were marks set upon an offending family, and scarce were less the results of miracle than the confusion of tongues. We are far from sure, however, that it is necessary to have recourse to miracle. The Goth is widely removed from the negro; but there are intermediate types of man that stand in such a midway relation to both, that each variety, taking these as the central type, is divested of half its extremeness.

"We had purposed referring at some length to that portion of the argument which is made to rest on analogy. We have, however, more than exhausted our space; and merely remark, that it is not at all a settled point that the analogies are in favour of creation in a plurality of centres. Linnæus, and his followers in the past, and men such as Edward Forbes in the present, assert exactly the contrary; and, though the question is, doubtless, an obscure and difficult one—so much so, that he who takes up either side, and incurs the *onus probandi* of what he asserts, will find he has but a doubtful case—the doubt and obscurity lie quite as much on the one side as the other. Even, however, were the analogies with regard to vegetables and the lower animals in favour of creation in various centres, it would utterly fail to affect the argument. Though the dormouse and the Scotch fir had been created in fifty places at once, the fact would not yield us the slightest foundation for inferring that man had originated in more than a single centre. Ultimately, controversies of this character will not fail to be productive of good. They will leave the truth more firmly established, because more thoroughly tried, and the churches more learned. Nay, should such a controversy as the present at length convince the churches that those physical and natural sciences which, during the present century, have been changing the very face of the world and the entire region of human thought, must be sedulously studied by them, and that they can no more remain ignorant without sin than a shepherd can remain unarmed in a country infested by beasts of prey, without breach of trust, it will be productive of much greater good than harm."

On such a subject the opinions of Dr Pye Smith, who was justly esteemed both as a naturalist and a divine, will be consulted with interest. And, accordingly, we find him quoted in the *Scotsman* newspaper as supporting Agassiz:—

OPINION OF DR PYE SMITH.

"'It would be wrong,' says Dr Smith, 'to conceal the difficulties with which the subject is surrounded, however satisfied we may be with the evidence in favour of the descent of all mankind from one original pair of ancestors;' and he then quotes a targum, or old Jewish paraphrase, of Gen. ii. 7, in which it is explained that God created man *red, black, and white*.

"Dr Smith admits that the action of the solar light and heat in tropical climates only produces various shades of brown, but 'we have no instance of a white family or community acquiring the proper negro colour;' nor of a negro family becoming of a healthy European white, except by intermarriages. This permanence of the white and black complexions suggests another difficulty. 'The recent explorings,' he observes, 'of the Egyptian tombs and temples have brought to light pictures of native Egyptians, and of men and women of other nations, comprising *negroes*, who are distinguished by their characteristic form of face, and their *completely black* colour. Some of these highly interesting representations are proved to be of the age of Joseph, and earlier, and some in which the negro figures occur are of the eighth century after the Flood. Assuming, then, that the complexion of Noah's family was what I have ventured to suppose as the normal brown, *there was not time for a negro race to be produced* by the operation of all the causes of change with which we are acquainted.' Who, indeed, will believe that a Spaniard transplanted to Guinea would become a negro in twenty-four generations? The force of the objection is vastly increased when we refer to the history of the Berbers, Tibboos, and Tuaricks, all speaking the same radical language, and spread over the oases of the Sahara, from Morocco to Egypt, who have lived under the same burning sun with the blacks since the time of Herodotus (2300 years), and are only *brown*—no more negroes than the Moors or Egyptians.

"Adam might be the first created man, the protoplast of the race, a fair representative of all its qualities, without being literally the father of all man-

kind. 'Mr Edward King, a zealous Christian,' says Dr Smith, 'strenuously maintained the opinion of the plurality of human ancestry.' The illustrious Dr Arnold of Rugby also held that 'the physiological question was not settled.' 'Nor can we affirm it to be an impossibility that the Almighty Creator should have seen fit to bring originally into being duplicates, triplicates, or other multiples of pairs, formed so alike that there should be no specific difference between them.' In a word, that they should be capable of perfect amalgamation.

"With regard to Acts xvii. 26, it cannot be proved that 'one blood' necessarily signifies descent from a common ancestry; for, admitting a specific identity, though having proceeded from distinct foci of creation, both the physical and mental characteristics would be the same in all essential qualities.

"But if we carry our concessions to the very last point—if the progress of investigation should indeed bring out such kinds and degrees of evidence as shall rightfully turn the scale in favour of the hypothesis that there are several races of mankind, each having originated in a different pair of ancestors—what would be the consequence to our highest interests, as rational, accountable, and immortal beings? Would our *faith*, the fountain of motives for love and obedience to God, virtuous self-government, and universal justice and kindness—would this faith, 'the substance of things hoped for, the evidence of things not seen,' sustain any detriment, after, by due meditation and prayer, we had surmounted the first shock? Let us survey those consequences.

"If the two first inhabitants of Eden were the progenitors, not of all human beings, but only of the race whence sprung the Hebrew family, still it would remain the fact, that *all* were formed by the immediate power of God, and all their circumstances, stated or implied in the Scriptures, would remain the same as to moral and practical purposes.

"Adam would be "a figure of Him that was to come," the Saviour of mankind; just as Melchizedek, or Moses, or Aaron, or David; the spiritual lesson will be the same.

"The sinful character of all the tribes of men, and the individuals composing them, would remain determined by the most abundant and painfully demonstrated proofs, in the history of all times and nations. The way and manner in which moral corruption has thus infected all men, under their several heads of primeval ancestry, would be an inscrutable mystery (which it is *now*); but the need of divine mercy, and the duty to seek it, would be the same; the same necessity would exist of a Saviour, a redemption, and a renovation of the internal character by efficacious grace.

"That the Saviour was, in his human nature, a descendant of Adam, would not militate against his being a proper Redeemer for all the races of mankind, any more than his being a descendant of Abraham, Judah, and David, at all diminishes his perfection to save us, "sinners of the Gentiles."

"Some difficulties in the Scripture-history would be taken away; such as—the sons of Adam obtaining wives not their own sisters;—Cain's acquiring instruments of husbandry, which must have been furnished by miracle immediately from God upon the usual supposition;—his apprehensions of summary punishment ("any man that findeth me will slay me");—his fleeing into another region, of which Josephus so understands the text, as to affirm that Cain obtained confederates, and became a plunderer and robber, implying the existence of a population beyond his own family;—and his building a "city," a considerable collection of habitations.

"Thus, if, contrarily to all reasonable probability, this great question should ever be determined in the way opposite to what we now think the verdict of truth, the highest interests of man will not be affected."

This seems distinct enough, and yet in a note to Dr Smyth of Charleston, Dr Pye Smith apparently takes an opposite view. He writes in reference to the treatise of that gentleman as follows:—

"This work of the Rev. Dr Smyth I have perused with much satisfaction.

He has collected a vast variety of arguments and evidences, which establish, with accumulated force, his position—the ‘Unity of the Human Races,’ as to both species and origin. I think it impossible for an upright mind to refuse acquiescence in his conclusion. With him, too, I agree that there are difficulties, as in all science, which we cannot at present remove; but weighed against the positive arguments, they cannot rationally arrest our conviction. It is to be expected that the progress of observation, and the augmentation of accurate knowledge in meteorology, actinology, terrestrial magnetism, and probably some agencies in natural history not yet thoroughly understood, will contribute much to the resolving of the perplexity. There may also have been something preternatural in a judicial infliction upon Ham. Gesenius tells us from Plutarch, that this term, in the old Coptic, denotes both *heat* and *blackness*. It might be a case somewhat analogous to that of Gehazi.”

We have thus indicated the position of parties in this controversy. In America it has been agitated for some time back, and with no little keenness, on account of the bearing which it has on negro slavery; in this country that source of prejudice would of course be removed; but Transatlantic writers have expended so much learning and industry on the subject, that if it is to be discussed here we must begin with an examination of their labours.

VARIETIES.

INDUSTRIAL MUSEUM, EDINBURGH.—The office of Director of the Chemical Department of the Industrial Museum, Edinburgh, has been conferred by Government on Dr George Wilson—an appointment which has given much satisfaction, and which, while securing that the interests of science will be attended to, also furnishes a guarantee that popular utility will be duly cared for. x

TRAINING IN THE UNIVERSITY OF EDINBURGH.—In the course of a recent discussion on university reform in the Commons, Lord Palmerston, after condemning the system of keeping boys for years learning the dead languages, said he happened to have passed three years of his life in studying at Edinburgh, and two years afterwards at Cambridge; and he was bound in frankness to say, that the information which he had learned at Edinburgh was infinitely more useful and general than what he had learned at Cambridge—(hear, hear.) Indeed the two years he had spent at Cambridge seemed to be passed in forgetting what he had learned at Edinburgh—(a laugh.) Since that time he believed that the system at Cambridge had been very much liberalised, and that the range of instruction had been considerably enlarged.

ROYAL INFIRMARY, EDINBURGH.—This institution is deficient in funds to the extent of L.10,318; and an extraordinary public meeting was held on the 5th ult., for the purpose of devising means for the liquidation of the debt. A large committee was appointed with a view to raising subscriptions by house to house visitation. The embarrassments of the Infirmary have been the work of five years, and are owing to no sudden casualty. In 1850-1 the deficit was L.2973; in 1851-2, L.2797; in 1852-3, L.1912; and in 1853-4, L.2635. A large individual subscription has kept down the gross deficiency to the above amount. It will, of course, be borne in mind, in connection with this subject, that the Edinburgh hospital receives no Government grants like the Dublin institutions, and that it has few private bequests like the London hospitals, while no price is paid for the office of governor. It is not, therefore, to be wondered at if the coffers of an institution wholly dependant on voluntary effort should occasionally be empty.

CLERICAL LECTURE ON THE LAWS OF HEALTH.—The Rev. Dr Robert Lee, Professor of Biblical Criticism in the University of Edinburgh, delivered, on the 3d ultimo, a lecture on the laws of health, in Infirmary Street Church, Edinburgh, under the auspices of the Saturday Half-Holiday Association, of a character suited to be of essential service to the community, particularly if the hints he gives were to be supported by other clergymen. There was a numerous attendance, chiefly composed of the working classes of both sexes. After showing that the adoption of every means to preserve health was a religious duty incumbent upon all, and reading extracts from the works of Dr Andrew Combe on physiology, the reverend lecturer proceeded to say that the chief essentials to secure health among the working classes were good food, cleanliness, comfortable clothing, dry and well-aired houses, frugality, sobriety, and rational amusement. Until these were attended to, the people would never be comfortable, virtuous, or religious in the proper sense of the word. The causes which prevented the labouring population from enjoying wholesome food, were whisky, tobacco, and bad cookery. If the money presently expended on whisky and tobacco were put to more legitimate uses, he was satisfied that two-thirds of the misery now prevailing would be removed. With respect to cookery, the Scotch women were as inferior to their English neighbours as they in turn were to the French. He referred to the experiment about to be tried in the Edinburgh Heriot out-door schools, and intimated that, if the attempt to teach physiology proved successful, an effort was intended to be made to open a great kitchen, where girls would be taught cookery and other matters of domestic economy, so as to fit them for being good wives and mothers. It might appear a very strange thing for a clergyman to advocate amusements for the people, but he was of opinion that, were these more general and cheap, there would not be so much wretchedness in the land. The fact was, that Scotland was the only country in Europe which was coming to be without customs. Every amusement was frowned upon; the people needed pleasurable excitement, and if they did not get them innocently, they would rush to those of a sinful tendency. The Jews had one fast in a year, and a great many holidays and feasts; whereas we had two fasts and no feasts at all. As a practical remedy for many existing social evils, he recommended the introduction into all our schools of classes for imparting a popular knowledge of the structure and functions of the body, and concluded by stating it as his opinion that a staff of intelligent medical men would be of essential service in visiting those in health—a system which had been adopted with great success during the cholera. The lecture was much applauded throughout, and a cordial vote of thanks passed to Dr Lee at the close.

TO OUR READERS.

FINDING it impossible any longer to pay that attention to the Editorial department of this Journal which it requires, we have been induced, by a series of negotiations which commenced last December, to dispose of it to our excellent publishers, Messrs Sutherland and Knox. Our editorship terminated with the March number; and to the present proprietors and editors we are indebted for this opportunity of taking leave of our readers. In now doing so, we can conscientiously declare that our sole object has invariably been to advance the scientific, improve the practical, and elevate the political

status of the profession of medicine. In this endeavour we have been most ably supported by an array of distinguished contributors, whose names are identified with the past history of the MONTHLY JOURNAL, and whose valuable communications have been translated into every civilized language, as well as embodied in the systematic medical literature of the age. But it is not we, so much as the profession at large, which ought to thank them for performing a duty whereby the medical commonwealth, and through it the public, has so greatly benefited. To Dr Mercer Adam of Dumfries, however, we beg to express our individual obligations. For the last twelve-months he has assisted us in translating from the foreign journals a *Periscope* which, as it has been principally the result of his labours, we can venture to say, is highly honourable to his literary ability and medical knowledge.

We have of late also done our best to instruct and interest our readers in the important subject of Medical Reform, the carrying out of which many recent public events have only demonstrated to be more and more imperative. We are satisfied that if the great body of practitioners who are pursuing their important vocation in our large towns and among our rural population could only be roused to the consideration and due appreciation of this subject, nothing further would be required to crush the selfish views of a few individuals, who are pressing forward their own interests in the name of our chartered institutions. How preposterous would it then appear for certain parties in our colleges to assert that they are the representatives of the entire profession, and, in consequence of this, to argue that they only should possess all the offices, honours, and emoluments which are to be obtained under a new system of organization. If we may judge of the future by the past, nothing would be more injurious to our profession; nothing would cast a greater blight on the onward progress of medicine than would be the result of intrusting such men with increased powers and privileges. At the same time it is to be wished that a termination were put to the contest now proceeding between those who demand collegiate aggrandizement at the expense of the Universities, and those who like ourselves have endeavoured to support the efficiency of our great academical institutions. At present, cordial professional intercourse both in private and at our public societies is much diminished by the petty jealousies which a party warfare has engendered, and every well-wisher to the prosperity of medicine, and of those who

really cultivate it, must desire to see the early settlement of this agitating matter.

For ourselves we have never swerved from the truth, and in stating it boldly, have performed what we conceive to be our duty. If that truth has offended any individual, we sincerely regret it. But we were not to be deterred from criticizing at this juncture, the public acts of public men and public institutions, or from exposing the results of the collegiate system, because the partizans of the latter have accused us of personality, envy, and malice. From such feelings our conscience entirely acquits us. What we have said has been in all honour and for the common good—nor have we used an expression that we are not prepared on all occasions to defend, nor put forth opinions which we shall ever be ashamed of having supported. We have not belonged to any of the parties who have brought forward exclusive plans of medical reform to benefit particular institutions, and have considered all of them solely in reference to the public welfare. On the other hand we have endeavoured, not unsuccessfully, in the pages of this *Journal*, to give increased development to medical truth in theory and practice, and we desire to see an organization sanctioned by Parliament, which shall be carried out by those who are likely to encourage its onward progress. Such, we feel satisfied, must be a necessary element in any medical reform which is calculated to secure the confidence of the thinking men among us. Let us, then, be careful to whom we entrust the task of founding and inaugurating our new medical government, remembering that whoever the parties may be, their important duties are to institute a system of proper education and examination, as well as to obtain equal privileges for individual practitioners; to give respectability and dignity to the profession as an influential body in the state, and to free it from the distracting and ignoble contentions which now disfigure it.

The Communication from Professor JOHNSTON of Durham will appear in the next Number.

Books received, and list of exchange Journals, also in our next.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Composition and Physiological Action of the Water recently used in the Durham County Jail.* By JAMES F. W. JOHNSTON, F.R.S.S.L. and E.

THE well from which the water was taken which forms the subject of the following communication was sunk in 1843, within the walls of the Durham County Jail. It has a depth of 84 feet, and is situated among the shales and sandstone of the coal measures on which the city of Durham stands. It is built inside with stone and cement, and the water at present stands in it to a depth of 21 feet. Over the water rests an atmosphere of carbonic acid gas.

The pipes formerly in connection with the pump were made of lead, but after a time these were eaten into large holes in several places, and coated inside with an incrustation which is said to have been nearly an inch in thickness. I never saw these incrustated pipes, and had no opportunity of examining the deposit formed in them. In 1851 the failing lead pipes were removed, and in their stead cast-iron pipes were introduced, which still remain.

When recently drawn from the well, the water is bright and clear, has no sensible smell, and only a very slight saline taste. When heated gently in an open vessel, it speedily becomes covered with a thin white film; when boiled, it becomes milky, and gives a white deposit, consisting of sulphate and carbonate of lime.

It appears that when this well was sunk no analysis of the water was made before it was applied to the general purposes of the prison. Its bright, clear, and sparkling appearance, no doubt satisfied those in whose province the matter lay, that it was not unfit for domestic use. For ten years, therefore—from 1843 to July 1853, when the pumping machinery went out of order—it was constantly used in the food and drink of the prisoners.

From time to time, however, suspicions seem to have arisen in the mind of the medical officer that there must be something unwholesome in this water. Certain peculiar forms of indispotion

constantly presented themselves in the greater number of the prisoners, after they had remained a certain length of time in the jail, the occurrence of which from natural causes it was difficult to account for. In such circumstances it was natural for the medical officer to suspect the presence of something unwholesome in the water which the prisoners drank, and that such suspicions did occur to him is shown by the following extract from the journal of Mr Shaw, the surgeon, which is kept regularly at the jail. It is under the date of the 21st of January 1853, exactly six months before the use of the water was discontinued.

"Examined every prisoner very minutely. The general health of the prisoners is good, but I am sorry to report that I find several prisoners have glandular enlargements in their necks. This swelling is not confined to men who are on low diet. Men in all classes are affected equally, therefore I see no reason to attribute it to the food of the prisoners. I am inclined to think that this affection is from the water, and the gas may also predispose them to it. I recommend all the prisoners to have their neckcloths put on so as to cover the neck and throat properly."

Nothing, however, was done in consequence of this entry. The enlarged necks were washed with solution of iodine, but no examination of the water was proposed with the view of ascertaining how far it was really to blame.

But in July 1853, six months after the above memorandum was inserted in the journal of the medical officer, the pumping machinery went out of order, and the water of the river Wear, filtered and supplied by the Durham Water Company, was temporarily introduced for the use of the jail. The effect of this change soon became apparent upon the health of the prisoners. In a note dated the 25th of September 1854, Mr Shaw informed me that when the use of the river water commenced, the affection in the neck immediately ceased, and now no more cases of this kind are observed in the jail than are usually met with in general practice. So striking, indeed, was the change, that the officers of the prison were not long in asking why the enlarged necks had disappeared; or, as they expressed it, why Mr Shaw did not paint the men's necks as he used to do.

With this apparent proof of the existence of an injurious quality in the well water, a return to the use of it was of course resisted by all those persons having authority in the prison to whom the above facts were known. Accordingly, when it was proposed, some months ago, to repair the pumping apparatus, it was resolved to have the water carefully analysed before any further steps were taken. This analysis was made in my laboratory with the following results.

An imperial gallon evaporated to dryness, left of solid matter, dried at 270° Fahr., ninety-four and a half grains. When dried at only 212° Fahr., the solid matter amounted to upwards of a hundred grains. This solid matter was found to consist of—

	Grains.
Sulphate of Lime (anhydrous),	31·38
Carbonate of Lime,	15·35
Sulphate of Magnesia,	4·49
Carbonate of Magnesia,	1·48
Chloride of Magnesium,	14·01
Chloride of Sodium,	6·19
Silica,	1·28
Water, retained by the salts of magnesia after drying at 270° Fahr.,	16·82
Nitric acid, ammonia, and oxide of iron, each a trace.	
Organic matter and loss,	3·50
	<hr/> 94·50

The results of this analysis show how important it was that this water should have been analysed, and they are very creditable to the sagacity of the medical officer, by whom the water was supposed to be unwholesome. Thus,

First. The quantity of solid matter which the water contains alone shows it, according to our present views, to be entirely unfit for domestic use. Of such matter an imperial gallon left—

100 + grains, dried at 212° Fahr.
94½ " dried at 270° Fahr.
77 " when entirely deprived of water.

The reason of the great difference between the first and last of these numbers, amounting to 23 grains a gallon, is found in the large quantities of gypsum and of chloride of magnesium which the solid matter contains. Of these both retain their water at 212° Fahr. Only the gypsum loses its water at 270° Fahr., while the chloride of magnesium requires to be heated to incipient redness before it becomes quite anhydrous.

At present, waters are considered already somewhat impure for domestic use when they contain from 20 to 30 grains of solid matter to the gallon. That of the river Wear, supplied by the Durham Water Company, and those of the best wells in the city, contain about 18 grains to the gallon. It was by passing from the use of a water containing 77 grains to that of one containing only 18 grains, that the good effects observed in the Durham Jail were produced.

Second. The chemical nature of the solid contents of the water had, no doubt, quite as much to do with its observed effects as the quantity it contained to the gallon.

An inspection of the numbers above given shows that sulphate of lime, or gypsum, is the predominating ingredient. Of this substance an imperial gallon contains 31·38 grains in the dry state, which are equal to 38 grains of common crystallized gypsum. The main effect which this substance is known to produce when taken *alone* into the stomach in considerable quantity, is to induce costiveness. I am not aware that it has ever been accused of producing glandular swellings.

It is doubtful, however, if any sensible constipating effect could

be anticipated from this ingredient, when conjoined, as it is in this water, with so large a proportion of the salts of magnesia.

According to the numbers above given, an imperial gallon contains as much as 20 grains of magnesian salts in the perfectly dry state. These salts, and especially the chloride of magnesium, usually exercise a laxative effect upon the system, and so far, therefore, would counteract the immediate effect of the gypsum.

Third. But the really interesting circumstance in connection with the chemical nature of these principal constituents of the water, is their probable connection with the observed swellings in the neck of the prisoners. And in relation to this point, it is curious that one of the more recent of the many local influences to which the *goîtres* of the Swiss valleys, and of our own Derbyshire mountain limestone districts have been ascribed, is the large quantity of magnesia said to be contained in the waters there employed for domestic purposes. Of course this explanation of the occurrence of the disease has not been universally received, but neither has it been shown that such a chemical quality in the waters used, may not, under certain circumstances have such an effect. And it is, at least, a remarkable coincidence that, in the jail of Durham, the occurrence of glandular swellings in the neck should appear with the use of a water containing an unusual quantity of magnesia, and should disappear when the use of this water was abandoned. It may be that the result is connected with, or is a consequence of, the united physiological actions of the gypsum and the chloride of magnesium taken together.

I need scarcely add, that my report in regard to this water was, that it was unfit for ordinary domestic purposes, and that in consequence the use of it has not been resumed in the Durham Jail.

ARTICLE II.—*On Local Bloodletting in Inflammation of the Unimpregnated Uterus.* By J. MATTHEWS DUNCAN, F.R.C.P.E.,
Lecturer on Midwifery, Edinburgh, etc., etc.

It is a very general belief, if not the doctrine of the profession, that inflammation of the unimpregnated uterus is a rare disease, and that its symptoms are of little urgency. "This disease," says Dr Churchill, "is by no means of frequent occurrence, neither are the symptoms to which it gives rise at all so marked as might be expected." Another distinguished author, Dr Bennett, observes that, "Inflammation of the body of the uterus in the acute or subacute state, is not of very frequent occurrence." These beliefs, erroneous and unfounded as we believe they are, cause obstetric practitioners frequently to fail in diagnosing this affection, and to pursue an injudicious line of treatment of it.

Of late years so much attention has been directed to the relations, pathological and therapeutical, of inflammation, abrasion and ulcera-

tion, of the uterine neck, that its importance has been in many quarters greatly exaggerated. The occurrence of the inflammatory process in the cervix uteri is undoubtedly a common and important condition. Moreover, treatment of it is imperative, and its cure frequently removes obstinate leucorrhœa, and the other local and general symptoms attendant upon it. But we are sure that in confirmed cases of uterine suffering, and especially in those where the symptoms persist after continued treatment or removal of cervical disease, there frequently exists along with the morbid condition of the cervix, an inflamed state of the body of the uterus. This is generally characterized by more or less slight enlargement and increased heaviness of the womb, which is also tender to the touch, displaced, and does not perform its functions in a natural manner. In addition, there are present more or less of the lumbar, pelvic, and femoral pains of uterine diseases in general.

The over-estimated importance of the comparatively trivial diseases of the uterine neck is not the only cause of the neglect, of late years, of the chronic inflammatory condition of the body of the organ. One important symptom and element of the affection has been seized and described as if it were its essence and the cause of all the symptoms—we allude to the displacement which, in some direction and form, almost always accompanies chronic metritis.

Inflammation of the unimpregnated uterus may assume an acute form, inducing peritonitis, or abscess, rarely in its own tissue, oftener in the region of the roof of the vagina, or in the broad ligaments. The chronic form is more common, being very frequently the cause of the long continued illness of many females suffering from depraved general health, but more particularly from pelvic, lumbar, and femoral pains, difficulty or pain in walking, in urination, and in defecation, with irregular and often painful performance of the uterine functions, and unhealthy discharges from the organ. Cases of this kind are familiar to most obstetric practitioners, and occasionally baffle their greatest care and skill. Our object in the present article is to inculcate what we believe on theoretical grounds, as well as the results of experience, to be the proper method of use of a valuable remedial agent in cases of chronic metritis.

Unlike acute inflammation of the womb in the puerperal or unimpregnated female, the affection we are speaking of demands bloodletting only with a local aim and object. Various means are in general use in a variety of affections, for withdrawing blood from the uterus and its neighbourhood. The most important of these are cupping the region of the loins, leeching the hypogastrium, or groins, or perineum, or vagina, or the uterus itself. In practice all these different modes of bloodletting are found to be applicable and advantageous in proper cases.

For the purposes of a purely local bloodletting, however, in an inflammatory case, it is not a matter of indifference which of the above-named sites is selected for the operation. In a valuable paper

read before the Medico-Chirurgical Society by Mr John Struthers, on the principles which should guide the practitioner in the use of local bloodletting in affections of the viscera, that gentleman points out that bloodletting on the abdomen is truly local only so far as the wall of that cavity is concerned, and can act on the viscera only on the principle of general bloodletting. This observation is not absolutely true in regard to the pelvic viscus, whose affections we are now considering. Cupping on the loins can, indeed, have no local effect upon the uterine circulation, the blood-vessels of the two parts being distant from each other, and having their origin from different parts of the great stem of the circulation. Leeching on the groin is scarcely a more direct form of local bloodletting than on the loins, for the only direct communication between this part and the uterus is through a series of small anastomoses of the epigastric artery, with arterial twigs descending to the inguinal opening with the round ligament, and deriving their origin from the ovarian artery, which also sends branches into the uterus to anastomose with ramifications of the uterine branch of the internal iliac. The application of leeches to the perineum or vagina may have a much more direct influence on the womb, for there are numerous anastomoses of the uterine with the vaginal arteries, and of these last with the inferior hemorrhoidal and other branches of the pudic. More distant bloodletting in various parts of the leg or foot have, in former times, been recommended as having special influence over the womb. The profession in this country, at least, has lost all faith in this treatment, as well as in the corresponding doctrine regarding venesection of special veins of the upper extremity in disorders of the head. But enough remains in the well-known, and, it appears to me, well-founded belief in the value and efficacy of the pediluvium in menstrual affections to prevent us from regarding these therapeutics as absurd; and although not dreamt of in our modern and too self-sufficient medical philosophy, yet laws of sympathy between distant parts may be discovered which will explain and inculcate some such remedial measures which now appear to be unreasonable.

In describing what appears to us the proper, although not a novel method of using this valuable remedy, we shall take opportunity to compare it with plans at present in use. This will not only give point to our argument, but perhaps be serviceable in finally disposing of those modes of local bloodletting now too frequently employed, and which are certainly sometimes very injurious both in their local and general effects.

1. *The Site Selected.*—When a local bloodletting is desired, it is of course most effectually performed when that fluid is withdrawn from the affected part. Failing that, the parts nearest and most intimately connected by vascular ramifications with the inflamed part are the best suited for the purpose. It may be affirmed, that

according as these conditions are fulfilled the bleeding will be more effectual, especially if a moderate quantity only be abstracted. When the patient is in such a condition of health that a few ounces of blood saved or lost is not of great importance, then it may be, in that respect, comparatively unimportant whether the blood be drawn from the groins, perineum, vagina, or cervix uteri. But, in the class of cases in which the remedy is of greatest service, the saving or losing of a few ounces of blood is a matter of the greatest moment. The depraved state of health and anemic condition of such patients is sometimes so great as to preclude the use of the remedy altogether. It is incumbent on us, therefore, to have the bleeding as direct as possible. A very common plan is to insert into the vagina a tube full of leeches, which are allowed to fix upon the cervix uteri or vagina, as chance directs. Of course the area of vaginal surface being far greater than that of the cervix uteri ensures the fixing of most of the leeches upon the former. This leeching of the vagina is not the most suitable, because, not being the most direct, it necessitates the withdrawal of more blood than is necessary. The bleeding from the vaginal leech-bites is also apt to be profuse and unmanageable, and thus highly injurious. The part to which the leeches should be applied is the uterine cervix, because it is the nearest to, if not itself actually, part of the inflamed tissues.

2. *The Instrument Used.*—The leeching tubes in ordinary use may be said to be applied blindly; that is, the operator has it not in his power to effect the leeching of any particular part, the instrument not admitting of his using his eyes to direct its open end. In order that the leeches may be accurately applied upon the os uteri itself, it is necessary to expose it with a speculum whose end encircles and receives the cervix. Upon this part the leeches are applied, and, if necessary, retained by a dossil of lint. When they are filled and separate, they glide easily out of the tube and are removed. By the use of the speculum for this purpose, two evils attendant upon the use of the ordinary leech-tube are avoided, for with the latter it is sometimes impossible to prevent the leeches attaching themselves low down on the vagina, or even on the vulva, where they cause pain and discomfort; moreover, some of the tubes only introduce the leeches, and these, when filled, occasionally do not come readily away, thus inducing some tedious delay or trouble in seizing their glabrous and wriggling bodies to pull them out.

3. *The Number of Leeches Applied.*—A leech bite on the external integument is, I believe, calculated generally as yielding less than an ounce of blood. When such are on the cervix uteri or vagina, they certainly yield on an average very much more; the hemorrhage, indeed, from vaginal leech-bites is sometimes alarming and dangerous in extent, and that far more frequently than from the outer skin. This may be accounted for by the greater vascularity, moistness, and heat

of the internal parts. By arrangements to which we shall afterwards allude, bleeding from vaginal and uterine leech-bites may generally be increased to almost any extent.

The unimpregnated uterus weighs about an ounce, and has a pyriform shape, with a dimension of nearly three inches in its longest diameter. To disengage the vessels of such an organ, a very small quantity of blood will be required; to maintain the disengorged state by continued bleeding for the necessary time a very minute stream of blood will be effectual. For these reasons, and on account of the patient's general condition being generally the reverse of full-blooded in the obstinate cases, the use of many leeches is much to be discommended. In cases which bleed copiously one or two leeches, and in any three or four will be sufficient.

4. *Circumstances to be attended to after the Separation of the Leeches.*—For the continued disengagement of such an organ as the uterus, I have already said that a very small stream of blood is sufficient. This is procured by the oozing which continues for many hours, and even for days, after leeching. If it is desired to encourage and increase it, this can be done by warm applications to the vulva and hypogastrium; if to repress and diminish it, then cold applications will be useful.

To derive all possible advantage from local bloodletting of the uterus, it is necessary that the female should remain for a considerable time, say one or two days after commencing the operation, confined to the horizontal position.

A plan at present frequently pursued is, after applying a large number of leeches, to place the woman in the erect position, so as to sit over or in hot water. Such a proceeding frequently causes so great a discharge of blood as not only induces fainting at the time, but prostrates the woman's general health for an indefinite period afterwards. Besides, the erect position leads to the renewed overfilling of the vessels disengorged by the leeching, and sometimes produces painful feelings of prolapsus of the parts relaxed by the operation.

5. *Repetition of the Local Bloodletting.*—In the treatment of inflammatory affections of the unimpregnated uterus, it is well known that an extraordinary difficulty arises from the recurrence of menstruation. This function is accompanied by such vascular excitement and engorgement of the womb, as has a great tendency to efface the beneficial results of the antiphlogistic treatment in the preceding interval. In most cases, indeed, of chronic metritis there is much suffering attendant on menstruation, and generally for some time after it the affection is aggravated. In these circumstances it is frequently found useful to recur to the abstraction of small quantities of blood from the uterus in the interval between the menstrual periods, a circumstance which affords an additional

reason for keeping the quantity of blood abstracted within narrow limits.

A question of great importance, and sometimes of difficulty in the cases where this remedy is recommended, has always to be solved, namely, When is the operation to be performed? It appears to us that in regard to this no very definite rule can be laid down. In some of the cases, where there is intense dysmenorrhœa, the leeches are applied before the monthly period, in order to diminish the sufferings attendant upon the function, as well as to repress the engorgement. It frequently happens that in these cases no remedy affords relief equal to that procured by bleeding, and then it may be urgently called for by the patient to anticipate and moderate her coming pains. In the generality of cases of chronic metritis, it has appeared to us best to employ the remedy in question shortly after the menstrual period is past. But the varying circumstances of different cases, or of the same case at different times, interfere with the value of this statement as a practical recommendation.

The above considerations refer only to a single arrow in the practical physician's well filled quiver against this important enemy of the female sex. But its potency and very general applicability justifies its special discussion.

ARTICLE III.—*On the Epidemic Measles of 1854 in Leith.* By JOHN BROWN, L.R.C.S. Edin., H.E.I.C.S., Hon. Pres. Hunterian Med. Soc., Edin., late House-Surgeon, Leith Hospital.

(Continued from p. 313.)

AND now having detailed the leading features of this epidemic, we will briefly advert to the state of the urinary secretion in these cases. It has been already incidentally stated that the urine was albuminous, as proved either by chemical tests or by the presence of blood globules under the microscope. On the difficulties connected with the obtaining of urine for continuous examinations in children we need not dwell; suffice it to say, that in only eighteen cases could this secretion be obtained regularly at various intervals in the progress of the disease, but in those cases in which the observations were interrupted, the results thus imperfectly obtained were not at variance with those which were more precise. In accordance with the arrangement followed in a previous part of this paper, we will consider:—1st, The secretion as occurring in cases of uncomplicated measles. At the outset of the eruption in those cases in which the body was quickly covered, an appreciable quantity of albumen was discovered by chemical tests, and under the microscope blood globules were also found, from this period, till the fading of the rash, the urine continued free of albumen, but attended by a slight deposit of urate of soda, and a density varying from 1015 to 1030,

the varying nature of the specific gravity being apparently caused by the comparative dilution of equal quantities of solids. We use the term apparently, because upon such a point in children exactitude is next to impossible. On the third day, and in proportion to the disappearance of the rash, the quantity of solids was increased by urate of soda, and albumen found either by chemical tests, or the presence of blood globules under the microscope, in no instance accompanied by an increase of epithelium, and in only one case by fibrinous tube-casts in small quantity. In two cases both brothers, and convalescent from pertussis, no albumen was found either on the third or subsequent days, but in them the eruption was remarkably persistent, and showed no signs of fading till the fifth day, when it commenced to disappear slowly. On the fourth day, in those cases in which albumen was appreciable on the third day by chemical tests, a small deposit was still found. In those cases in which the quantity was so small on the third day as only to be diagnosed from the presence of blood globules under the microscope, it was absent on the fourth day. In only one case was the deposit of albumen visible after the fourth day, and in this case the deposit was copious on the third day, absent on the fourth, and again present on the fifth. In all the cases throughout, the chlorides did not seem to be diminished as compared with normal urine. It was also noticed that in those cases in which the albuminous deposit was best marked, the recoveries were most speedy.

The urine could not be obtained for examination in any of the cases of toxæmia from sudden recession of the rash.

In the class of cases in which bronchitis supervened, 4 were examined for changes in the urine; in two cases on the 3d day, while the bronchitis was forming, so to speak, one or two blood globules were observed under the microscope, but no other sign of albumen was again detected. In one case the urine was slightly albuminous on the 3d day; on the evening of the 4th day, with amendment of the bronchial symptoms, an increase of albumen was observed in the urine, with numerous flakes of fibrin under the microscope; this appearance was diminished on the 5th day, and was not subsequently found present. In the 4th case an opalescence of the urine was produced by heat and nitric acid on the 3d day, with a few blood globules under the microscope; this appearance was not again found until on the evening of the 5th day, coincident with amendment of the bronchial symptoms, a deposit of albumen, with blood globules under the microscope was observed. In all these cases throughout an abundant deposit of urates was observed, with scanty high-coloured urine, and a diminution of the relative amount of the chlorides.

In those cases in which dysentery supervened, the urine was examined in one case on the 3d day, before the symptoms were well formed, a few blood globules were observed under the microscope; the after examination was interrupted.

In those cases in which typhoid fever supervened, no continuous observations could be made. In one case the urine was examined on the 3d day, and found to contain a few blood globules under the microscope; the meagre results in such an important inquiry were caused by the difficulty of obtaining the secretion—for in all these cases an almost total suppression of urine was observed, and that which was secreted was passed involuntarily in bed.

These observations are not so numerous as to warrant us in inferring that the same results would have been obtained had *all* the cases been examined; yet we would, from these examinations, infer as highly probable:—1st, That in cases of quickly spreading eruption albuminous urine results, and that this is probably owing to the sudden suspension of the functions of the skin. 2d, That the system accommodating itself to this change, or the skin in part resuming its functions, no albumen is found during the onward progress of the eruption. 3d, That in uncomplicated measles a critical deposit of albumen occurs in the urine on the 3d day, simultaneous with the fading of the rash, which gradually disappears; and that in proportion to the presence of this deposit, so is the recovery quickened. 4th, That when the eruption is very tardy in its disappearance, the urine was not found albuminous; and that probably the albumen is present in proportion to the quick recedence of the rash. 5th, That in sequelæ, involving the respiratory mucous membrane, the character of the secretion on the 3d day is not so decided, but that, simultaneous with the decline of the sequelæ, the albumen becomes better marked, and large quantities of fibrin are excreted. 6th, That in sequelæ involving the alimentary mucous membrane, the same characters as found in respiratory sequelæ are probably present; but that the observations made are insufficient to determine this point with precision. 7th, That in the typhoid form of sequelæ the observations are insufficient to determine the quality of the urine, but that its quantity was invariably very much diminished.

We now proceed to consider the treatment adopted in these cases. Treatment—In our description we will follow the arrangement adopted in a previous part of this paper, and consider the treatment followed in the various stages of the disease, as well as in the sequelæ, which were developed during its abatement.

1st. Treatment of the disease.

a. *Previous to Eruption.*—In all cases the patients were confined to the house; and on the evening of the 4th day a warm bath was given, and in some cases an emetic administered. Of the 170 cases only two were treated otherwise, in consequence of the occurrence of croup.

b. *During progress of Eruption.*—With one exception, a case of croup, this stage required no special treatment. In one or two cases with rough cough an emetic was given; in all the use of cold water as a drink was permitted, and the ventilation of the apartment enjoined.

c. *During subsidence of Rash.*—In cases unattended by sequelæ, nothing was given beyond, in some cases, a mild purgative, and then only when constipation existed; a practice with difficulty followed, as the old pathological notions of “peccant humours” is still fashionable among the lower orders; and the sequelæ which follow the disease are attributed by them to a want of purgatives. It may not be out of place to mention here another pernicious practice adopted by the lower orders during the incubative stage of the exanthemata generally, namely, the giving of sulphur and whisky with exemplary regularity, to children of whatever age, with the object of bringing out the eruption.

2d. Treatment in cases of toxæmia from sudden secession of rash.—In 2 cases where the pulse was perceptible, warm baths and sinapisms to the chest and abdomen were had recourse to, along with the exhibition of carbonate of ammonia and chloric ether internally. In one case (æt. 13) convulsions ensuing, with signs of determination of blood to head, as flushed face, increased temperature, strong pulsation of carotids, and a quick, strong, hard pulse; the temporal artery was opened and about 6 oz. of blood abstracted without benefit.

3d. Treatment of sequelæ of respiratory system.

a. *Of Bronchitis.*—In the inflammatory form of the disease it was observed that, simultaneous with the recession of the rash, the physical signs of bronchitis were established in most. In none of these cases was blood-letting had recourse to; indeed, the early tendency of the disease to collapse and exhaustion, along with the satisfactory effect of a less exhausting treatment, rendered it unnecessary. The treatment adopted was to administer emetic doses of tartrate of antimony every two hours until an impression was made upon the system and the disease; in those below one year ipecacuan was given in preference to antimony. If this treatment was promptly adopted, the progress of the disease in most cases was broken, and the results speedily disappeared under the use of small doses of ipecacuan and antimony. If, on the contrary, this treatment was not attended to, or if aid was not called in early, the disease progressed and terminated with great rapidity in marked collapse of the lung, with extreme exhaustion, requiring the use of stimulants, the most serviceable being a mixture containing carbonate of ammonia, chloric ether, and infusion of senega; and in all the liberal administration of wine was had recourse to upon the supervention of these symptoms. When the disease became chronic, and was attended with profuse secretion from the bronchi, marked benefit was derived from the use of blisters, applied so as to produce redness—not vesication—and applied successively to various portions of the chest.

b. *Of Croup.*—In three cases in which croup appeared before the eruption; local blood-letting, with frequent emetic doses of antimony, were used with success, along with small doses of ipecacuan and calomel in the interval; but in all but one the symptoms disappeared on the occurrence of the eruption. The case which supervened after the

recession of the rash was treated first by emetic doses of tartrate of antimony, and then of sulphate of copper; but owing to the exhaustion which ensued, recourse was speedily had to stimulants similar to those specified, when collapse of the lung was established in bronchitis.

c. *Of Pneumonia*.—In the two cases in which this supervened, consolidation of the lung had occurred before they were seen. The general symptoms were of a kind demanding stimulants, which were accordingly given; the most serviceable being the senega infusion, with chloric ether and carbonate of ammonia; blisters were also had recourse to with apparent benefit.

4th. Treatment of sequelæ of digestive system.

a. *Of Dysentery*.—In no case was blood-letting used, or apparently required—the symptoms in all but one yielding to the use of the warm bath, Dover's powder in small doses at first, and then united with gallic acid after the acuteness of the disease had abated; in one case, which was less acute than ordinary, no remedies seemed to have much effect, and the patient sunk, the disease being latterly complicated with cancrum oris.

b. *Apthæ*.—This disease speedily yielded to the influence of chlorate of potash, combined with borax locally.

5th. Treatment in those cases in which typhoid fever supervened.—The treatment was in all cases of a tonic and stimulating nature; at the outset when warrantable, warm baths were given with sinapisms to the chest, thereafter quinine, wine, and chlorate of potash frequently, but without benefit of a permanent kind, except in one case, which recovered without any complication. The cases which proved fatal from pneumonia were so rapid, that in no case could any treatment be adopted for this disease. In two cases in which diphtherite supervened, the fauces were touched at intervals with Green's nitrate of silver solution, without the slightest appreciable benefit.

It would be foreign to the scope of this paper to enter into any comparison between this epidemic and previous ones; we leave that for abler hands, contenting ourselves with taking in conclusion a hurried glance at the various points elucidated in our investigation, as well as their bearing upon the pathology of measles. To recapitulate these we find that the greatest proportion of cases occurred in those under five years of age, after which period the seizures gradually diminished.¹ We have seen that during the premonitory stage, with three exceptions, this stage was free from any complications,

¹ In a previous part of this paper this fact was considered to be owing not to the susceptibility of children in particular to the disease, but to the frequent occurrence of measles as an epidemic, so that few escape its influence. The recent observations of Dr Panum of Copenhagen upon an epidemic of measles in the Faroe Islands prove this. From his observations, he concludes that age, however advanced, in no measure diminishes the susceptibility. His conclusions are decisive, for all were ready to receive the disease, no cases having occurred there since 1781.—See *Monthly Journal*, June 1851, p. 589.

that in none was there any disturbance of the nervous system, and that in the majority of cases the eruption appeared upon the fifth day; but that in some it was delayed, and in others irregular, in the period of its appearance. In the stage of eruption it was found that with one exception the disease went on uncomplicated, and was principally noted for its characters, as affording the means of guarding against or being prepared for certain sequelæ.

In the stage of recession of the rash, we found that excluding those cases fatal from blood poisoning, the sequelæ which followed showed a marked tendency to divide into three classes—two of these, affections of the respiratory and digestive system being observed to follow a vivid eruption with accompanying inflammatory fever—while the third class consisted of a low form of fever, following an imperfectly established or quickly receding eruption. This stage was also remarkable from the almost total immunity observed in diseases of the nervous system, and of the organs of sense, while in it the whole mortality of this epidemic is to be found.

The effects of various causes of disease upon the mortality of this epidemic, have shown us some curious facts, the most notable being the varying nature of the mortality at different seasons, the influence of age and sex upon it, the negative evidence afforded by other causes, and, lastly, the effects of early treatment in diminishing the mortality.

The post-mortem examinations have principally shown us the effects of various other diseases in an extreme degree, while the results obtained in an examination of the disease itself have been of a negative kind.

It has been also shown that the urine in uncomplicated measles undergoes various changes, but that these changes are interrupted or suspended when sequelæ arise.

From a consideration of the facts which have been advanced in a previous part of this paper, it will be obvious to the reader that the leading characters of this epidemic are not to be found in the disease itself, but in the pathological states which became developed during its abatement. The mortality from this cause has been high when we consider that the mortality of measles, according to Dr West, is 3 per cent., while in these cases it was 9·7 per cent.; but we find an approximation to a similar mortality in this visitation as seen in other places, leading us to believe that this epidemic has generally been attended with a high mortality.¹

Although the disease thus proved fatal upon its decline from the occurrence of sequelæ, the characters assumed by the disease itself were so distinctive as to warn us of sequelæ of a certain kind, and thus enabled us to act upon slighter grounds than a want of knowledge upon this point would have warranted. But the experience

¹ See Abstract of Registrar General's Report for 1853. Ranking's Abstract January—June 1854, p. 277, *et seq.*

had to be gained, observations had to be made, comparisons instituted, and treatment tried, modified, or altered, so that during its progress and decline, the remedial measures adapted to the peculiar nature of the epidemic were better understood and more successfully practised.

On a careful consideration of the sequelæ which followed, we find them divided into two distinct forms of diseased action, and answering to the description of a sthenic and asthenic disease. In the first great class we found preceding it a vivid bright eruption, with concomitant fever of a sthenic type, and the evidence of inflammatory disease of the respiratory or digestive system springing up during its decline.

In the respiratory form there was observed a tendency to the rapid formation of a permanent pathological change, collapse of the lung, to an extreme degree. In these cases the remarkable reversal of the breathing movements pointed out by Dr Rees were well marked, the lower part of the chest being retracted and drawn inwards during inspiration;¹ another occurrence was also observed in these cases, namely, that upon the supervention of collapse the rales formerly abundant were found to cease, while a post-mortem examination disclosed the presence of a viscid semipurulent secretion in the bronchi; the cause of this seemed to be the want of inspiratory power, so that air was not permitted to penetrate, the respiratory murmur was consequently more harsh, accompanied by slightly impaired percussion note, but with well-marked sense of resistance. Contrary to what is observed in the bronchitis of the adult, emphysema was generally absent in these cases, which was doubtless owing to the feeble inspiratory power which in healthy children is never strong, but in disease becomes notably weak, and likewise to the yielding nature of the parietes, which permits of their accommodating themselves readily to the changes in their interior.

In the second class of sequelæ, which we have termed asthenic, the symptoms were also preceded by a peculiar form of eruption, and consisted of a general typhoid state, which we have previously described as being attended with great dyspnoea, scanty urine, pro-

¹ The discovery of collapse of the lung, and its relations to respiratory diseases, which has almost revolutionised infantile pathology, like auscultation and Bright's disease, was nearly made years ago. Reil, in *Memor. Clin.* 1792, seems to have first separated bronchitis from other lesions of the lung, and describes as occurring in 1791 a form of variola, complicated with severe bronchitis, in which he found the most formidable symptoms to be retraction of the sternum. At each inspiration he found that the sternum, especially its lower end, was, with the cartilages of the ribs, drawn so much inwards that it appeared to touch the dorsal vertebræ, and formed a depression in which the fist could be placed. This was, when well marked, always fatal, and occurred chiefly in children below six years of age. The change in the parenchyma of the lung he attributed, like many after him, to inflammation; but the accurate description of symptoms leave no doubt as to the change being collapse.

gressive emaciation, and high mortality. Although a general disease, it had local symptoms, of which the principal and most constant was dyspnœa; this symptom at first sight struck you with the idea that you had to deal with some grave pulmonary disease, but on careful and daily examination no rale was audible, the respiratory murmur being normal but accelerated—this state going on for a variable period, the shortest being eight days, the longest nineteen—the child dying quickly after the supervention of pneumonia or diphtheria. The remarkable dyspnœa which formed so prominent a symptom in this form of sequela, bore a striking analogy to that observed in the influenza of the adult; in that disease, as in the sequela we are describing, the dyspnœa is out of all proportion to the implication of the lung, and seems at first to exist without any evidence of its implication. To carry the analogy still further, we find that a formidable and sometimes fatal implication of the lung frequently results.

From a consideration of these cases, we would infer that the typhoid state was produced by the rubeolous poison circulating in the blood, and that its principal action was upon the pneumogastric, as illustrated by the dyspnœa, and subsequent changes in parts supplied by this nerve.¹

It will doubtless be considered by many that the dyspnœa may be satisfactorily accounted for in a more ordinary way, namely, as resulting from the presence of fluid in the bronchi and air cells; but our observations in these cases tend to prove that effusion into the vesicular structure of the lung was never found till shortly before death, while the dyspnœa was a constant symptom. Whatever, then, may have been the cause of this symptom, whether from a purely nervous origin or associated with a loaded state of the pulmonary capillaries, we cannot assert, but of this much we are certain, that the dyspnœa, however produced, did not result from effusion into the structure of the lung. That the dyspnœa during life, and the pathological changes observed after death, resulted from congestion of the lung, *per se*, we can scarcely think probable. If we consider the terminations of this sequela, we undoubtedly find pneumonia frequently supervening—a result which might be attributable to a long continued congestion. But again, we find that in others diphtherite was the fatal complication; so that although all the cases had a dyspnœa, which could be produced by perversion of nervous function in the pneumogastric, yet all, as would be the case, were congestion of the lung the cause, had not the lung implicated, while the implication of parts under the influence of the pneumogastric was always observed.

¹ The recent observations of Bernard upon animals, after division of the pneumogastric in the neck, present many analogies to this form of sequela. The inspirations were exaggerated, and the animals died of pneumonia. The older experiments of John Reid are similar with regard to the termination of such experiments, but different concerning the respiration, which he found to be, with a solitary exception, diminished.

We now briefly revert to the state of the urine in these cases, for we consider that its relation to the sequelæ just described, is far from uninteresting. The published observations upon the state of the urine in measles, as far as we have been able to find, are very few; indeed, they seem to be comprised in the observations of Becquerel and Simon.¹ On the characters of this urine Simon writes thus:—"In most cases it more or less resembles the inflammatory type; it is red (as in inflammatory measles), acid, and sometimes jumentous (turbid), as in gastric measles, or deposits a mucous sediment during the course of the morning (as in catarrhal measles)." Becquerel states, as the result of his observations, that the urine is generally inflammatory at the commencement of the febrile period. It becomes very dark, and of high specific gravity, and frequently deposits a sediment of uric acid; a small quantity of albumen was found in a few of these cases. During the eruptive period the character of the urine changes; if the eruption is slight, and there is not much fever, it resumes the normal type; if the contrary is the case, the urine retains the inflammatory appearance. Becquerel did not meet with any case in which the urine was turbid or sedimentary towards the close of the eruptive stage. "During the period of desquamation and of convalescence, the urine either returns at once to the normal state, or continues turbid and sedimentary for some time, or becomes pale, clear, and anæmic." These observations, however correct as to measles generally, were not found corroborated in some respects in this epidemic. The observations made as to the state of the urine during the stage of eruption were similar to our own, with this exception, that in all, the urine deposited urates instead of uric acid. The observations of Becquerel, that the urine did not in any case become sedimentary towards the end of the eruptive stage, was not found in these cases; for in proportion to the subsidence of the eruption in normal measles, was the increase of the deposit, and in all it was well marked. The presence of albumen is only noticed as an occasional occurrence during the premonitory stage; no further mention being made of it. We now in conclusion dwell shortly upon the probable cause of albuminuria in the disease, and its relation to the sequelæ which followed it. We found that in cases where the eruption appeared quickly, albuminuria occurred, a fact which seems easily accounted for, if we consider that the functions are suspended quickly, if a large excretory surface giving off water and various salts, and furnishing in its whole extent an albuminous secretion which forms cuticle, as well as the secretion found in the sebaceous follicles. We have probable evidence in support of the idea that the kidney may take upon it the functions which are thus temporarily suspended, especially when we consider the intimate connection which exists between the two organs: the absence of albu-

¹ Simon's Animal Chemistry, vol. ii. p. 269. Sydenham Soc. Translation.

minuria during the progress of the eruption may be accounted for by the system accommodating itself to the change.

We found that albuminuria was again present on the recedence of the rash, and that the quantity was greatest in those cases where the rash faded quickly, and the recoveries most perfect. This occurrence is probably owing to the sudden disappearance of a quantity of blood from the surface to the internal organs generally, but to the kidneys in particular, as superadded to them is the function of blood depuration, which at this period commences. As it appears to us, the presence of albuminuria is only of service as pointing to the perfect elimination of the morbid poison; its absence, either that other organs are taking this office upon them, or that it is not taking place at all.

When we consider the ordinary effects of cold applied to the skin, we think that three organs are most prone to suffer,—the lung, the kidney, and the intestine. In inflammatory measles we find that the depurating function of the kidney is impeded, as seen by the absence of albuminuria, and that in consequence, this function is taken by the lung or intestine, with this important exception, that while in the kidney the depurating process, so to speak, seems the normal one, in the case of the lung and intestine, the substance presented to them for excretion is one essentially different from that they are usually called upon to dispose of. It is worthy of remark that amendment of the diseases of the respiratory and digestive system was found to follow, or be coincident with the appearance of albuminuria. The facts observed with regard to these cases would lead to the probable inference that the sequelæ arose from the function of the kidney being suspended, and its functions adopted by the lung and intestine.

The facts advanced upon the condition of the urine in the typhoid form of sequela, do not warrant any decided inferences; the small quantity taken in connection with the imperfect eruption, would lead us to believe that the poison was circulating in the blood, and that efforts at elimination were probably not made by any organ. It will doubtless be considered that such views of the state of the urine in this disease warranted the trial of that class of remedies called "renal depurants," of which potash and its salts are the principal; but these observations were collecting during the progress of the disease, and their probable value only determined upon its decline; should it ever fall to my lot to witness another epidemic, their utility would be quickly tested.

In conclusion, let it be remembered that we have not here attempted to give a history of measles, but that of an epidemic, and that the deductions are made with reference to it alone, our aim having been to describe the peculiarities observed, which we feel confident will not be found dissimilar to those perceived by others who have seen this epidemic on a large scale.

ARTICLE IV.—*Notes of some of the More Interesting Cases among the Wounded from the Alma.* By JAMES JOHNSTON, M.D., F.R.C.S.E., Surgeon to the Hampshire Militia.

(*Communicated in a Letter to the Secretary of the Medico-Chirurgical Society of Edinburgh, and read on Feb. 7, 1855.*)

CASE I.—Enoch Pugh, *æt.* 29, private 55th Regiment, wounded at Alma 20th Sept. 1854. He had just crossed the river, the regiment was forming preparatory to advancing up the heights; at the moment of being struck he was standing in an upright posture. The shot by which he was wounded was of the form and about the size of an egg, and must have proceeded from the left in front of him. This shot entered the epigastrium at the edge of the cartilage of the 10th rib, on the right side of the thorax. As may be supposed, he was immediately struck down, gasping for breath in consequence of the sudden shock to the solar plexus. This breathlessness, amounting almost to orthopnoea, continued more or less for 30 hours, when it gradually abated. But to proceed—he rolled over and struggled on the ground, in the midst of which the shot fell out where it had entered, and was found in his trousers afterwards. There was not a single drop of hæmorrhage; the wound was much contused, and there was ecchymosis of the integuments covering the ribs on the right side, as far as the spine. He lay on the battlefield until the 22d, taking nothing but liquids. Several days after the wound he expectorated clots of blood from time to time. The wound finally healed in November, at Malta. The intermediate history of the case is obscure, but the treatment was the simplest. It is probable that in this case of miraculous escape the shot had entered under the ribs, pierced the margin of the diaphragm, entered the right pleura, bruising seriously, perhaps wounding the right lung, bruising but not wounding the liver, and after rolling about in the chest, most fortunately had hit upon the wound and passed out. This explanation is based on the fact that there is condensation of the right lung, with absence of respiratory murmur to about a finger breadth above the right nipple all round, and there is also sallowness of complexion from disordered functions of the liver. He has occasional breathlessness, and some pain round the chest towards the spine, in the direction in which the ecchymosis was noticed soon after the wound; there is now no external mark there. His health is wonderfully good.

CASE II.—Benjamin Pierce, *æt.* 27, private 95th Regiment, also wounded at Alma. He was struck by a ball between the tubercle and the inner tuberosity of the right tibia—perhaps rather nearer the former. This ball must have been fired from directly in front of him. The tibia was shattered in a comminuted manner by this ball, but not fractured across by it; it penetrated at right angles and lodged in the medullary cavity; little hæmorrhage resulted. Shortly after portions of the tibia were extracted, and about twelve days afterwards the ball itself. This he now exhibits; it is of the size of an ordinary musket bullet, but is cleanly cut through the centre for more than two-thirds of its diameter, and in this latter fact lies the interest of the case. It is unlikely that the bullet should have come in contact with anything capable of dividing it so exactly in its course before striking the man; a rock could not have done so. Again, had it been divided by the tibia, by coming in contact with its sharp edge, we would suppose that it would either have been completely divided, or would have lain flat on the edge of the bone, or have fallen to the ground. But we are slow to believe that after having its force expended by being three parts cut in two, it could have penetrated to the position from whence it was extracted. On interrogating the man for a better explanation, it appears that the Russians have been supposed to split their bullets in this way, in order that they may do more injury, and perhaps fly

asunder and inflict two wounds. The man stated that his impression was that the ball did not come from a musket, but from a cannon; five or six men and an officer were struck down, all around him, at the same moment as he was—all struck about the legs; he felt as if a handful of bullets had been thrown at them, in the same way as if they had been pelted with pebbles. The wound has never healed, and more dead bone has still to come away.

CASE III.—Eugene Brennen, *et.* 27, private 95th Regiment, was wounded in the height of the action in a cross fire. The musket ball by which he was struck probably proceeded from the left, in front and above him. It entered to the right of the symphysis pubis, passed across the spermatic cord, which it probably wounded slightly, and plunged into the deep seated part of the right thigh under Poupart's ligament. He was for the moment doubled up on receipt of the wound; ten minutes after, not before, there was some bleeding, and soon after much swelling of the abdomen, thigh, scrotum, and right testis, followed by inflammation. He never has had any pain in passing urine or *fœces*. Several abscesses formed, one over the inguinal ring, the other underneath in the scrotum, and there are now three fistulous openings; the testicle is quite sound. He can walk, but lamely and with pain. Searching for the ball, which has never been reached, I have once, while rotating the limb, felt a peculiar grating feeling, as of muscular fibres in action passing over a foreign body, and at the same moment the man winced. This, however, I could never again detect. However, I fancy I can grasp the ball deeply lodged under Poupart's ligament, if so it cannot be far from the femoral nerve, by pressure on which it will cause the uneasiness of which he complains. The symptoms not being urgent, it has not been deemed expedient to search with the knife, in order to extraction, for this ball; every effort, however, will be exerted to identify its position.

CASE IV.—Sergt. George Hargrave, 19th Regiment, *et.* 34, also wounded at Alma. He was on duty with the colours of his regiment. They had crossed the river and were advancing rapidly towards the heights, the ground being moist he stumbled, and it was while running to recover his position that he was struck by a musket ball, from a range of about 350 yards. He must have been in the act of doubling up his left leg, for the ball entered a little above the centre of the tibia, injuring the bone there, passing downwards under the integuments at the inner ankle, quite superficially, without damaging the joint. It was speedily cut out. He fell, and bleeding immediately ensued. The presence of splintered bone and injured periosteum have caused the cutaneous wounds to make little progress hitherto.

About three weeks ago, at Malta, two small portions of bone came away, since which he has been doing much better. Examination now still shows the existence of much denuded bone, the removal of which by suppuration is now rapidly proceeding. All the wounds are healing, except the entrance of the ball, where the damaged bone lies.

CASE V.—There is little if any interest in the case of private Martin Conway, of the 55th Regiment, whose right leg was carried away by a cannon ball, which also carried away the foot of his rear rank man. He saw the ball strike the ground in front of him; and he asserts, that the previous shot from the same gun killed an officer, and wounded a corporal next to him. The leg was amputated about three hours after the receipt of the wound, as soon as he had recovered from the shock. The stump did well, and was nearly healed, but soon after leaving Malta, which he did on the 19th December, a slight injury caused the entire flap to give way, and to slough. On arriving here, on the 3d of January, the entire flap (it was an amputation below the knee) was in a state of slough—the effect of change of air, rest, fresh diet, and the simplest and lightest dressings, was marvellous—in three days the entire surface was a healthy, florid, granulating sore

and now (the 13th) is contracting rapidly. On interrogating this and other men, they describe the hospital at Scutari as clean, comfortable, and not crowded—the attendance good, and the supplies excellent; but an impression exists upon their minds, that however well wounds may appear to heal at the time, there is something in the air or otherwise of the place, that predisposes to relapse, even after an apparent cure—they instanced cases of men who had returned to the Crimea as cured, but whose wounds opened up afresh after their arrival there. I have only alluded to this case to mention this impression, and to show how a flap nearly healed may open up three months after the operation. There is nothing in the case otherwise at all interesting.

CASE VI.—To-day (January 12) a Guardsman's arm has been amputated, in consequence of dangerous hemorrhage from a sloughing wound of the forearm. The ball had passed between the bones; he had been doing well, when some unknown cause induced sloughing; the superficial muscles and tendons were hanging in shreds; but as he retained the use of the hand notwithstanding, an attempt was made to save the limb without success, as I have stated.

CASE VII.—Charles Page, *æt.* 23, private 23d Fusiliers, wounded at Alma. This is another case of wound of the forearm, of a serious character, but I hope to save the limb with a flexed but somewhat moveable elbow-joint. A musket ball, fired close to him from the left in front, struck him while he had his firelock at the present, and was pulling the trigger. The ball entered the inner aspect of the right forearm, below the elbow-joint, over the coronoid process of the ulna, traversed the bend of the elbow, and passed out behind the head of the radius. The ulna was probably uninjured; the radius shattered in fragments; perhaps, also, the internal condyle of the humerus, the joint being seriously involved. There was bleeding *per saltem*, but the brachial or radial arteries had escaped, the pulse at wrist being normal. The median nerve, however, was divided, as thumb, first, second, and external side of third finger were paralysed; bleeding occurred at once. At Malta the wounds were nearly healed; there, however, a portion of bone was extracted, and he has been retrograding ever since, until his arrival in England, 3d January. On examination then, there were several ulcers, at the entrance and exit of the ball, and on several spots, both above and below the elbow-joint, where sinuses had formed; the ulcer, more especially where the ball entered, was ragged, angry, with everted edges; the discharge was unhealthy; the fingers, as already stated, were quite paralysed; bone was felt denuded in the direction of the radius. Simple applications, with the occasional use of *potassa fusa*, enable me to report as follows:—To-day (13th January) all the ulcers looking well; discharge less profuse, more laudable; the swelling reducing; slight motion, without pain, of the elbow-joint; and what is still more satisfactory, slight return of motion and sensation in the paralysed fingers. I look to a favourable issue in this case with confidence.

CASE VIII.—Thomas Jones, *æt.* 18, private 19th Regiment, wounded also at Alma. This man only landed yesterday (12th January). Towards the close of the action, just as he had gained the level on the top of the heights, distant about 100 yards from the batteries, he was struck by a grape-shot in the right leg. He states that he saw it strike a man some distance before him, and that he endeavoured to avoid it. It, however, was not to be dodged; it entered over the tubercle at the head of the right tibia, extensively shattered the bone, without breaking it across, and passed out below the head of the fibula, which latter bone has been uninjured. There was immediate profuse hemorrhage, arrested by tourniquet, and which ceased in about an hour entirely. When at Scutari portions of the tibia were removed; three pieces afterwards at Malta, but more since; some of these pieces were as large as an almond out of its shell. I have to-day (13th January) extracted two considerable portions, with great ease to

the patient, and I passed a probe along the course of the shot, and out at the point of exit. There is discharge of a fetid character, from the decay of men bone; there is effusion around the knee-joint, but no pain, and I have every reason to suppose that recovery will be permanent and speedy. *Jan. 14.*—I have to-day enlarged the opening over the head of the tibia, and extracted a very large portion of the cancellated structure of the head of that bone, about 1½ inches long and 1 inch broad. There was very profuse hemorrhage, which required to be checked by plugging. *16th.*—Another very considerable portion of bone has been to-day extracted; as a precaution a splint is applied to strengthen the support, so large a portion of the tibia being deficient.

CASE IX.—Samuel Nichols, *æt.* 22, private Grenadier Guards, wounded at Alma. At the close of the action had reached the level space on the top of the heights, when he was struck by a musket ball, fired from a distance of about 30 yards. At the moment of being struck he had loaded, and was in the act of priming, or rather applying the copper cap. He felt the place struck suddenly warm; however, he fired the shot; but when he attempted to move the right leg he could not do so, and had no feeling in it. The ball had struck the abdomen a little way above Poupart's ligament, one-third distant from the superior and anterior spinous process of the ilium, two-thirds from the pubis. The ball must have passed into the pelvis, avoiding the peritoneum. The bleeding was immediate (the hot feeling described was probably blood flowing); it was profuse, and recurred several times. The numbness of the right lower extremity continued several days, namely, until he reached Scutari, about the 26th of September, the battle having taken place on the 20th. Immediately after the wound, and for nearly a month, he had difficulty in making water, and pain when he went to stool, passing mucus and blood from the rectum. It ought to be stated, that he was labouring under diarrhoea previously, but had not been passing blood; his urine became very high-coloured, but he does not think that it was ever bloody. The ball has never been traced. The wound is now healing rapidly, and he passes water freely, and is quite regular in his bowels.

In this case it is just possible that the ball may have found its way into the rectum, and been passed at stool, or may be now in the bladder, although of the latter fact there are no symptoms. His health is excellent; the right lower extremity is somewhat emaciated.

CASE X.—Christopher Wooten, *æt.* 34, private 23d Fusiliers, wounded at Alma, quite at the close of the action, alongside of the batteries. He was in the act of putting on a copper cap, and was therefore standing obliquely. The shot by which he was wounded was fired from a distance of 30 yards in front of the line, and but for the attitude in which he was would have struck him in the front of the leg; as it was, it entered behind the right tibia and passed out behind the right fibula; both wounds bled immediately but not profusely. Where the ball entered was circular and regular, and comparatively a small opening; where it passed out the orifice was much larger and ragged. It was afterwards ascertained that a slice had been shaved off the bullet by the sharp edge of the tibia, portions of which had been knocked off by the blow. The portion of lead he exhibits and also the fragments of bone, which are very small, these all came away some time after the wound, in the poultices. The wound continues open, and from the profuse discharge there is every reason to fear that some extensive destruction is in progress in the shaft of the tibia.

CASE XI.—Edward Staunton, private 33d Regiment, *æt.* 21 years 5 months, wounded at Alma, about 3 p.m., soon after the heights were gained, by a cannister shot weighing about 3 ounces, and fired from a battery on his left, distant about 100 yards. At the moment of being struck he was priming, and

therefore facing partly to his left in that act. The shot struck him about the junction of the tenth rib with its cartilage on the left side, entered there and made its exit nearly opposite to the transverse process of the last lumbar vertebra, on the same side, a little above the junction of the ilium and sacrum. The course of the shot was most probably direct from the force with which it must have been travelling. The position of the man's body at the moment must have accounted for the rather puzzling course it took; when struck the man advanced a few paces and fell, not feeling pain where he was actually shot, but in the centre of the abdomen, like a severe griping, and it was only when he applied his hand to his side and felt the gush of blood that he became aware of the position of his wound. He bled much from both wounds for two days, by which most probably his life was saved. His breathing was unaffected. He made water freely next morning, and had a passage from his bowels; but a week after the wound he had great difficulty in passing urine. He now has no obstruction either in passing water or feces. Discharge from the wounds once established soon became very profuse; a portion of his trousers, and one or two small portions of bone, probably from the transverse process (perhaps from the rib), came from the posterior wound. But the striking feature in the case is that, when at Scutari, he passed feces from the upper wound, squeezed out whenever he strained at stool; this was disbelieved on his own *ipse dixit*, but subsequently the surgeon in charge of him satisfied himself by inspection. Thus then the whole thickness of the flank muscles, the peritoneum, and the descending colon, must have been wounded, adhesion round the wound in the gut must have formed, and finally, the wound there, probably slight, must have closed. I do not think any of the urinary apparatus could have been injured. In two months the wounds had healed. He is now bent towards the left side, and has a belt like a small sausage across the abdomen, very tender to touch, which I strongly suspect to be an abscess forming, and probably containing some of his clothing.

CASE XII.—George Hood, private 21st Fusiliers, æt. 38 years, wounded at Inkermann, 5th Nov. 1854, two hours after the action commenced. He says, by the by, that the surprise was all but complete enough to place the Russians in possession of our camp. When wounded he was advancing down hill, over uneven ground; had halted to fire, left leg in advance, when he was struck by a grape-shot, from 4 to 5 oz. in weight; he fancies it came from his left. This shot entered below the inner tuberosity of the left tibia, splintering, in its passage, the edge of the tibia, and passing out, perhaps, a couple of inches lower down the leg, through the thick part of the calf; a circular patch of his clothes is deficient, where the ball entered; none where it passed out; and from the clean rent in the clothes behind, the shot evidently did not carry the cloth out again; however, next day portions of two pairs of trousers came out, or were removed. But like a canny Scot, he now tells me (Jan. 26) that he has not yet recovered the missing fragment of his drawers, and sure enough I believe it is still there; at least I think I can detect some substance which may be the missing article, near the wound in the calf of the leg, which would have been healed long ago had no cause of irritation existed. I have to-day enlarged the wound, and hope to-morrow to remove it; in the meantime I have, by enlarging the wound over the tibia, removed several portions of it. At Scutari seven small portions of bone, and at Malta several more, were removed, none of them so large as those removed to-day. The tourniquet was required for four days, to check hemorrhage from some branch of the posterior tibial, after the wound.

This is the third case of wound near the knee-joint which I have given, all much resembling each other.

And now I cannot resist the temptation of setting a few minds right on the asserted neglect which these poor men and their companions in suffering and glory received on arriving here. As far as

I know, no one has ever condescended to contradict the unfounded accusations made by the "Times." That paper sadly presumes on its power, and sometimes has not the best of information. Now, I for one had nothing to do with the landing of the sick from the *Himalaya*, and scarcely knew they had arrived until sent for to dress some of the wounded. The "Times" says that they were still in the Dockyard at *seven o'clock p.m.*, now I declare that all those that were detained in Portsmouth were in bed in the General Hospital here about *four p.m.* I was dressing some of them at half-past four, not later. It is possible that some of the invalids may have been detained in the Dockyard to a later hour, but be assured that all who required immediate medical or surgical aid were speedily attended to; greater speed might have resulted in injury to the wounded, some of whom were in a most precarious state, and required much caution and care in transferring them from the vessel to the wharf.

P.S.—I had hoped to have noted some other interesting cases lately arrived, but I cannot find a moment's time; in the meantime I am anxious to add one or two later notes about the cases already in your possession. In one or two *conservative surgery* has been most successful.

Sergeant Hargrave's wounds healed, except the one over the dead bone; being now a simple case of necrosis, no interest remains.

Charles Page.—Since this case was reported last, at which time I despaired of saving the limb, the wounds have been healing, a little bone having been removed. He is recovering *motion and sensation* in the fingers, and has limited motion of elbow-joint. This has been a most satisfactory case.

Thomas Jones.—The progress of this case is well worthy of record. The grape-shot had most extensively shattered the tibia, carrying large portions of the shaft before it amongst the soft parts; daily, for a week, I removed astonishing quantities of dead bone; on the two last occasions, pieces, perhaps 2 inches by 1½, one of them lying across, towards the fibula; startling hemorrhage followed. There was now a deep black cavern, capable of holding an ordinary sized egg, and which was usually full of pus. The dead bones having been all removed, immediate improvement followed; the deep cavity is rapidly filling up; it is still, however, startling to look into it; ignorant of the danger of fracture, he is now anxious to walk about. His limb is, doubtless, safe.

Enoch Pugh.—There has been every prospect that this very interesting case would have been elucidated by post-mortem examination; but, strange to say, he is, *for the present*, rallying out of the following symptoms:—sudden orthopnea, during which he almost expired, was followed by a profuse discharge, upwards and downwards, of bright yellow frothy bilious matter, that passed by stool being frequently precisely similar to that brought up by a kind of half cough half vomit; sometimes it was so profuse as nearly to suffocate him. He was hectic, and had intense anxiety. This continued for a week, when, yesterday, Jan. 31, it nearly ceased, and he felt comparatively well; to-day, however, he again has unpleasant symptoms. The case will, doubtless, prove fatal sooner or later. The interest of the dissection will be considerable.

ARTICLE V.—On Asphyxia Neonatorum and Infantile Mortality at Birth. By Dr G. HAMILTON, Falkirk.

IN the British and Foreign Medico-Chirurgical Review for April 1853, I published a short account of the infantile mortality in my obstetric practice, from which it appeared that, while the ordinary recorded loss ranged from 1 in 17 to 1 in 46, I had lost in my last 315 cases only one child. Since that time to the present (March 2, 1855), in the whole of the labours attended by myself from the commencement, all the children have been born alive; and the numbers now stand 1 in 467, or 416 children born alive consecutively.¹ This, so far as I know, unprecedented result has been obtained mainly by the means described in the article referred to, though to some extent also by energetic and persevering measures directed to the preservation of the child when born more or less asphyxiated. As there are some points of practical importance connected with the following case of asphyxia, its narration may be interesting to the profession:—

About 10 minutes before 7 P.M., Mrs C. was delivered of a large male child. A knee presented, and to effect delivery considerable force was required, though the time occupied in passing the head through the pelvis was not very protracted. Even before one leg had passed from the vagina, meconium was voided very copiously, and when the child was born it showed not the slightest movement or trace of sensibility. The usual appliances of rubbing the breast with spirits, plunging the body alternately into cold and warm water, etc., were rapidly had recourse to, but apparently without producing any effect. As a last resource, artificial respiration was employed. In a short time a slight pulsation of the heart became perceptible, and gradually increased in strength, but this was the only motion which could be observed. After having continued the artificial respiration for an hour and a half, a slight tremulous motion of the abdomen was noticed, which at first seemed to be merely the effect of the heart's pulsation propagated downwards, but which, after another half hour, was distinctly seen to be produced by very weak and rapid respiratory movements. Along with this improvement, the colour of the lips, previously somewhat livid, became of a red natural hue. In three hours the temperature was good, the pulsation of the heart was strong, and the respiratory motions extended to the upper parts of the chest, and could be continued without the use of inflation for a much longer time than at first. The movements of the heart and chest continued in nearly the same state for another hour, when they began gradually to fail, and they ceased at 11 P.M., after the artificial respiration had been continued for rather more than four hours. No deep inspiration, or other manifestation of sensibility than has been mentioned, was observed during the whole of this period.

Though the means used in this instance did not save the child, the marked improvement that for a time took place, made me very sanguine at one period that such would have been the case; and, under more favourable conditions, they probably would have been

¹ The cases in which the children were certainly dead before labour commenced are here, of course, omitted; and the case of which the details are now given, is included among those classed "born alive."

successful. In a good number of other cases, indeed, the same means as were here employed have perfectly succeeded, and in some instances when further perseverance seemed all but hopeless. In the management of the artificial respiration a few circumstances struck me as important, and I shall therefore shortly advert to them, as it becomes a matter of the utmost moment, in such cases as this, that the most effectual means for recalling sensibility should be clearly understood and promptly used.

For convenience, in inflating the lungs, I had the child laid on the front of the bed, the arms and legs, and as much of the body as possible, being wrapped in warm flannel, which was changed at short intervals, by a person in attendance; under the back part of the neck a pledget of flannel was placed, so as to throw the head gently backwards, and the front of the neck slightly forwards, as I found that the air entered easiest into the lungs in this position. With my left hand I kept steady the head and closed the nares, and with my right I grasped the thorax, so as to be able to compress it—principally in a lateral direction. The air being expelled from the lungs by the right hand, I applied my mouth to that of the child, and inflated the lungs, at the same moment that the elasticity of the ribs was brought into action by the removal of the right hand's pressure. This procedure was regularly repeated, and a certain rhythm, if I may so speak, in the movements, after a short practice was attained, so that the fatigue, at first considerable, became afterwards greatly lessened.

As has been mentioned, the heart's action became visible shortly after inflation, in this way, had been begun. In about half an hour, however, the strength of its beat began manifestly to fail, so that I feared it was about to stop altogether. Under these circumstances I tried what effect pressure on the sternum would have, when I immediately found that the action of the heart became decidedly stronger. To satisfy myself that I was not deceived in this, I repeated the process several times with a like result, and, being assured of the fact, during the remainder of the four hours I combined the two movements described, in the following manner:—I first inflated the lungs with my right hand and mouth, three or four times consecutively, at short intervals, and then tried to imitate the motions of respiration by exerting pressure on the lower third of the sternum, and again quickly removing it, fifteen or twenty times in rapid succession. Whether the pressure on the sternum increased the heart's action by directly stimulating it, or by more thoroughly imitating the respiratory process, or partly in both ways, I do not undertake to say, but the beneficial effect of conjoining the two movements seemed to me manifest. Perhaps, by acting as I did, I, to a certain extent, imitated what the child, had it possessed a greater amount of sensibility, would have done in like circumstances; for we notice, when a new-born child is beginning to recover from a state of asphyxia, 1st, that it makes deep inspirations at considerable intervals; while, 2d, small rapid respiratory movements may be plainly

noticed, going on during these intervals. The rest, also, which this change in the manipulations allows to the operator, is a matter of some moment, where inflation has to be carried on for any length of time.

In order to satisfy myself as to the comparative efficiency of these procedures, in introducing air into the lungs, I next day got the permission of the friends to make a few experiments on the body of the child. I first tried the effect of compressing the nares and blowing into the child's mouth, after the chest had been compressed, as previously described. When this was done, even though the muscles of the chest had become rigid, the full inflation of the lungs became obvious, and the sound produced by inflation, as in using a hare's lungs, could be distinctly heard. I then fixed into the trachea a short tin tube attached to a long gutta percha tube, which latter had connected with its other extremity a bent glass tube, partly filled with water. The motion of the water, in this little apparatus, showed, with great delicacy and precision, the effect of movements impressed on the ribs or sternum, and the following are the results obtained: 1st, the largest amount of air forced from, and again taken into, the lungs, was found to be produced by pressing the ribs laterally, as I did before inflating the lungs of this child; 2d, pressure on the lower third of the sternum had a less, though still a decided effect on the air contained in the lungs; and my impression, after making this experiment is, that the much greater number of movements that can, in this way, be given to the chest, make it, *per se*, nearly, if not altogether, as powerful a means as the first for introducing fresh air into the lungs; 3d, forcing upwards the diaphragm, by making pressure on the abdomen, produced on the water in the tube little or no effect.

I have mentioned that, at the end of three hours, the pulsation of the heart was vigorous, and that the respiratory motions could be noticed to extend to the upper part of the thorax. I then began to use inflation less frequently; and, as the position of the child on the front of the bed was somewhat inconvenient, I thought I might venture to remove it to a table near the fire, on which were placed warm pillows and flannel. Almost immediately upon making this alteration, I noticed that the respiration became less distinct; and in a short time the change was so decided as to compel me to replace the child in its former position; after which, both the heart and chest began shortly to move nearly as before. Perhaps the cause of this unfavourable change may have been that, in the new position, the air did not get access to the lungs so easily as formerly, although I did not observe any very decided difference in this respect; but there was also another circumstance which I suspect might exercise some influence in producing the unfavourable change. While the child lay on the bed, the head and upper part of the body occupied a slightly dependent position; whereas, on the table, the whole body was placed horizontally. Might it not be, that, in the former position, the arterialized blood

found its way more easily to the nervous centres, and thus preserved a greater amount of sensibility? Whatever may have been the cause, the fact referred to shows how slight are the alterations, under such circumstances, which may produce important differences in the result. In this respect, I recollect, many years ago, while assisting Dr G. C. Holland in performing experiments on artificial respiration in rabbits, in connection with his work on "Animal and Organic Life," being struck with the fact, that even such a slight circumstance as occasionally altering the position of the animal, had some effect on the experiment. If, for example, while the lungs were being inflated, the action of the heart became weak, the posterior extremities had merely to be elevated to cause the heart to be immediately, for a time, stimulated into vigorous action. At the commencement of operations, in the case I have been relating, I kept this observation in mind, and perhaps the alterations of position which were at that time frequently made, assisted in keeping up the then faint action of the heart.

It is hardly necessary to say that, before beginning the inflation of the lungs, I introduced my finger into the pharynx for the purpose of clearing away any mucus which might be lodging at the top of the windpipe; and also, that the inflation itself was performed very gently, in order to avoid the risk of rupturing the air tubes. I may remark, however, that the introduction of the finger into the pharynx may have some effect in rousing the dormant sensibility in such cases, as we are by this means acting upon parts peculiarly susceptible of being powerfully stimulated. To increase this stimulus, and also in the hope that a small portion might find its way into the stomach, or be absorbed, I occasionally, during the continuance of inflation, dipped my finger into spirits, and rapidly passed it down to the top of the œsophagus.

The posterior extremities presented in this case, which can certainly be classed as but one degree removed from "still-born." The single case which, out of 467, was "still-born," had a similar presentation. These, therefore, have been, in my practice, the cases in which the greatest danger to life occurred. In both instances I used every means in my power to effect the delivery rapidly; but still more time was lost than could have been desired. Perhaps, also, the amount of traction necessary, even with the finger in the mouth, may have injured the spinal cord. Where considerable force is required, the main strain must fall upon the spinal column, and I have seen paralysis of one arm for some months produced by it, where the child survived. It has struck me that considerable assistance in making traction might be derived from the use of the "lever," passed along the face of the child, and operating on the vertex; only, that in making such a use of it, the handle would require to be somewhat more bent than in the usual forms of this instrument. I shall certainly try what assistance can be derived from it in the first suitable case that occurs to me.

Although not immediately connected with the subject of this paper, it may be interesting for me, in concluding it, to refer very shortly to the startling statement which I have made at its commencement, that I have now delivered 416 cases in succession, in every one of which (where not previously dead) the child was born alive; and that only one child was "still born" out of my last 467. Some of these children lived for only a short time, from being somewhat premature, from malformation, or from other causes, but they were all "born alive." If I am not very much mistaken, this is "a great fact" in obstetric science, and ought certainly to make those practitioners pause who have hitherto been contenting themselves with a mode of practice which has yielded a mortality of one in twenty, or even one in forty. Having explained in detail, as already stated, in the 22d No. of the British and Foreign Medico-Chirurgical Review, the practice which I have pursued in order to procure this result, I shall at present content myself with saying, that its chief feature consists in *shortening the second half of the labour process*. This I have done mainly by a more frequent use of the forceps than has hitherto been practised, the numbers delivered by them, in the 300 cases already published, having been not quite 1 in 7. In the whole 467 cases the numbers remain nearly the same, so that I have delivered successively 66 times with the forceps without the loss of a single child; while, from a statement published in the 20th No. of the British and Foreign Medico-Chirurgical Review, it appears that in 78,892 midwifery cases, the mortality when the forceps were used, in the hands of British, French, and German practitioners, was nearly 1 in 4. This discrepancy in the results is easily explained, when we notice that, while I have had to use the forceps in nearly every seventh case, other parties referred to have employed them only once in every 200, 300, or 700 cases, probably trusting instead of them, except in extreme cases, to nature and the *secale cornutum*. As a natural consequence, in a large proportion of these extreme cases, in which the forceps were used, the children were "still-born," not because instrumental interference materially increased the hazard to the child, but because it was already dead, or in articulo, before this was resorted to. In a lingering labour I hold it to be the part of a good practitioner, never, if possible, to allow his case to become "extreme;" and this, I have endeavoured to show, a correct knowledge of the mechanism of labour will, to a large extent, enable us to accomplish.

The maternal mortality in the 467 cases, I may mention, was four, or about 1 in 117, and one of these could not properly be attributed to the labour process, as the patient was in the last stage of phthisis, and almost in articulo, when premature labour came on, and death followed within an hour or two.

The two questions in obstetrics, "In an average healthy population, how often will the forceps require to be used in order to

secure the smallest amount of infantile mortality?" and "What ought that mortality to be over the whole, in a well conducted practice?" are of vast importance to the accoucheur, and to the general practitioner. I have given my experience on the subject, and although it may probably appear to some who have been accustomed to trust, in a great measure, to the efforts of nature for the accomplishment of labour, that one in seven or eight is an amount of interference with the forceps unusual and uncalled for, I can say most conscientiously that I am not aware, among the cases reported, of a single instance in which they could with safety to mother and child have been dispensed with. In the last two years of my practice particularly, during which I have now had the advantage of a pretty ample experience, I may say that I have been extremely anxious to limit their application as much as prudence would allow, and yet I find that I have still been compelled to resort to their use in about the same ratio, with the gratifying result, however, both as to mothers and children, that has already been noticed.

As a rather interesting corollary, in several respects, to the foregoing remarks, I may add, by way of postscript, the history of the following case, which has occurred since they were written:—On April 7th, at 9 p.m., I was called to Mrs J., in labour for the fourth time, the three former having been uncommonly easy. She informed me that for two days labour pains had been occurring occasionally, but that they had become frequent and pretty severe for the last three or four hours. On examination, I found that the os uteri dilated to rather more than the size of a half-crown piece, could easily be reached with the finger, but the presenting part of the head (the left parietal bone, though of this at the time I could not be very certain) could barely be felt, there being an interval of an inch and half, or two inches, between the head and the os uteri. For an hour and a half, or so, the membranes at each pain were partially pushed down into this elongated portion of the uterus, the os uteri becoming at the sametime somewhat more dilated. Thinking to bring the head closer to the os uteri, and hence to assist in its dilatation, I ruptured the membranes; but I found, after doing this, that the head and uterus still continued to maintain nearly the same relation to each other, though the head could shortly afterwards be touched more easily than before. Things remained in nearly the same condition till about one a.m. on the 8th, when the pains almost entirely subsided. I remained in the house till eight a.m., and then left, having given directions to let me know when the labour returned with any vigour. I was not called again until April 9th, at half-past two p.m., when I found that pains had been occurring at considerable intervals during all the time I had been absent, until within the last two or three hours, during which they had been frequent and brisk. On examination, I now found that the head could be reached by the finger with ease, and that the

elongated uterus, well dilated, which formerly felt thin and tense, was swollen and soft, and was still more elongated than formerly, reaching, in fact, close down to the os externum. For the next two hours, the head and uterus advanced very slowly together, until at about half-past four p.m. one ear could be felt a little above the symphysis pubis, and to the right side of it, the face of the child being towards the left acetabulum, while the anterior lip of the os uteri overlapped the os externum, and touched its posterior or perineal portion. After waiting for nearly an hour without any further advance of the head having taken place, it became clear that this protracted case (already very unfavourable as to position, from the head having to perform three-eighths of a revolution, before the face could get into the hollow of the sacrum) would shortly become fatal to the child and hazardous to the mother, from detention, if not also from impaction, unless the swollen uterus could be got over the head. After making vigorous efforts to effect this, I found that, in the present position of the head, it was impossible to do so, and I therefore lost no time in applying the forceps, in order that the head might be forced down upon the elongated uterus, and greater advantage given in pushing the latter upwards. My usual rule in applying the blades of the forceps (Zeigler's) is, to fix the first over that ear which is the easier come at. On this occasion, however, as I have frequently before experienced, when the head is high in the pelvis, the application of the first blade over the ear next the pubis, prevented the introduction of the second blade, as the latter could not be thrown sufficiently backwards to slip over the promontory of the sacrum. I therefore withdrew the anterior blade, and fixed the posterior one first, making the ear next the pubis my guide in doing so. The anterior blade was then easily fixed. Having got a firm hold of the head, I exerted a considerable amount of traction (turning the face at the same time into the hollow of the sacrum) before much impression was made. After some time, however, a sensible advance had taken place, the now elongated head pressing slightly on the perinæum. I then withdrew the instruments, and some vigorous pains coming on shortly afterwards, I pushed the uterus over the head with ease, and the child was born in a few minutes afterwards, at near six p.m., having a very short umbilical cord tightly twisted round its neck. This last circumstance, which, at an early stage of the labour, I suspected to exist, I may remark, was doubtless the chief primary cause of the detention of the head, and secondarily, of the elongation and swelling of the uterus, which necessitated the application of the forceps within its swollen neck and lower portion; and I may also remark, that I think it extremely probable from the appearance of the placenta, as well as from the discharge of some clots of blood during the latter part of the labour, that a portion of the placenta had been detached before the child could be born.

When born, the child was completely asphyxiated, and I had

therefore a good opportunity of applying the means for its recovery which have been already mentioned. Without losing a moment, the head of the child was placed in a slightly depending position on the front of the bed, inflation succeeding lateral compression of the chest under the axillæ was had recourse to, a quantity of fluid was expelled by pressure and cleared out with the finger from the air-passages and pharynx, intermittent pressure was applied to the sternum in the manner which has been described, the chest was repeatedly and briskly rubbed with spirits, and in a short time the indigo colour of the lips began to change to vermilion, and respiration commenced. It had, however, to be occasionally assisted by pressure of the chest, and inflation, for about a quarter of an hour. The child, when born, and to a considerable extent for the first quarter of an hour, was too feeble to cry, and its sensibility was too obtuse to make me hope that any benefit would arise from "smacking" the nates or soles of the feet, or from using other indirect means to excite respiration which in less severe cases are usually so effectual. All the measures adopted, it will be noticed, had direct reference to the establishment of the respiratory functions. In such a case, it should constantly be kept in mind that every moment lost before a pure circulation has been restored, rapidly diminishes our chances of ultimate success.

It will be observed also that, in the management of the first part of the labour in this case, I interfered only so far as to rupture the membranes; and perhaps it would have been better practice not to have done even this until the os uteri had been dilated to about double the size of a crown piece, which I reckon generally finishes the first half of the labour process. This is my usual practice, as I am, as a general rule, very averse to any active interference in the first half of labour; but not at first surmising the exact cause of detention, I thought the labour would be greatly advanced by bringing the head and os uteri more nearly into contact. Not succeeding in this, I immediately conjectured, from the woman's previous labours having been so very easy, that the advance of the head was prevented by the shortness of the cord, and I left matters to nature for the next forty hours, being pretty confident that no material injury would result to either mother or child until more vigorous pains had forced the head deeper into the pelvis. It will be recollected that there had been more or less of labour for two days previous to this time, but never of a very active character, so that the woman had been in labour, when I was called the second time, for nearly four days. As she was not at all exhausted, however, I felt I was safe, even at this stage, though the pains occasionally flagged, in waiting for nature, and in not using any forcing measures. I did not, therefore, as perhaps would have been considered proper by some practitioners, think it necessary to use the *secale cornutum*; indeed I now very rarely resort to the use of this medicine at all in obstetric cases, having employed it during the last two and a half years not more than twice,

and not for many years in the first half of labour.¹ It was at half-past four p.m. on the 9th that the real danger commenced, when the ear could be felt a little above and to the right of the symphysis pubis, and when the head was about to suffer compression from entering the bony pelvic circle, lessened in its dimensions by the swollen uterus. Every hour lost after this point of progress had been attained became perilous in the extreme. I waited nearly an hour without finding that almost any advance or revolution of the head took place. To have trusted longer to nature would, I consider, have been certainly to have sacrificed the child at least; while, at this stage, to have employed the comparatively inefficient ergot (or at least to have trusted to it to any extent), when the head had come within reach of the forceps, I hold to be unsafe practice. Even had this been an otherwise comparatively easy case, where the head had emerged from the uterus, I should not have felt myself justified in allowing the head to remain in the unfavourable position mentioned more than a couple of hours without interfering. Here, with the disadvantage of an intervening thickened uterus, interference became a matter of necessity, there being no probability of the revolution of the face from the left acetabulum into the hollow of the sacrum being accomplished in safety. At first, in using the forceps, I simply exerted traction; but, as the head advanced, I also turned it round, as I usually do, so as also to secure its revolution—in precisely the same mode, indeed, as the head would naturally advance from this position by the vis a tergo of labour pains. I notice, from an article of news in the 22d No. of the British and Foreign Medico-Chirurgical Review, p. 565, that on this point a controversy has taken place between Professor Scanzoni and Dr Moser, the latter of whom speaks of it

¹ It may be interesting to the profession to know that, in all the 467 cases referred to, I have used the ergot, I think, only five times, and solely in labours where the pains had first failed to bring the head within reach of the forceps; and that in none of them was blood-letting, tartar emetic, etc., resorted to with the idea of removing rigidity of the uterus or other parts. I believe such practice, from the experience I have had, to be very rarely called for, it being about twenty years since I have bled any patient in labour. The rule I have acted upon has been simply to wait in the first half of labour (using in rare instances moderate doses of T. Opii. to allay excessive irritation, or to abate fruitless pains), and to prevent by the active measures which I have described, the second half from becoming protracted. This practice, therefore, contrasts on the one hand with the late Professor Hamilton's rule, that the first stage of labour should not be allowed to continue more than twelve or fourteen hours; and on the other with the inert, and, I must be allowed to think, dangerous practice which has latterly been followed by many eminent accoucheurs, of leaving, with few exceptions, the whole of the labour process almost entirely to nature. It may be satisfactory also to state, that notwithstanding the forceps have been used so often, not the slightest local injury has happened to any of the mothers, and very rarely even the least excoriation to any of the children. I think it right to repeat that Dr Zeigler's form of forceps was used, as it appears to me that these possess several advantages over any others I have seen.

as "erroneous and dangerous in the highest degree." As far as my own experience goes, I have no hesitation in saying, very decidedly, that attention to it is of very great importance in the application of the forceps, and that, to the ease with which this manipulation enables many otherwise difficult labours to be accomplished, I owe much of my success in these instances.

On the present occasion, I found it necessary to withdraw the forceps before pushing the uterus over the head. In other similar cases, however, I have repeatedly succeeded in pushing up the uterus with the left hand while the forceps were held with the right.

The child, I may mention, continued feeble and uneasy for the first twelve hours, but afterwards did well; and the mother made an excellent recovery.

Part Second.

REVIEWS.

Obstetric Memoirs and Contributions of James Y. Simpson, M.D., etc., Vol. I. Edited by Drs PRIESTLEY and STORER. Edinburgh, 1855.

WE have frequently, of late years, had the collected writings of various physicians brought under our critical pen; and whilst most of these volumes have justified the projects of their authors in the gratifying reception they have met with from the profession, they have, at the same time, tended greatly to enhance and diffuse their reputation.

In more widely popular walks of literature this practice of publishing collected essays has been approved and sanctioned by the illustrious names of Mackintosh, Macaulay, Jeffrey, Rogers, Foster, and many others. In these instances the public mind had already long and fully appreciated the character and value of the writings, and, in some sense, called for their publication in a separate form. The authors were already well-known and well-read; their collected essays could do very little to increase their present reputation, however much they might contribute to consolidate and perpetuate posthumous fame. Scientific are not justly comparable to purely literary works in these respects. While the latter may have an everlasting value arising from sources quite independent of truth or of facts, scientific labours, unless philosophically exact, are always at the mercy of the latest inquirer. They may be at once shivered and destroyed by the artillery of the critic, and their highest glory is to have stretched so far in advance of their times as to re-

main long in use before they are consigned to the shelf to fill their small space in the historical ranks.

The obstetric memoirs and contributions of the eminent author under review are gathered together by two of his pupils, under a feeling that justice to his high reputation as an obstetrician demanded a more tangible and permanent form for his works than the loose and mutually distant pages of the British journals of medicine. The editors also believe that they will be the means of supplying many of his former pupils with papers which the latter have vainly striven to obtain, inasmuch as they were published originally in journals which are now out of print. Before entering on a brief notice of Dr Simpson's labours, we shall take leave of his editors. That they have acquitted themselves in a manner deserving of all praise we are urgent to acknowledge. Their publishers have issued the work in the form of a handsome octavo volume, beautifully printed. The numerous and miscellaneous subjects are well-arranged and classified, and the only desideratum is an alphabetical index, which we hope to find in the next volume.

This, the first volume of the work, has 857 pages. Within these limits we have no less than 81 different chapters on the same number of topics. These are classified as follows:—43 topics under the head of the Special Pathology of the Unimpregnated Female; 5 on the Physiology and Pathology of Pregnancy; 33 on Natural and Morbid Parturition. This mere numerical analysis of the multifarious researches and observations of Dr Simpson will not afford a fair and full idea of the range of subjects which have occupied his pen. In the preface are mentioned many valuable papers in medical ethics, science, and practice, as well as in antiquities, which are properly omitted from an obstetrical work. Many of the papers or chapters in the present volume consist of a few lines of summary statement, and do not deserve these names; but this is amply made up for in numerous others, which are deeply engraven with marks of their author's energy, in the widest literary research, in acute and searching criticism of the opinions of others, in original and forcible suggestions, and intrepid practice.

The great majority of the subjects discussed in the book are now, or have recently been, the obstetrical topics of the day. They are presented here, just as they were left by the author, without the very much to be desired addition of being advanced nearer to final decision. We regret that, from their very number, we cannot enter upon any of these subjects so inviting to the controversial appetites of our critical pen. They are open subjects for future obstetricians, who will find them here admirably exposed, and defended with the greatest zeal and ability.

The first part comprises topics on the Special Pathology of the Unimpregnated Female. In it we would mark for special perusal the elaborate Memoir on the Uterine Sound. The uses of this instrument are, of course, enumerated and enforced by the author of

its name and general introduction into practice, with all the zeal of an enthusiast. The chief value of the paper, in our eyes, consists of the great quantity and variety of pathological facts which Dr Simpson introduces in the course of his descriptions of the applications of this valuable instrument. The chapter on Morbid Involution of the Uterus is highly important, and is an able description of the deviations from the natural progress of the involution or diminution of the womb after delivery. Much more evidence, however, is required to prove the occurrence of morbidly excessive involution. The uterus atrophies in old age, and under some other circumstances. But the discovery of a small womb in a woman who had previously borne a child, subsequently laboured under amenorrhœa, and who died with grave and complicated chronic disease of the viscera, does not prove the existence of such a condition as super-involution of the womb after delivery. In the case described it was as likely to be the result of the amenorrhœa induced by the constitutional disease; and if the woman had recovered, might have been temporary, the organ enlarging again as restored health restored the catamenia. Besides, in this interesting case, it is also possible that the womb may have just returned to the same size and condition as before pregnancy. The girl menstruated at the early age of thirteen, and possibly, if not probably, with the small uterus of that age. This uterus menstruating at that early period thus gave indication of its ability to hold and mature a fœtus in its probably small cavity at that time. What its size was before pregnancy no one knows, and consequently no one can decide exactly whether it became morbidly atrophied after delivery or not.

In this part we have also excellent chapters on polypus, on excision of cauliflower excrescence, on retroversion of the unimpregnated uterus, on pelvic fistulæ, on the different ruptures of ovarian cysts, on the dysmenorrhœal membrane, and on the fecundity of females co-twin with males. In many of the chapters we desiderate the full and latest experience of the author, with results. We also find some terms grating on our ears as paradoxes, the results of misused words; for example, *acne* of the cervix, a part where such a disease cannot exist, there being there none of the peculiar structures which *acne* attacks; also, *hemorrhoids* of the cervix, where nothing like hemorrhoids in structure ever was seen.

Part second of the work consists of five brief chapters on the physiology and pathology of pregnancy.

The third part on natural and morbid parturition, occupies more than a half of the entire volume. A great deal of this space is filled with the author's well known and very able essays on the separation of the placenta before the birth of the child in placenta prævia, on turning as a substitute for craniotomy and the long forceps, on the sex of the child as a cause of difficulty during parturition, and a Report of the Edinburgh Royal Maternity Hospital during a singularly fortunate term of two years, when it was under the immediate

charge of the famous embryologist, Dr Martin Barry. These essays have already excited an extraordinary amount of criticism favourable and adverse. Our only intention at present, in regard to them, is to recommend them to the careful perusal of the profession, as not only excellent specimens of able medical writing, but as everywhere teeming with facts and observations of the greatest interest, important suggestions, and elaborate research.

When the second volume of the work appears, we shall return again to this notice. It is destined to contain, among others, the papers on the use of chloroform as an anæsthetic agent, which on the wings of fame have carried the name of Dr Simpson to every quarter of the globe, and to the bosoms of myriads of the afflicted.

On Injection of the Bronchial Tubes, and Tubercular Cavities of the Lungs. By HORACE GREEN, M.D. New York, 1855. 8vo. Pp. 20.

THE reader who may be startled, as we have been, by the title of this pamphlet, will be at no loss to understand the feelings with which we approach it as reviewers and critics. The judicial tone which is universally conceded to remarks divested of the weight of personal authority, must be laid aside before an achievement on which it would be absurd to suppose that any one of our mysterious brotherhood is in a condition to pronounce judgment. Even if we could conscientiously profess (which we cannot and do not) to have performed the operation here described by Dr Horace Green, President of the Faculty, and Professor of the Theory and Practice of Medicine in the New York Medical College, we should barely have had time to recognize, in one or two cases, its immediate, to say nothing of its ultimate, results. It will be more in accordance with our present views and intentions, as well as with the position held by the author, and the object and habitual purpose of this Journal, if we keep our readers informed as to the manner in which our neighbours across the water are, as usual, "going a-head" of us in the practical business of life; and vindicate to the world the claim of Dr Horace Green and some of his friends, "*as Americans, . . . to perform operations of which the conservative Englishmen and sceptical Frenchmen have never dreamed!*" We here catch a glimpse of the policy of the "Know-nothings," and might be tempted to discuss, with the author, the propriety of mixing up medical and scientific improvements with the other objects of the "great American party;" but we forbear, remembering that for this time, at least, we have sworn to sit humbly, not as teachers, but as disciples, at the feet of our transatlantic Gamaliel.

Dr Horace Green, who is sufficiently well-known to our readers as having been the first to propose the direct local treatment of parts of the air passages beyond the glottis (by means of the introduction of

a whalebone probang, armed with a piece of sponge saturated with solution of nitrate of silver), was led to the present extension of his views, and improvement of his practice, by the doubt which some "conservative Englishmen and sceptical Frenchmen" regarded his former undertaking. We cannot charge ourselves with having been in this matter, among those shortsighted persons pointed at by the author. On the contrary, in reviewing his former works in 1849 (vol. ix., p. 1081), and in 1850 (vol. x., p. 546), we have uniformly allowed the author's statements on practical points to reach our readers unaccompanied by any expressions of our opinion, except in those cases in which we were able to corroborate his experience by our own. This, too, at a time when we were well aware, that the ill-founded and loosely expressed pathological views of the author, and of some of his adherents in this country, had given birth to not a little of that specialist quackery, which is ever ready to spring up for a season under the sun of fashion, like a bed of weeds on the virgin soil of a railway embankment. We are well aware, that sound improvements, as well as novelties which are not improvements, may be attended by such an efflorescence; and if Dr Green can make good to his brethren and to ourselves his new project, we shall be well content to let him drive his engine through the tunnel of the glottis without molestation, not reckoning to his disadvantage the character of the flora which is springing up on each side of his peculiar "line."

The present extension of Dr Horace Green's operations owes its origin, as we said, to certain doubts which had arisen as to the possibility of overcoming the engineering difficulties in the construction of the original trunk-line along the trachea. M.M. Trousseau and Belloc (to say nothing of M. Bretonneau) had been accustomed, as all the world knows, to maintain a pretty considerable traffic as far as the glottis; and when it was announced from the far west that Dr Horace Green's explorations had extended "deeper than did ever plummet sound," at all events that his sponge had gone where never European sponge went before (excepting by accident); there were found some daring sceptics and obstinate conservatives who, on the strength of ten paltry degrees of latitude, fancied themselves a little "too far north" for the Professor of Theory and Practice of Physic in New York. Foolish Mr Erichsen! to declare with "as much positiveness" (sic) that the sponge "has never been passed in the living subject beyond the true vocal chords." Doubly foolish Dr Marshall Hall! to hold, and still worse to express within a thousand miles of Dr Green's office ("where he had an opportunity of witnessing its accomplishment in many instances,") the belief that Dr Green's operation could not be done, because "the passage of a sponge wet with the caustic solution into the larynx and trachea would prove fatal to animal life." To the scepticism of Dr Marshall Hall in particular, or perhaps rather to his strong and decided convictions upon the subject of reflex action, we owe

it that Dr Green, acting on a suggestion of the English physiologist, "procured several of Hutchings's flexible tubes, of different sizes, and to the extremity of one of these, thirteen inches in length, attached a sponge of the same size with those which are used with the ordinary throat probang." With this new instrument, "on the 5th of October, in the presence of several physicians," Dr Green succeeded in affording to the most sceptical among them positive proof that he had passed the sponge beyond the glottis; and Dr Marshall Hall must have felt considerably "chawed up," when, in spite of reflex action, two respectable clergymen, on whom Dr Horace Green had repeatedly passed the sponge in the ordinary way, were able, by means of the tube aforesaid, to blow out a lamp, collapse or inflate an elastic bag, and perform a number of other satisfactory and convincing feats for the edification and benefit of sceptical humanity. The names of these gentlemen may hereafter deserve to be mentioned along with that of James Phipps, the first subject of vaccination in the hands of Dr Jenner. We therefore regret that in regard to the first of these, we are not able to communicate more than that he was "an intelligent clergyman from Canada." The other was "the Rev. Mr M'Ann, the superior third of whose epiglottis could be seen easily by depressing the tongue." This gentleman deserves also to be commemorated as having nearly fallen a martyr to his zeal for illustrating the obscure truths of medical science. In operating with a tube thirteen inches long, with an elastic bag at the end of it, Dr Green happened to let the latter slip out of his fingers. "Just then the patient made a strong inspiration, when the whole instrument, sac and all, was drawn suddenly in, and for a moment disappeared out of sight. Thrusting my fingers immediately into the throat of the patient, I could barely reach, at the base of the tongue, the upper extremity of the bag, which I seized with my thumb and finger, and drew out the whole together." Dr Green contemplates this little mischance in the true spirit of a philosopher and a man of science. Referring to it he says:—"An incident which, had the tube been shorter, might have proved an accident, is an *additional proof of the position of the instrument.*" We shudder to think that the *incidental accident* thus coolly contemplated might have proved, in the person of the Rev. Mr M'Ann, an *incident* (*in*, *into*, and *cado*, I fall) which would have demonstrated to all Europe as well as America, the position of the instrument, by connecting Dr Horace Green's name with a verdict of manslaughter.

Hitherto we have seen the "catheterism of the air passages" performed, not with the expectation of any new benefit to the patient, but for the satisfaction of Dr Marshall Hall, and other sceptics as to Dr Green's earlier operations. We do not doubt that they came away fully satisfied. Dr Green now, however, determined upon a new venture, which occurred to him in the form of the following questions: "What shall now hinder the introduction

of medicinal agents, through this tube, into the lungs, or directly into the bronchi and their terminations? What will prevent the injecting even of a vomica under favourable circumstances, with appropriate remedies?"

No sooner said than done. Dr Green had not to wait long for "favourable circumstances." On the 13th of October he injected one drachm of a solution of nitrate of silver (gr. xl. to ʒj.), into the "left bronchial division" of a lady from Connecticut, in the advanced stage of tubercular consumption, and having, according to several experienced auscultators, a large excavation in the apex of the left lung. We give the account of the sequel in the author's own words:—

"No cough whatever, or any sense of suffocation, was produced by this operation, nor did the patient observe in the least the ordinary bitter taste of the solution. A few minutes after the operation she stated that she 'felt a warm sensation' in the upper portion of the left lung, but no pain, or any unpleasant feeling whatever, followed the operation. Mrs A. did not return to have the operation repeated until the 17th, four days afterwards, when she stated that for twenty-four hours after the use of the injecting tube, her cough and expectoration were both greatly diminished, that she had breathed with more freedom than before: that these favourable symptoms had continued, though not as marked as at first up to the present time. She was therefore much disposed to have the operation repeated. The tube was again introduced through the trachea its entire length, and at this time one and a half fluid drachms of the solution were thrown into the lungs. The immediate results were the same as at first, but after some minutes, she began to cough, and expectorated easily, and at once, nearly two ounces of purulent matter, changed in its colour and consistence, apparently, by its immediate contact with the argentine solution. Indeed, the expectorated matter presented precisely the appearance which is observed to take place with the purulent matter of an external ulcer when cauterized with the nitrate of silver. This changed condition of the expectoration was observed by several physicians who were present when the operation was performed. The relief which followed this last operation in Mrs A.'s case was still more marked and decided than in the first instance. Her cough she stated was much relieved, the expectoration yet more diminished, and her breathing was easier. A pain in the chest of which she had complained was removed; and during the two nights which followed the operation her sleep was better than it had been for a long period before. Mrs A. remained until the 26th, during which time the elastic tube was introduced into the left bronchial division seven times, and on each occasion from one and a half to two drachms of a strong solution of the nitrate of silver were injected into the lungs. Her improvement was constant. She grew stronger, and gained flesh in this period; but, being obliged at this time to return to her home, she left with the intention of coming back to renew the treatment in a few weeks.

"The same day on which I succeeded in introducing medication into the air-tubes of the above patient, I commenced in like manner the treatment of other cases, and since the thirteenth day of October, there have been treated for a longer or a shorter period, thirty-two patients labouring under tubercular or bronchial diseases, by the direct introduction into the lungs of a strong solution of the nitrate of silver injected through the elastic tube. Of these thirty-two cases, nineteen showed unequivocal physical signs and symptoms of tuberculosis in the different stages of the disease; complicated, many of them, with bronchial inflammation. Thirteen of the number are cases of chronic bronchitis, the disease in some of them being of many years' standing. Of the nineteen cases of tuberculosis, nine of the number presented, on auscultation, the usual signs of the presence of tubercular cavities in one or both lungs. All these cases of thoracic disease, with

one or two exceptions, appear to be benefited, some of them greatly, by this method of topical treatment."

We conclude our extracts with the following testimony of "Dr Pittard, an intelligent and experienced physician from North Carolina, who came to this city (New York) to be treated for a severe and long-continued bronchial disease":—

"In compliance with your request, I give you a simple statement of the effect of an injection into my lungs of the nitrate of silver. The application of the remedy caused a considerable glow through the chest, which was felt for several hours. There was no irritation produced on the bronchial membrane, by the introduction of the fluid; but, on the contrary, the cough was suspended, or greatly moderated for a day or two.

"It may be said that the injection passed into the stomach, instead of the lungs. This may have been possible in some other instances; but in my case there could be no doubt of its having entered the air-passages, for you will recollect that the breath was passed out through the tube, as soon as it was inserted, which may be considered conclusive evidence; besides, I tasted the nitrate of silver in the matter expectorated for twelve or fifteen hours after the administration of the remedy."

Our readers are now in possession of the principal facts adduced by Dr Horace Green in this pamphlet. We are content simply to narrate them. We trust we have

"Nothing extenuate,
Nor ought set down in malice."

That the operation proposed by the author is *practicable*, we see no reason to deny; whether it will prove *useful*, or even *justifiable*, are points on which we have grave doubts, but on which we do not feel bound to offer any opinion at present. We cannot say that we feel ourselves strongly moved, on the sole authority of Dr Horace Green, to repeat experiments fraught with so many possible "incidents" and "accidents," on all our "cases of thoracic disease, with one or two exceptions;" yet Dr Green does not inform us as to the principle on which he selected his cases for this treatment, but leaves us to suppose that it is applicable to all cases of tubercular and bronchial disease indifferently. We must take the liberty of remarking, that the history of the "one or two exceptions" above alluded to, would have been no small addition to the value of his pamphlet. It is impossible to avoid a suspicion that a gentleman who views with such remarkable *sang-froid* the narrow escape of our friend Mr M'Ann from being throttled for the benefit of science, may have allowed these opposing instances (to which he only alludes in the above half line) to weigh too lightly in the balance of his judgment. Dr Green is an enthusiast. It is his boast, as an American, to demonstrate *what is possible*, rather than to separate with a discerning glance and a cautious hand what is good from what is bad in practice. The latter is the task of the "conservative Englishman."

Well, be it so. We are willing to take the catheterism of the trachea as a *fait accompli* though that of the individual bronchial tubes seems to us somewhat to lack evidence. This done, there comes the question, "cui bono?"—for whose benefit? That of the patient, or that of the physician? We wish to avoid prejudice, but we cannot help regarding the attempt to heal a tubercular cavity by irrigating or scalding the whole surface of the bronchial mucous membrane with a solution of nitrate of silver (gr. xl. to ʒj), with much the same feelings with which we should view a proposition to cure an ulcer on the leg, or an obstinate sinus in the groin, by a general shower-bath of the same material.

The Pathology and Treatment of Leucorrhœa. By W. TYLER SMITH, M.D., etc. London, 1855.

HAVING had much satisfaction in perusing Dr Smith's memoir on uterine and vaginal discharges, as presented to the Royal Medical and Chirurgical Society, we have looked forward with desire to see the present work, which is founded upon the researches above-mentioned. The volume is of very great value; it contains matter of importance which is really original; although for the most part the anatomical, microscopical, and chemical observations introduced into the work have been previously recorded. The author deserves much credit for having, with great care and intelligence, investigated the subject of his work for himself, described it distinctly, and impressed its importance upon the professional mind.

The first chapter contains an account of the minute anatomy of the mucous membrane of the uterine neck and of the vagina. The villi of the mucous membrane covering the vaginal portion of the uterine neck are carefully described and beautifully figured, as also the peculiar mode in which they are surrounded and covered by scaly epithelium.

The second chapter is a very full account of the abundant glandular structures in the cervical canal, and of their arrangement. This is the part to which Dr Smith desires to attract attention, as the common seat and source of leucorrhœa. The anatomy, function, and disease of this important part are indeed the whole theme of the book. It appears to us, however, that by using certain terms he has come to believe, certainly very erroneously, that it has been neglected by previous authors in their writings on that affection. If he will turn to the work of Dr Henry Bennet, for example, he will find it very particularly insisted upon that the mucous follicles, or glands, of the cervix are frequently the seat of disease. The researches of Dr Smith are valuable and important so far as they are positive observations of structure and function, or descriptions of leucorrhœa; but, in pathology, the chief point in his views is the

translation of *inflammation and abrasion of the uterine cervix* into *cervical leucorrhœa*. If the glandular structure of the cervix be secreting an excessive quantity of mucus, and that mucus be not natural in its characters, if this be accompanied with swelling, pain, and tenderness; if in addition, there be present, redness of the vaginal part of the cervix, denudation of the villi of the same part, or ulcerative destruction of them, then, surely, in all these cases there is no error in speaking of inflammation and abrasion. To our author, nevertheless, we are indebted for an unprecedentedly scientific and accurate description of these conditions.

Since Dr Bennet's last edition appeared, but before Dr Smith's work, the Croonian Lectures of a distinguished Fellow of the College of Physicians in London, on ulceration of the os uteri, had been published, but without our author having given them sufficient attention. If he had done so, he would certainly have assigned a much more modest scope than he does, to the pathology of affections of the uterine cervix. The man who wishes to make a decided advance in the pathology of the ordinary uterine ailments, must have studied carefully the recent works of Henry Bennet and of Tyler Smith, but must take the conclusions, negative as they are, of Dr West (whose work we lately reviewed), and with these as his text and stand point make progress in this difficult investigation. The work before us is, indeed, born a little after its time, but is therefore less valuable or welcome. It is the finishing, or topstone, to a pillar which has been much looked at and discussed during its erection. As far as our present means and appliances can go in the investigation of leucorrhœa, so far they have nearly gone. This is the last step.

In the third chapter the healthy secretions of the genital passages are ably and admirably described. The sebaceous follicles of the vulva secrete an acid, oily, odorous matter. The glands of Duverney are believed to secrete in sexual excitement an abundant fluid, which is discharged at the ostium vaginæ; it forms the female semen of some ancient authors. The vaginal canal is coated by a rather scanty, acid, milky fluid, consisting of a hyaline plasma containing abundance of scales of the flat or squamous epithelium of the passage, with detritus of the same. The cervical canal is generally filled with transparent brilliant viscid secretion, of a yellowish tint, and alkaline reaction, and consisting of a plasma containing mucus globules. In the lower part of the cervix this secretion of its numerous glands may contain squamous particles derived from the os uteri, or that part of the vagina with which it may be in contact, and the acid secretion of these latter parts sometimes destroys the transparency of this part of the plug of cervical mucus, by coagulating the albumen in it. In pregnancy the lower part of this plug becomes often indurated, white, and tough, and Dr Smith seeks to found upon this fact, and our power of seeing it through the speculum, a new sign of pregnancy. In speaking of this he forgets to estimate

its value as an indication. It must be worthy of very inconsiderable confidence. For, it is often absent in pregnancy; it is often present without pregnancy. Lately we removed an enormous plug of this white inspissated mucus from the hypertrophied cervix of a non-pregnant womb. The plug of pregnancy is dry, tough, and incapable of solution, and comes away with or before the show, which consists of fresh mucus poured out consentaneously with the advance of the healthy relaxation of parts. The plug itself cannot be said to form the show.

The fourth chapter contains an excellent description of two forms of leucorrhœa; the first vaginal in its source, and called epithelial from its containing abundance of the squamous particles of the surface of that canal; the second cervical in its origin, and called mucous from its consisting essentially of a fluid containing mucus globules, but often containing epithelial scales derived from the vaginal surface over which it passes.

"In cervical or mucous leucorrhœa, the glandular portion of the canal of the cervix uteri is the chief source of the discharge. This form of leucorrhœa is, when simple and uncomplicated, the result of a morbid activity of the glandular cervix. A follicular organ, which should only take on an active condition at certain intervals, becomes, from a variety of causes, constantly engaged in profuse secretion. Instead of the discharge of the plug of mucus at the catamenial period, an incessant discharge is set up. This discharge, it cannot be too often repeated, is a special glandular secretion, elaborated by the glands of the canal of the cervix uteri. In the first instance the leucorrhœal discharge consists of nothing more than an unusual quantity of the elements found in the healthy mucus of the cervical canal. Quantities of mucus corpuscles and oily particles, with particles of epithelium entangled in the viscid alkaline plasma which gives the mucus its clearness and consistence, are found. The clear mucus is seen at the os uteri, sometimes adhering to the os itself, at others extending through the vagina, and presenting at the os externum in the form of a string, and also lying upon the walls of the vagina, in the curdy or creamy state to which it is reduced by the action of the vaginal acid. The presence of oily matter in the discharge from the cervix is constant, and so is the presence of occasional particles of scaly epithelium, which, as I have before remarked, appears to ascend from the vaginal portion of the os and cervix. I should state that, in obtaining matter from the cervix for microscopical examination, I have always used a bivalve speculum, free from grease or oil, dilating the os uteri as much as possible by the expansion of the valves, in order to get the secretion of the cervical canal without the admixture of any vaginal mucus. In making an examination in a case of cervical leucorrhœa of recent origin, when the disorder consists merely of a hyper-secretion of the mucous follicles, without any manifest lesion of structure, the cervical discharge hanging at the os uteri, or adhering to the vaginal portion of the os uteri, is almost always viscid and transparent. It may be drawn out with the forceps as a long tenacious string of the utmost clearness. The chief exception is in cases of pregnancy, where, from the highly acid condition of the vaginal mucus, the lower part of the plug of mucus is whitened and curded before it is exit from the os uteri."—Pp. 54, 55.

"In vaginal or epithelial leucorrhœa, the seat of the discharge is in the muco-cutaneous lining of the vagina, and the portion of this membrane reflected over the external surface of the cervix to the margin of the os uteri. In strictly vaginal leucorrhœa, there may be no discharge whatever issuing

from the canal of the cervix, and in some cases the secretion of the cervix seems almost suspended, the os uteri appearing drier than natural, and no mucus being visible between the labia uteri. In others, the cervical glands are excited by the condition of the vagina, and secrete copiously, a mixed epithelial and mucous leucorrhœa from the union of the two kinds of discharge being the result. The discharge in vaginal leucorrhœa may arise chiefly, either from the lower portion of the vaginal membrane, or from that part which is reflected over the cervix; but in severe cases the whole surface of the vagina is involved. The secretion in these cases generally consists entirely of epithelium in every possible phase of development, mixed with acid mucous plasma. A portion of the secretion diluted with a little water, and placed under the microscope, is seen to consist of myriads of epithelial particles, in the form of mere nuclei, young scales which have not reached their full development, and perfect scales. If the case be acute, there are no old and broken scales, such as are found in the healthy secretion, the epithelium being separated too rapidly in the formation and flow of the discharge to admit of their coming to maturity and wearing away in the vagina. In mild cases, when the separation is more slow, ripe and well worn scales are sometimes present. When the vaginal form of leucorrhœa becomes very severe, the villi become affected, and not only is epithelium separated with extraordinary rapidity, but pus is formed upon the irritable sub-epithelial or villous surface, which when mixed with the epithelial matter can hardly be distinguished from the mucus corpuscles of the cervix mixed with scaly epithelium. The state of the vagina, as seen by the eye, will, however, remove all doubt as to the nature of the discharge in these cases. A further complication of vaginal leucorrhœa may occur, as when portions of the vaginal surface are so abraded that blood globules escape and mix with the other constituents of vaginal discharge. The vaginal secretions now described are those most commonly found in vaginal or epithelial leucorrhœa; but there is another form of vaginal discharge which deserves consideration. In that already mentioned, the secretion consists of epithelial matter thrown off from the surface in such a state of separation that the scales are in a confused mass, the fluid portion being exuded from the vessels of the villi or papillæ below the epithelium. But in the second form of epithelial disorder, to which I now refer, the epithelium is thrown off in large shreds or pieces, in which the pavement-like arrangement of the scales is perfectly preserved. These laminae frequently have upon them marks of the rugæ of the vagina, and somewhat resemble the cuticle, in cases of acute desquamation of the surface of the body. The under surfaces of these masses are also rough from the indentations of the vaginal papillæ. Sometimes, on making a specular examination in these cases, the whole surface of the vagina is seen covered with a white coating, which may be removed by a forceps in membranous pieces of considerable extent and thickness. This affection may be attended with a slight discharge from the sub-epithelial surface; but in many cases the vagina does not contain more secretion than usual, or it may be unnaturally dry. In all epithelial affections of the vagina the discharge is acid; but the acidity is particularly marked in this—the membranous form of leucorrhœa, as it may be termed. Some of the instances in which I have seen this affection in its most marked form have been in cases of pregnancy. I have sometimes had patients bring me a mass as large as a walnut, consisting of pieces of the epithelial coat of the vagina rolled up like paper; or I have seen a tumblerful of water rendered perfectly thick with the quantity of shreds removed from the vagina by a single injection.”—Pp. 57–59.

Of the accuracy of these observations it is easy to satisfy any one. But Dr Smith must be cautious lest he carry his microscope into regions where it cannot stand. In severe cases of—call it leucorrhœa, or inflammation, or abrasion of the cervix, or what

you will, there will be mixed with the discharge many pus globules, which are notoriously very difficult to distinguish from mucus globules. We might then suppose a glass given to the microscopist, having in the field some scales old and young, with mucus and pus globules, and it would be a sad puzzle to him to say what was the seat of the disease. The microscope is very useful in its own place. Its uses in practice in cases of leucorrhœa are *nil*. In all cases when the distinction between cervical and vaginal discharges is possible, it will be more securely done by the naked eye than by aid of the microscope.

The fifth chapter, on the sequelæ of leucorrhœa, is one of the most valuable and interesting. It shows beautifully the progress of the leucorrhœa of Smith, of the inflammation of others, from abrasion of the epithelium and laying bare of the villi, to ulceration or destruction of the villi, and lastly of the subjacent tissues. The last result of the irritation is hypertrophy.

These first five excellent chapters contain almost all that is positive in the book, and certainly form the chiefly valuable part of it. In the next two chapters our author enters upon the subject of leucorrhœa as a phenomenon in secondary syphilis, and discusses the question of the communicability of secondary affections. No new point is established, and little additional confirmation given to old views, except the expression of our author's well-weighed judgment.

In the eighth chapter the ovula nabothi are described, and reasons given for believing that they are not obstructed follicles of the cervix uteri. The necessity of distinguishing various appearances confused under this name is well pointed out.

The ninth and tenth chapters enter at length into the usual concomitants of leucorrhœa. Their chief fault lies, as we have already pointed out, in ascribing too much to the affection of the cervix. Of this our author appears himself to become occasionally conscious. For after describing the connection between leucorrhœa and sterility, he takes care to overthrow all he has said, by adding that no difference can be discovered between that affection in women who breed well and in those who are sterile, an observation which should have led him to see the unsatisfactory nature of his original conclusions. The connection of leucorrhœa with disorders of menstruation are equally incompletely given, and the author ignores altogether the only real form of membranous menstruation.

The work ends with two very good chapters on the causes and treatment of leucorrhœa. It is a book which ought to be in the hands of all interested in obstetrical studies. The whole profession must be gratified to see the true spirit of science carried into the troubled questions in regard to leucorrhœa, and to observe the great progress recently made in the investigation.

On the Use of Creasote in Scorbatic Camp Dysentery. By JOHN BRAMSTON WILMOT, M.D. London, 1855. 8vo, pp. 16.

THIS pamphlet describes an epidemic of intestinal disease which occurred in 1844, under the observation of the author and of Mr Dakins, in the Union workhouse at Pembury. The facts on which it is founded were laid before the Medico-Chirurgical Society of London in 1845, and are now republished, we presume, *apropos* of the epidemic among our soldiers in the East. There are many minor points which would demand criticism in Dr Wilmot's observations, were we disposed to enter on a detailed discussion of them. Thus, "scorbatic" dysentery is a misnomer, or, at least, implies a totally unsupported hypothesis; "camp" dysentery is improperly used as a term synonymous with epidemic dysentery. Many points in the descriptions, particularly of the *post-mortem* appearances, are confused; and we might even raise the question whether a disease in which the lower part of the colon was always unaffected, while the ilium and cæcum were disorganised, was, in the strict pathological sense of the term, dysentery at all. We are almost tempted to think that a typhoid fever of peculiar type may have been under the author's observation, although the symptoms described are unquestionably dysenteric. Be this as it may, we feel bound to say, that the author's statement carries internal evidence of an honesty and candour, which would give to his observations a very high degree of importance, were they somewhat more numerous; and that we have perused them with something more of a disposition to repeat them than their mere number would have led us to anticipate. There is nothing impracticable or absurd in Dr Wilmot's therapeutics; and we think that the injection of creasote in drachm doses, with a pint of gruel or starch, into the bowel in dysenteric affections, is a suggestion worthy of a more extended trial. According to the author, it caused in some cases a good deal of temporary irritation; in others, and these the worst, "a tingling sensation, but no pain." Probably a much smaller quantity, suspended or dissolved by means of diluted acetic acid, and administered cold in the form of frequent large enemata, would be a preferable plan to that of the author. We think it due to him, however, to give the summary of results according to his own experience.

"I have thus given a true and unadorned account of the cases treated with creasote. The numbers were but few. For the sake of illustration, though not of humanity, I could wish they were more numerous. Every one was successful; no death occurred after the practice was adopted: and let it be remembered that, in order to test its efficacy, I purposely avoided directing it in any but the worst cases, where the ordinary remedies had been continued to the very verge of safety, and had proved ineffectual. I am as satisfied that these cases were saved by the plan, as I am morally convinced that some of those who died before it was adopted, would have been restored.

"The number in the Union-house who were attacked by this epidemic was thirty-four :—seventeen males, seventeen females. The deaths were eight :—four males, four females. The cases were not all of equal severity. There were besides, four other inmates, who died with dysenteric symptoms; but they were extremely decrepid and advanced in years, they sunk in the very earliest stage of the complaint, and could scarcely be returned as dying of dysentery."

Biographical Sketch of the late Dr Golding Bird. By Professor
BALFOUR. Edinburgh, 1855.

THIS sketch of Dr Bird's history and character was originally prepared as an address to medical students, and delivered as one of a series of lectures at the instance of the Edinburgh Medical Missionary Society. The professor seems to have been led to the choice of his subject, as an eminently suitable one for the occasion, by the circumstance that Dr Bird's life, "while it encourages the student in his aspiration after professional eminence, and inculcates industry and perseverance as the means of advancement, warns, at the same time, against overmuch exertion, points out the vanity of earthly distinction and fame, and shows by a living example the value of those heavenly blessings which are alone enduring." Much of the lecture is occupied by quotations from letters written by Dr Bird's friends, illustrative of various points of his remarkable character. Some of these show how singularly versatile, as well as active, were his intellectual powers. It appears, that after his health began to fail, during his occasional residences on the coast for the benefit of change of air and relaxation, he set himself very earnestly to the study of marine zoology, although he had not previously been conversant with the subject. The same diligence and acuteness which enabled him to produce the very valuable works on practical medicine, which laid the foundation of his fame and fortune, were brought into play even in his hours of recreation, and while labouring under fatal disease. And it is deeply interesting to learn, that in the observation of the habits, and the examination by the microscope, of the structure of the lowest forms of animal life, he experienced the greatest refreshment and pleasure.

Brief as this memoir is, Professor Balfour merits the gratitude of the profession for its publication; and we hope that it may be made very useful amongst that interesting class for whose benefit chiefly it has been prepared.

Part Third.

PERISCOPE.

MEDICINE.

ON THE ORIGIN OF RETRO-UTERINE HEMATOCELE.

M. Langiar read a communication on this subject at the meeting of the Academy of Sciences of Feb. 26. After adverting to the imperfect state of our knowledge on this disease, he entered at some length into several interesting questions connected with it, and concluded with the following remarks as the result of his investigations:—

1. The spontaneous evolution of the ovule is, as has been alleged, an occasional cause of retro-uterine hematocèle.

2. The physiological state of congestion in the ovaries during this spontaneous evolution, and the persistence of the opening in the Graafian vesicle, do not occasion retro-uterine hematocèle.

3. To produce this there must exist an increased degree of congestion, sometimes occasioned by accidental causes, during, or a few days after, menstruation. Abortion is not an immediate cause of hematocèle, as has been erroneously supposed.

4. It is especially the recurrence of this spontaneous evolution which gradually increases the volume of the hematocèle.

5. The ovarian vesicles successively opening into the cyst of the hematocèle remain open there, so that the ovary is destroyed by a small number of spontaneous evolutions taking place in the condition which that organ presents at the commencement of hematocèle.

6. The rupture of a Graafian vesicle affording a passage for the blood which escapes from the ovary, the cyst of the hematocèle will be most frequently intra-peritoneal.

7. Spontaneous evolution of the ovule and hematocèle have one character in common, namely, pain situated in one side of the abdomen, and the seat of which is the ovary where the vascular evolution occurs.

8. The rut may occasion ovarian congestion in animals, and may be followed by rupture of that organ, that is to say, by consequences simulating retro-uterine hematocèle.—*Gazette Médicale*, March 10.

RETRO-PERITONEAL CANCER.

Dr Stokes laid before the Society the morbid parts, illustrative of retro-peritoneal cancer, taken from the body of a man who died in the Meath Hospital on the previous day, and made the following observations on the case.

The patient, as far as we know, has been subject for about two years and a half to a wasting diarrhoea; but we cannot say what was the nature of his first attack. He came into hospital under the care of my colleague, Dr Lees, in the early part of last spring. At that time he was greatly emaciated, and had been nearly a year labouring under the diarrhoea. He exhibited all the symptoms of extreme anemia; he was colourless, and singularly white—to a degree that I do not remember to have seen equalled even in young females. He had also a venous murmur in the neck, and a murmur in the carotids, and these were accompanied with a doubtful venous pulsation. The patient presented also at the heart a loud bellows murmur, which predominated at the apex,

and diminished towards the base of the organ. This bears directly on the question of diagnosis between organic and functional disease, and is on the account a point of great interest in the case. The abdomen was slightly enlarged. I did not see the patient at the first period of his admission, but when he came under my notice he had ascites, and I am inclined to think there was abdominal effusion throughout. He complained of pain under the false ribs and on examination a tumour could be detected under the margin of the liver. This tumour was very variable, both with respect to size and to the ease with which it could be felt: these circumstances evidently depending on the condition of the intestines. During the whole of the long period in which he was in hospital there was scarcely any change either in his symptoms or in his appearance. After repeated examinations of this man, I came to the conclusion that he was labouring under a cancerous disease. He had certainly no external appearance of this affection; but he had that collection of symptoms which lead to the diagnosis of cancer. In the first place, the diarrhoea had resisted every attempt which could be made to check it; and, though the stools were diminished for a time, they afterwards returned, and were established with the same persistence as before, which indicated that the cause of it was owing to some organic lesion. We felt satisfied, too, that the tumour in the abdomen was not a tumour of the liver; for, between the tumour and the natural situation of the edge of the liver, there was a deep sulcus to be felt. As the disease proceeded, the finger struck down on a tumour of extreme hardness, and at one time it was considered whether this might possibly be the liver in a state of cirrhosis. We concluded against this, first, because we had ascites preceding the existence of the tumour; and secondly, I was led to the opinion by the recollection that in the great majority of cases we have enlarged spleen; but we found no enlargement of the spleen in this case, and therefore, we concluded that the ascites was not the result of cirrhosis. Having then a hard tumour with ascites, but no enlargement of the spleen, we came to the conclusion that it was a case of cancer.

I may now mention another circumstance in connection with the circulatory system. The bellows murmur, which was well marked last summer, latterly disappeared, so that the heart's sounds were presented without murmur. The *bruit* in the carotids had likewise ceased; but I am not prepared to say that the venous murmur had entirely disappeared; with respect to the state of the heart, however, there is not the least doubt. Now, presuming the murmur to have been anæmic, while the original cause of disease went on increasing, the question arises why the murmur should have disappeared when the patient was every day advancing in disease.

On opening the abdomen we found that the peritoneum contained a large quantity of serous fluid. The deposit which you see here appears to be that form of cancer described by Lobstein, under the name of "nodulated," and which is formed in the retro-peritoneal cellular tissue exclusively, and infringing little on other parts. In this case we find it converting the cæcum into a hard irregular mass, but the intestinal canal is perfectly pervious. The term nodulated is very well applied to this form of cancer. The scirrhus masses are of great hardness, and fixed in the omentum, and of various sizes, from that of a grain of duck shot to that of a bean; you see them in some places forming grape-like clusters, and all in the retro-peritoneal tissue. A very interesting fact, to which I beg to draw attention, is, that while there was this mass of disease in the cæcum, the ileum leading down to it, so far from being dilated, was the most narrow and contracted ileum I had ever seen, and was not, in fact, large enough to admit anything bigger than the little finger of a child. The mucous membrane of the colon presents a singular appearance. It is mottled with dark lines running in a transverse direction, and is marked by numerous white lines; while at the same time it has a gelatinous appearance, and seems as if portions of it had been the seat of old ulcerations which have cicatrized, without having been afterwards provided with villi. On pursuing

the examination, we found the spleen extremely small, and a mass of cancer just at the insertion of the vessels, pushing the organ before it, but the spleen is itself healthy. The same thing occurred in the liver. The lungs were free from disease, and the kidneys presented a similar condition with that of the other organs of the abdomen.

The last observation which I shall make is in connection with the state of the heart. There is no valvular disease, and I had no reason to doubt at first that the murmur in the heart was anything but anæmic, but so far from predominating at the base, it predominated at the apex. It is asserted that murmurs at the apex occur only in connection with organic disease. I have long believed, however, that this doctrine is not to be trusted, and this case illustrates the correctness of that opinion, for here we had functional murmur occupying the same situation in which it is found in mitral disease. The heart, moreover, is contracted on itself, and very small. I think it probable that this condition of the heart existed for a considerable time before the patient's death, and was the mode which the heart took of adapting itself to the diminished stream of fluid it had to transmit. Can it be that to this concentric hypertrophy, which the heart has undergone, the loss of the murmur is due. If this be so, it is a new fact in the history of anæmic murmurs, and a point of no small interest in cardiac pathology. The heart appears to me to have adapted itself to the diminished quantity of blood it had to discharge; and the fact itself throws considerable light on Dr Corrigan's theory, which attributes anæmic murmurs to flaccidity of the vessels.—*Pathological Society of Dublin.*

FOETID ABSCESS OF THE LUNG FROM PRESENCE OF A FOREIGN BODY.

Dr Hughes exhibited the trachea and lungs of a patient who was admitted into Jervis Street hospital five weeks ago, complaining of great debility and constant cough, which was attended with foetid expectoration. He was generally wasted; had almost complete anorexia, a rapid feeble pulse, and night sweats.

Anteriorly the physical examination gave comparative dulness over the left side, with resonance of the voice, and bronchial breathing at the apex of the lung. Posteriorly there was muco-crepitating râle from the angle of the scapula to the base. The right side was normal.

The crepitating râle disappeared in some days. But there was no improvement in his symptoms, and little change in the physical signs, until ten days before his death, when well marked signs of a cavity under the left clavicle were present.

From that period the expectoration became more profuse, and he rapidly sank.

On a *post-mortem* examination we found adhesion of the pleura around the apex of left lung. This portion was solidified, and contained a cavity in which was found a small fish bone. (Dr Hughes then showed a fish bone about one inch and a half long, sharply pointed at one end, and curved. It was one of the bones of a plaice.) The cavity also contained some purulent matter, and communicated with several others which penetrated through the centre of the lung to its base.

The right lung was healthy: there was no trace of tubercle in either. The mucous membrane lining the trachea and left bronchus appears of a dusky red colour, and is softened. The œsophagus is perfectly healthy, and all the other organs were found in a normal condition, save the liver, which was large.—*Pathological Society of Dublin.*

CASE OF ABSCESS OF THE LIVER OPENING INTO THE GASTRO-DUODENALIS ARTERY, AND ALSO INTO THE PERITONEAL CAVITY AND DUODENUM. BY DR M'DOWELL.

It is well known to practical physicians that there are various ways by which nature gets rid of the contents of a hepatic abscess.

1st, The abscess may burst externally ; in some cases this has occurred a high up between the ribs as to resemble closely an "empyema of necessity;" in others, on the contrary, so near the umbilicus, and consequently so remote from the hypochondrium, as to remove almost all suspicion of its hepatic origin.

2d, It may pass through the diaphragm, and open into the pleura, or into the lung, so as to be evacuated through the bronchial tubes.

3d, The pus may escape into the cavity of the peritoneum—a rare occurrence ; or,

4th, Into some portion of the intestinal canal—the stomach, duodenum, or colon, most frequently.

5th, The abscess may open into the gall-bladder, and the matter escape through the biliary ducts into the duodenum.

6th, The abscess may open into the vena cava ; or,

7th, Into the pericardium.

8th, In a last group may be included those cases in which the abscess makes its way in two different directions ; thus, it may open externally through the parietes, and at the same time into one of the hollow viscera.

In a remarkable case described by Dr Graves, an hepatic abscess communicated with the stomach by three openings, and with the pericardium by a fourth.

The following case belongs to the last group, and furnishes an example of an hepatic abscess opening—1, into the peritoneum ; 2, into the duodenum ; 3, into the gastro-duodenalis artery.

CASE.—The patient (Richard Stanley, 53 years of age) was a man of intemperate and dissolute habits, who, for many years, had suffered from a tight stricture of the urethra.

I first saw him on account of a violent and sudden attack of hematemesis. He had lost, literally, an immense quantity of blood, part of which was red and florid, the rest being black and coagulated. He was thoroughly blanched, cold, and almost lifeless. There was, at this time, a circumscribed hard tumour in the epigastrium, of a rounded form, and tender to the touch, which pulsated strongly, and over which a loud systolic *bruit de soufflet* existed.

The man was apparently in a dying state ; however, under the use of stimulants, etc., he rallied, the hemorrhage from the stomach ceased, but much dark melenic matter was passed from the bowels. The next evening a quantity of pus (about a pint) was discharged *per anum*, and for several days pus in smaller quantities continued to be passed.

After this the patient regained somewhat his strength, and for a month enjoyed tolerably good health, when he was again suddenly seized with hematemesis, and lost a very large quantity of blood in a few minutes. But, although the hemorrhage did not recur, the consequent prostration and debility were so extreme, that for a day or two his friends almost hourly expected his death. Nevertheless, he gradually rallied, and two days after the attack of hemorrhage, passed, as before, a large quantity of pus by stool. He now became a patient of mine in the Whitworth Hospital, and was under observation for twenty-two days.

During this period he occasionally passed purulent matter of a healthy character, but did not again lose any blood from the mucous surface. The waxy, pale hue of his countenance, however, plainly denoted how serious had been the loss of vital fluid in the two attacks of hemorrhage already described.

The tumour in the epigastrium, which the patient now stated had existed for several months, presented the same characters as before, but could now be traced under the ribs on the right side to become merged in a general enlargement of the liver.

It soon became evident that in this case the system was incapable of rallying ; the prostration continued to be extreme ; the feet and legs became anasarcaous, troublesome diarrhoea set in, the scrotum became gangrenous, and

death finally ensued from exhaustion, two months after the first attack of hematemesis.

Post-Mortem Examination of the Body.—The abdomen contained a good deal of serum, and pus likewise flowed from the peritoneal cavity at its upper part. The liver was much enlarged. The first portion of the duodenum adhered to the lobulus quadratus, and, in this situation, a circular opening, an inch in diameter, communicated between the intestine and an abscess which had extensively excavated the corresponding portion of the liver. By a second and smaller perforation, the abscess had opened into the cavity of the peritoneum.

The head of the pancreas was thickened and enlarged. The gastroduodenalis artery, in its course behind the first portion of the duodenum, had been opened into, but was now filled by a firm and lengthy coagulum.

The stomach and spleen were healthy. The kidneys had undergone degeneration; the left lung was tuberculous.

Remarks.—The occurrence of hematemesis in connection with hepatic abscess is most unusual. I am not, indeed, aware of any case, except the one now detailed, in which it has been observed.

This symptom was calculated to render the diagnosis more difficult, by directing attention to the stomach rather than to the organ primarily affected.

We observe also, that we have here an instance of blood of a florid colour, constituting the fluid ejected from the stomach by vomiting. This is to be explained not only by the direct arterial source of the hemorrhage, but also, probably, by the fact of a larger quantity of blood being poured into the stomach from the duodenum, than its acids were capable of altering in colour. It is also deserving of notice how much the external tumour possessed of the characters of an abdominal aneurism—as, for example, in its strictly median position, and in its possessing both impulse and *bruit de soufflet*. It is not a little remarkable that, although an artery of such magnitude as the gastroduodenalis was opened, death was not directly owing to hemorrhage. Twice the reparative powers of the system closed up the opened artery, and the last time so effectually, that a firm lengthy clot, similar to that which results from the application of a ligature, was found to have sealed up the vessel to some distance on either side of the ulcerated opening.

The communication between the abscess and the duodenum would appear to have been somewhat valvular, as the discharges of pus were occasional and intermittent.

The opening into the peritoneum must have occurred very shortly before death, as there were no traces of recent inflammation in the peritoneum.—*Dublin Medical Press.*

ECZHYMA ON THE FOREARM OCCURRING SUBSEQUENTLY TO ATTENDANCE ON LABOUR CASES.

Two instances of this description are reported. The first is that of M. Godefroy, Professor of Midwifery at Rennes, in whose case the eruption showed itself during the night following an accouchement, where, after a tedious labour, the patient was delivered by him of a still-born child, the operation of turning having been practised. A sanious and foetid fluid had been escaping from the vagina during labour, and at the time of delivery the child was found considerably decomposed; but the operator was not suffering from any wound or abrasion at the time, and had taken the precaution of carefully washing both his hands and forearms upon the case being completed. The second case is reported by M. Guillemant, physician at L'Huis. This also was an instance of the same eruption occurring on the forearm after attendance upon a protracted accouchement, where the woman was finally delivered by embryotomy. The medical man in this case had also washed his hands and forearms immediately after the labour terminated, and asserts that he was conscious of no wound or scratch being present at the time. Nevertheless,

during that day, symptoms began to manifest themselves, which speedily went on to the establishment of regular ecthyma, which for two months resisted all treatment. In both these instances, with the exception of difficult labour, the women were in perfect health.—*Gazette Médicale*, March 17.

M. BAIZEAU ON THE TREATMENT OF CROUP BY LARGE DOSES OF TARTAR EMETIC
(PAR LA DOSE BASORIENNE.)

The frequency and fatality of this disease, confer an additional value on any remarks suggestive of improvement in the measures adopted for its cure. With this view the subject appears to have been more fully investigated in France than in this country, and several important innovations in the method of treatment have originated there, as the result of this system of inquiry and observation.

Among these the employment of tartar emetic in large doses was suggested, as M. Baizeau allows, so far back as 1839, by M. Bazin; the same remedy was also proposed, but not employed by Laennec; and in the *Dictionnaire des Dictionnaires*, under the article *Croup*, we find it recommended by M. Fabre. Notwithstanding the opinion of such authorities regarding its value as a remedy in this disease, the matter seems to have been neglected; and it is with a view to reviving this mode of treatment that M. Baizeau has published his interesting communication in the *Gazette Médicale* for March 10th. Several cases are there cited by him as examples of the successful treatment of the disease by this method, and others are referred to where similar results have been obtained.

The success attending the employment of tartar emetic in croup, M. Baizeau ascribes to the contro-stimulant action of this substance, and not to its effects as an emetic, believing that its influence here is somewhat analogous to the apparently specific power exerted by it in other diseased conditions of the respiratory organs.

However, the vomiting necessarily arising from such doses as are recommended by M. Baizeau, can scarcely but be considered as materially assisting if not in some cases superseding the other effects of this remedy; the dose in different instances varying from six to twelve grains of tartar emetic in four ounces of water, and a teaspoonful of this to be given every two hours.

In combination with ipecacuanha, it is the emetic usually employed in the Hôpital des Enfants Malades at Paris, and only in those cases where it fails in producing its effects are others resorted to; the object being, that whatever emetic may be employed, the vomiting arising from it should be energetic and repeated, as would result from the doses given by M. Baizeau. And in support of this doctrine, M. Valleix has shown that out of fifty-three cases of croup, only one cure resulted in twenty-two of these cases where emetics were given sparingly; while fifteen recovered out of thirty-one cases in which they were administered more freely.

M. Baizeau, however, asserts that the absorption of this remedy into the circulation subsequently to the cessation of vomiting, and the fact that its contro-stimulant properties are thus brought into operation, afford the true explanation of its success. For, says our author, it generally becomes more difficult to excite vomiting in children after a few successive doses of any emetic, and thus as the remedy is longer retained by the stomach, the chances of its absorption are increased, and in this case its contro-stimulant action consequently becomes more active.

We do not clearly see, however, the manner in which M. Baizeau reconciles this theory with such statements as that of Barrier, who mentions that from numerous facts it is proved that emetics, among which he mentions tartar emetic, are efficacious only in those cases where they are followed by vigorous and continued vomiting; or with the twenty-two cases of M. Valliex, quoted above, in which the vomiting was mild, and in which only one recovery took place. This apparent discrepancy, however, is at once removed, if we recog-

nise the efficiency of tartar emetic as an emetic properly so called, and as at the same time, a powerful contro-stimulant, the combined actions mutually contributing to account for its success. In this way, along with its rapidity of action, and apparently specific powers, its superiority in such cases would be immediately apparent, as possessing advantages which are not to be found in any other remedy.

Although M. Baizeau has thus attached, perhaps, rather little importance to the occurrence of the vomiting produced in the first instance by this substance, his paper in other respects is a good one. And while the number of cases treated in this manner has been too small to justify any decided general opinion as to its merits, at the same time many facts in connection with these cases, would at least induce us to place more confidence in this remedy, and encourage its further trial by medical men.—*Gazette Médicale*, March 10.

SURGERY.

RESECTION OF THE HEAD OF THE HUMERUS ACCORDING TO A NEW METHOD OF OPERATING.

At the *seance* of Feb. 26, M. Baudens read before the Academy of Sciences, a memoir on this subject. No part of the skeleton, says M. Baudens, is better adapted for resection than the head of the humerus, and nowhere has the operation been attended with more encouraging results. In fourteen cases where he substituted resection for scapulo-humeral amputation, thirteen cures have been obtained. Referring principally to gun-shot injuries of this part, as our author appears to do, he states as the conclusion to which his experience has led him, that resection, when a ball has fractured the head of the humerus, ought to be the rule, and amputation the exception.

When the resection has been limited to the head of the humerus, he adds, that he has always obtained a re-establishment of the movements of the arm; the only difference being, that the new articulation consists in a ginglymoid instead of an arthrodial joint. The points most necessary to be attended to in order satisfactorily to obtain this new articulation, are to maintain the humerus in the most immediate possible contact with the glenoid cavity, and to interfere as little as possible with the muscular fibres and nerves.

The methods of operation, based upon the formation of a flap, are rejected by M. Baudens, and the simple incision adopted; not, however, according to the manner laid down by White, as this M. Baudens considers to present almost insurmountable difficulties to the surgeon. Instead of making as he did, the simple incision on the outer side of the shoulder, or in imitation of Percy, Larrey, etc., at the middle part, M. Baudens makes it on the inner side, and assigns three reasons for doing so, viz., 1st, the head of the humerus is at that point more superficially placed than anywhere else; 2d, we can expose it in its full length, by prolonging the incision in the space between the acromion and coracoid processes; 3d, the four muscles inserted into the tuberosities are more easily reached by the internal incision, which permits us at the first to reach the bicipital groove. These four muscles, the tendons of which are apt to be confounded with the capsule of the joint, must be cut at their insertions, for two additional reasons; first, to overcome the force which they exert in causing the head of the humerus to rise under the acromial arch; and, secondly, because these tendons being once divided, the capsule is itself, *ipso facto*, sufficiently opened for allowing a passage to the head of the humerus; and it will thus be seen that, contrary to the general statement of authors on this subject—it is sufficient, instead of cutting at first the articular capsule, merely to divide the four above mentioned tendons on the two tuberosities of the humerus.

M. Baudens then goes on to state that other considerations, which have hitherto been overlooked, are suggested by the experience of fourteen resections

of the head of the humerus, performed by him, and of which consideration the following is given as a short summary :—

1st, What limits must be assigned to resection of the head of the humerus? If this extremity be simply splintered by a ball, removal of a segment of it is sufficient.

2d, When the fracture extends from the head of the humerus towards the diaphysis of the bone, within the medullary cavity, is it a case for amputation?

We answer no; in full reliance on numerous facts occurring in our experience.

3d, When a ball has entered the head of the humerus, is resection indispensable?

When it is not resorted to, one of three results takes place; the patient either dies hectic, or has to undergo resection after all, or survives with an ankylosed limb, and suffers from fistulous openings and unceasingly renewed complications.

4th, Immediate resection ought to be preferred to that performed after any lapse of time.

This statement is supported by the following statistics—eleven immediate resections have afforded ten cures and one death. Fifteen wounded, treated by expectant practice, gave the following results :—Died from hectic, etc., 8; subsequent resection with cure, 3; surviving with fistulous openings, 4.

5th, How do the articular movements become re-established?

The articulation reproduced after the operation, differs from the old one, as in place of an arthrodial joint, we have always a true ginglymus formed—and this ginglymus is so much the more powerful according as the deltoid, pectoralis major, latissimus dorsi, and teres major muscles, have been preserved in greater or less integrity.—*Gazette Médicale*, March 17.

PARTICULAR METHOD OF APPLYING CAUTERIZATION FOR THE RE-UNION OF ANOMALOUS FISSURES, AND ESPECIALLY THOSE OF THE PALATE. BY J. CLOQUET.

His remarks are confined to the treatment, by this method, of divisions of the velum palati, and he proposes in such cases to take advantage of the great amount of retraction which occurs in the cicatrix consequent upon burns.

We need not in such cases, says M. Cloquet, cauterize the edges of the fissure throughout their whole length, converting them into a granulating sore, the cicatrization of which must be afterwards assisted by sutures, appropriate bandages and the maintenance of correct adaption. This method, long known to the profession, sometimes succeeds, but often entirely fails. The one which I propose, adds M. Cloquet, consists in applying the cautery to the angle of the fissure, and that only to a limited extent; leaving the contraction of the cicatricial tissue to operate, and then, practising a similar cauterization, and waiting for some time to renew the application in such a way as by repeated operations to bring the edges of the division towards each other, and to unite them by a succession of cicatrizations which may be regarded as so many successive points of suture. The double advantage is thereby gained of being thus enabled to watch, step by step, the results of the treatment, and to obtain unions of the most difficult nature by an operation, simple, scarcely painful, and exempt from all danger. It is especially in fissure of the palate that M. Cloquet considers the advantages of this mode of operating as incontestible, and he mentions four cases in all of which the operation had been attended with equally successful results. There had been no pain felt, no change in habits or regimen necessary, and no complications had arisen. The operation was of the most simple nature, every surgeon could perform it. It required the aid of no assistant, an advantage of great importance in country practice; and, lastly, it could be practised on very young children. One objection urged against this method was the length of time required for completion of the cure, but the slowness of its action constituted its safety, and the inconvenience arising in

this way was very small, as the patient experienced no alteration in his health or habits during its progress.

The cauterization may be effected by two different means; namely, either by caustics or the actual cautery. M. Cloquet states that in the first of those cases where he attempted this method, he used, as the cauterizing agent, the acid nitrate of mercury, and succeeded completely. However, he prefers the actual cautery, its action being deeper, almost instantaneous and consequently less painful, while it occasions a more firm cicatrix, and one which becomes more rapidly organised. The three other patients were treated in this manner, and the results obtained confirmed his opinion on this point. An almost insurmountable obstacle to its employment might be, however, occasionally met with in the terror of the patient. But, fortunately, science provides us with a means of obviating this inconvenience; as a platina wire introduced within the mouth, before the electric circuit is completed, cannot excite the patient's alarm, and as it can afterwards by this means be brought to a white heat, and be kept incandescent for any length of time, the surgeon is enabled to act with all the calmness and precision desirable.—*Gazette Médicale*, March 3.

LITHOTOMY; SECONDARY HÆMORRHAGE; THE ACTUAL CAUTERY.

BY MR ERICHSEN.

The case was that of a man from the country, 44 years of age, and of feeble constitution, but looking much older, who had suffered from stone for several years. He was cut on the 31st of January, and a broad, flat, lithate of ammonia calculus, two inches by one and a half, removed also by the scoop. There was no hæmorrhage of any consequence at or after the operation; but the patient complained of a good deal of pain in the wound, and fell into rather a low state. Stimulants and tonics were freely administered, and the patient went on well till the ninth day, when, as he was straining to pass a motion, very active arterial hæmorrhage suddenly occurred from the wound. The house-surgeon attempted to arrest this by plugging, but did not succeed in stopping it. Mr Erichsen coming to the hospital to make his visit about half an hour after the occurrence, cleared out the wound, and found that free arterial bleeding was occurring from its upper and anterior angles. He immediately introduced a tube and plugged the wound methodically around this. The hæmorrhage, however, continued, and it was evident that more efficient measures must be taken in order to arrest it. The tube and plugs were accordingly withdrawn, the patient brought to the edge of the bed, and chloroform having been administered, and the lower part of the wound carefully protected with wet lint held down by a spatula, the actual cautery was applied deeply towards the upper and anterior part of the inside of the wound. This immediately arrested the hæmorrhage; but, after four days, when he was straining to pass urine, which came by the natural channel, an inconsiderable oozing occurred. This was arrested by plugging the wound with lint soaked in alum solution.

With reference to the secondary hæmorrhage, Mr Erichsen said that it was difficult to account for it, except on the supposition that the wound having been attacked by a low, hospital, sloughy form of inflammation, of the kind that had been prevalent in the hospital of late, a vessel had been opened, and thus bled freely. He remarked that he had seen several instances of secondary hæmorrhage after lithotomy—once in a private patient of his as late as the fourteenth day; but in all these cases it had either cured of itself or had been readily arrested by plugging. In the case under consideration, however, these means were quite inadequate; the hæmorrhage was very profuse, and would soon have exhausted the patient. Under these circumstances, there was no alternative but to apply the actual cautery. This might appear at first sight a hazardous means to be employed deeply in the wound near the neck of the bladder, but it had been followed by the best results, and had not been attended

by the slightest inconvenience to the patient. Mr Dalrymple, who was making the visit of the hospital, stated that he had known it used by his father and Mr Martineau at the Norwich Hospital with complete success.

In plugging the wound in order to arrest bleeding, Mr Erichsen remarked that there were two points to be looked to. One was to keep a tube in the wound and a plug around this, so as to avoid extravasation of urine; and the other, to see, although no blood might escape externally, that the bladder did not slowly fill with coagula, as would sometimes happen.—*Lancet*.

VERICO VAGINAL FISTULA.

M. Jobert has brought under the notice of the Academy of Medicine at Paris, an interesting case of this affection, in which, by his auto-plastic method of operating (separation of the vagina at its junction with the neck of the uterus), he had obtained a complete cure.

The circumstances of the case were as follows:—The patient was a girl 15 years old; by accident during a fall, a pencil had been introduced within the vagina, and penetrated into the bladder, where it speedily became encrusted with lithates and formed the nucleus of a very large calculus. Some months afterwards this was extracted, by the vaginal operation, by a country practitioner, and, although the case was successful in other respects, the fistulous opening (for the cure of which M. Jobert had now operated) remained as the consequence.—*Gazette Médicale*, March 10.

ON THE EMPLOYMENT OF CHLOROFORM IN THE MILITARY HOSPITALS OF THE EAST.

After the recent remarks of Dr Hall, condemnatory of the use of this remedial agent in the hospitals at the seat of war, it affords us much gratification to observe the manner in which its merits, as an *adjuvant* in surgical operations, are regarded by our allies in that quarter, and it is with the view of showing in what estimation it is held, and how nearly its mode of administration, in the French hospitals, approaches that lately recommended by Mr Syme and others in this country, that we subjoin the following remarks, extracted from the proceedings of l'Académie des Sciences, as reported in the *Gazette Médicale* for March 17, 1855:—

M. Mounier states regarding this subject, that, having had six months' experience as surgeon in chief at the hospital of Dolma Bagtché, at Constantinople, during which period he had recourse to this agent many thousand times, and in slight as well as in very serious cases, he has the satisfaction of being able to announce that the inhalation of chloroform was universally attended with the most complete success.

The apparatus which he employed for the purpose of exhibiting the drug was extremely simple—consisting of a cone of paper, wide enough at its base to include the mouth and nostrils, and truncated at its apex with the intention of allowing a certain amount of air to enter at each inspiration. A small quantity of "charpie" was introduced within the cone, and twenty or thirty drops of chloroform poured upon this, and having previously bandaged the eyes of the patient, and enjoined strict silence, the apparatus was applied—nearer and nearer the face as the anæsthesia manifested itself—until sensibility appearing absent upon pinching the skin, and intelligence upon repeating questions, the silence of the patient was regarded as the signal for commencing the operation. If the operation were a tedious one, a second or third supply of chloroform was added, which was inspired each time similarly to the first dose, alternately approaching and withdrawing the cone from the mouth of the patient during the first few seconds of inhalation.

Such was the method employed in the case of all the wounded brought to this hospital after the battles of Alma and Inkermann, and not only was there immunity from any fatal result, but there was not even any untoward symptoms demanding treatment. The safety and efficiency of chloroform as an anæsthetic, M. Mounier mainly ascribes to its method of administration, that

method being in accordance with the theory advanced by M. Flourens, viz.—that this agent acts in a progressive manner, and that different stages of anaesthesia successively exhibit themselves under its action; first intelligence, then sensibility, and lastly locomotion, being brought under its influence, or—to speak anatomically—its effects are observed first upon the cerebral lobes, next upon the cerebellum, and afterwards on the spinal cord. According to a number of experiments made under his own direction and superintendence, M. Mounier is of opinion that there is no necessity for continuing the administration of chloroform until the cessation of all movements. When struggling occurs during the inhalation, he contends that the removal of the apparatus from the face is sufficient for its arrestment; and in all cases he considers it a serious matter, in the use of anaesthetics, to overstep the boundary which separates abolition of sensation from abolition of motion.

INTENSE PAIN IN THE FOOT, LASTING SEVERAL YEARS, CURED BY REMOVAL OF AN OSSEOUS GROWTH FROM THE INTERIOR OF THE POPLITEAL NERVE. BY MR QUAIN.

Elizabeth M—, aged 32, was admitted into University College Hospital, September 9th, 1845. She is married, has had two children, the second in July of the present year; she is of fair complexion and nervous temperament. A little time ago she was received as an out-patient, on account of intense pain in the sole of her foot, at its fore-part, beneath and behind the toes. The first feeling of pain occurred about six years ago, when a little twitching was felt at the inner side of the foot. At that time the pain lasted a few minutes only at each seizure, but it soon used to last for an hour and upwards, and the duration has gradually increased till it came lately to be constant, not ceasing night or day. At first the patient could, by an effort of the will, divert her attention from the pain, and when occupied, she used for awhile to forget it; but during the last four months she has been wholly unable to do so; indeed, she states that during those months she has not slept. She has not suckled either of her children, there never having been, according to her own statement, “an appearance of milk.” A variety of treatment was resorted to before the patient applied to this hospital. She had taken many medicines during several years at home and from different hospitals, but of all of them she says that only one seemed to have any influence over the pain—that was arsenic. When that medicine was used for a time, the pain ceased during two or three weeks; then, however, it returned again as bad as before, and the arsenic failed to have any beneficial effect. Having observed at the time that the woman was to all appearance in health, and that, likewise, she was an active-minded energetic person, most anxious to get relief, and to do all that might be thought necessary to effect it, he (Mr Quain) thought it likely that there should be a local cause—that is to say, an appreciable local cause—for the great suffering the woman had so long experienced, and he determined to examine the limb from the spine to the toes. In the appearance of the limb there was nothing noteworthy, and during the manipulation of the foot and leg nothing unwonted was discovered; but while pressure was made upon the back part of the thigh, in the course of the large nerves, a tumour not discernible upon the surface, was discovered. It was situated at two inches distance above the level of the upper end of the patella, and being covered by one of the two hamstring muscles, was but indistinctly felt; still, when the muscles were relaxed, the outline was pretty well made out, but its consistence could not be determined, on account of the thick covering of muscle. Pressure over the tumour gave rise to the usual pain in the sole of the foot. Here, then, obviously was the real disease. It may be worth mentioning that there was no more than a soreness in the seat of the tumour, and that only when it was pressed on. The patient now, for the first time, upon being questioned, mentioned that she felt a soreness when this part was accidentally struck, even before the pain arose in the foot; and she added that, about

three years before her coming to the hospital, on pressing against the part, she felt a small lump. At that time, too, the pressure was, she recollected, accompanied with pain in the foot. He decided on laying bare the tumour, and, if possible, separating it from the nerve. It was with a view to the operation that the patient was admitted into the hospital.

Operation.—When one of the hamstring muscles had been turned aside, the tumour came into view. It seemed an enlargement of the nerve, the internal popliteal (posterior tibial), immediately after its separation from the great sciatic. The enlargement did not affect the nerve equally all around; it projected almost altogether on the deeper side. Upon the surface the nerve-fibres were seen to pass uninterruptedly on; the tumour was therefore within. To reach it, the strands or funicula were separated by division, longitudinally, of the connecting cellular structure, and when they had been turned aside freely, the subjacent mass was pushed out without any difficulty. None of the nerve fibres were injured. The tumour, which seemed a mass of bone, was grooved partially upon the surface from the fibres of the nerve; it was the size and shape of a good-sized walnut, weighed 228 grains, and when analyzed was found by Mr Campbell to consist of phosphate and carbonate of lime, with a trace of sulphate of lime. It was not examined microscopically. On the day after the operation there was some local inflammation, accompanied with fever; but in a couple of days this subsided, and the patient left the hospital in a short time entirely relieved. There remained only a little numbness in the cicatrix when it was pressed on.—*Lancet*.

EXTRACTION OF FOREIGN BODIES FROM THE ŒSOPHAGUS.

M. Nelaton has collected with care the various modes of procedure for the extraction of foreign bodies arrested in the Œsophagus, and he in particular examines the mode of extracting fish-hooks, the removal of which is a matter of great difficulty. In speaking of œsophagotomy, this skilful surgeon proposes a proceeding which, according to him, is simpler than any other operation. Instead of making a lateral incision, M. Nelaton divides the integuments in the median line, as is done in tracheotomy, but making a more extended incision; he then separates to the same extent the sterno-hyoid muscles, so that they can be drawn apart by blunt hooks, or, if necessary, divided transversely, in order to give more space; that done the isthmus of the thyroid body is laid bare; beneath it is passed a blunt needle, carrying a double thread, in order that two ligatures may be applied; between the two ligatures the isthmus of the thyroid is divided. The trachea being thus laid bare, the left lobe of the thyroid is separated from it by a blunt instrument, keeping, at the same time, close to the trachea; at the bottom of this cleft, between the trachea and thyroid, the œsophagus is necessarily found, and is to be opened in the ordinary way. By acting thus, all risk of wounding the large vessels of the neck is avoided, and the operation may be performed without injuring the thyroid arteries.—*Elem. de Path. Chir.* and *New York Jour. of Med.*

DISLOCATION OF THE NAVICULAR BONE.

Professor R. W. Smith exhibited a cast of a foot, representing a luxation upwards of the navicular bone. The patient, an adult male, met with the accident while riding: the horse fell with him, and lay across his foot, which remained entangled in the stirrup. No effort was made to replace the bone, as the man did not apply for professional assistance until it was too late to reduce the displaced bone.

The features of the injury consisted in a projection, at the inner edge of the dorsum of the foot in front of the head of the astragalus, and a remarkable flattening of the plantar region; the sole of the foot had become perfectly flat. The accident occurred several years since, and the man now enjoys the unimpaired use of the limb. The partial displacement of the navicular and cuboid bones upwards has been described by Liston, but the preceding case furnishes

the first recorded instance of the luxation of the navicular bone alone.—
Pathological Society of Dublin.

DISLOCATION OF THE ASTRAGALUS BACKWARDS AND INWARDS. BY PROFESSOR
WILLIAMS.

PROFESSOR WILLIAMS said he would communicate to the Society a case of dislocation of the astragalus backwards, by reading the notes of the case taken by Mr Alfred H. Taylor, who was resident pupil at the City of Dublin Hospital at the time of the patient's admission.

Stephen Phillips, labourer, aged 54, of spare habit and sallow complexion, but who has uniformly enjoyed good health, and lived well and temperately, was admitted into the City of Dublin Hospital on the 12th of April 1852.

He states, that on the previous day he had been at work, along with some other men, beneath an overhanging bank of earth and gravel in the granite quarry at Kingstown, which they were undermining and removing in order to expose the granite strata underneath. At the time the accident occurred, the patient and another man were engaged shovelling the loose earth, as it was detached, into a truck or waggon, which was placed behind them on the "tramway." Whilst thus employed, another man ascended the bank, and without any warning to those below, struck a heavy iron bar, called a "clay-bar," into its upper surface, when it suddenly gave way, and being precipitated over, struck the patient violently on the left side of thorax (he was on the side of the waggon next the falling earth), he standing with his chest parallel with the front of the bank previous to its fall; but, *when struck*, his body was partially rotated backwards, and to the right side, as he was in the act of throwing a shovelful of earth into the waggon, which stood behind and to the right side of him. The force of the concussion threw him violently backwards across the roadway. The remainder of the earth fell over the lower part of his body, fixing his feet and legs to the ground. Though very much hurt, he says he was not at all confused, and describes the accident as having taken place exactly as above stated. He also says, that he does not think the mere weight of the falling earth was sufficient to produce the injuries received, as he was easily able to withdraw the injured limb without assistance; the right leg having been more heavily covered, had to be dug out before he could be completely extricated. He says he suffered acute pain in the left ankle from the moment of the accident, and it soon became swollen, and he also had severe pain in the left side of the chest. He was carried to a house in the neighbourhood, and the next day was admitted to the City of Dublin Hospital, under the care of Dr Williams.

He complained of pain and a stitch on taking a full inspiration, in the left side of the chest, and, on examination, the fifth and sixth ribs of the left side were found to be fractured about their centres. The left ankle-joint presents some curious and unusual appearances. The anterior relations of the tibia are very little disturbed, the anterior aspect of the foot being free from deformity, except indeed a nearly imperceptible shortening of the foot, which is a little everted. He has free motion of his toes, and some slight motion of the ankle-joint. Motion of the latter increases the pain, which is of a "burning" character. There is no fracture of either the tibia or the fibula, and there is some swelling and ecchymosis, with a hard tumour of an irregularly convex shape, lying between the inferior extremity of the tibia, the tendo-Achillis, and os calcis. Professor Williams came to the conclusion that this tumour was formed by the astragalus, which was dislocated backwards and inwards, and also so rotated on its antero-posterior axis, that its superior articulating surface looked almost directly inwards. A slight attempt was made to replace the bone, but was speedily abandoned, both because of the impediment to reduction presented by the above-mentioned rotation, and of the risk of further injury to the swollen and ecchymosed soft parts covering the astragalus. The limb was then placed

on a double inclined plane, so arranged that the leg lay horizontally. Leeches were applied to the joint, and subsequently cold water irrigation.

Professor Williams said he would not detain the Society by reading the details of the progress of the case, which had been fully and accurately taken by Mr Taylor. It would be enough to say, that the soft parts covering the displaced bone inflamed, and on the eighth day, when it was evident that their destruction was inevitable, they were divided by a crucial incision, which gave exit to a little sanious discharge, and exposed the bone, with its superior articular surface looking inwards. On the fourteenth day the bone was removed (some strong ligamentous attachments, which still held it, being divided with a probe-pointed bistoury guided by the forefinger), and it was then found that the astragalus had been fractured as well as dislocated, its head and a portion of the inferior surface having been broken, or rather ground off, and a quantity of the resulting small fragments were removed. The limb was then replaced in its previous position. Nothing requiring to be particularly noticed occurred until a fortnight after the removal of the bone, when an abscess formed below and behind the external malleolus, and was opened. The discharge, both from this abscess and from the cavity from which the astragalus had been removed, now rapidly diminished, and the parts presented a very healthy appearance. Matters went on favourably till about 6th of May, when some trouble was occasioned by stripping of the integuments over the sacrum and os calcis, in spite of every precaution that had been taken to guard against both. This, however, was remedied by attention to position, and on the 7th of June the limb was replaced in its original posture. From this time he went on steadily, but very slowly, improving; and on the 10th of August the cavity whence the bone had been removed had cicatrized, leaving a deep impression.

The foot was in an exceedingly slight degree extended, but not permanently so, for he possessed some power of moving the ankle, and was able to bring the foot to a right angle with the leg. He now began to move about on crutches, and at first the foot, when allowed to depend for some time, became painful and œdematous, but that inconvenience was relieved by careful bandaging, and soon ceased. Towards the latter end of August he left the hospital to go to the country, and was then able to walk pretty well with the aid of a stick.

Nothing was seen or heard of the patient until ten months after he had left the hospital, when he returned and stated that he had resumed work, though not of so laborious a kind as before, but that the extension of the foot had gradually increased, so as to cause considerable inconvenience in walking. The tendo-Achillis was now divided subcutaneously, and the foot was brought to about the same position it had been in when he first left the hospital. He was then provided with a high-heeled shoe, and left the hospital considerably improved, but using the help of a stick in walking.

Professor Williams said he was chiefly induced to bring this case before the Society, because it was rather curious so little attention has been directed to dislocation of the astragalus backward; in fact it has been scarcely noticed by systematic writers on surgery. Benjamin Bell, indeed, says that the astragalus may be displaced backwards, but he apparently does so wholly on theoretical grounds, and merely as contemplating the possibility of the occurrence; but Professor Williams could not then remember any other systematic writers who alluded to the subject, except Mr Lizars and Mr Liston, who have each very briefly mentioned a case that occurred in their own practice respectively, and Mr Liston observes that he never expected to see another. On the other hand, this dislocation was not noticed by Miller, Fergusson, Bransby Cooper, Skey, Pirie, or Erichsen, and it had even escaped the great experience and research of Mr South. The French systematic writers were quite silent on the subject. MM. Vidal de Casis and Nelaton, for example, the most recent of them, say nothing about it; and yet it was scarcely necessary to say before that Society, that there are several cases of dislocation of the astragalus

n record. The accident, however, was undoubtedly a rare one, as only six cases of it, he believed, had hitherto been published; and the fact that so few cases of the kind had yet been recorded had chiefly induced him to bring before the Society the present case, which would make the published examples of dislocation of the astragalus backwards amount to seven in number. There were, however, one or two points respecting which he would say a few words.

The six cases already known are collected by Mr Turner in his valuable monograph on "Dislocations of the Astragalus" (together with a case inadvertently quoted from Boyer as an example of that accident); and in two of those cases (Mr Philips') the bone appears to have been thrown directly backwards, as it is stated that in one the tendo-Achillis was forced back by the bone, so as to form an angle of 45 degrees, and that the appearances in the second case were exactly similar. In one case (Mr Turner's) the displacement was backwards, outwards, and downwards; and in three (those of Mr Lizards, Mr Liston, and one recorded in the *Lancet* that was admitted to University College Hospital), the astragalus was dislocated backwards and inwards. In the case read that evening, the displacement was also in the latter direction, so that in four of the seven cases now known, the astragalus has been backwards and inwards.

In only one of these seven cases (that recorded in the *Lancet*) was either the tibia or the fibula fractured; and in that case both those bones were broken at the level of the ankle-joint. This case is also the only one of the seven in which reduction was effected; and as Mr Turner observes, the existence of fracture of the bones of the leg no doubt facilitated the reduction.

In one case (Mr Turner's) the bone was removed, the dislocation having been compound, complete, and irreducible; and in four (Messrs Philips', Lizards', and Liston's) reduction being impossible, the bone was left in its new situation; in all these cases the patients did well, and had ultimately a useful limb, without death of the bone, suppuration, ulceration, or sloughing of the integuments. In the case read that night the result was different, as had appeared from the report of the case. This, then, was the first case of dislocation of the astragalus backwards, in which the soft parts inflamed, and sloughed, and exposed the bone.

It is stated in Mr Taylor's notes of the case that the attempts made to reduce the bone were not forcible, and were soon abandoned. The reasons for not making much or persevering effort at reduction were (Professor Williams said), that as soon as he had satisfied himself the bone had sustained about a quarter of a complete revolution inwards on its antero-posterior axis, so that its superior articulating surface looked directly inwards, he saw little prospect of effecting reduction, inasmuch as that deviation could scarcely be rectified by any force that could have been brought to bear on the bone, and even if it were practicable, it could not have been effected without inflicting an unjustifiable amount of injury upon the soft parts covering the bone; the attempt at reduction, therefore, was very slight; so slight, indeed, as scarcely to be called an attempt at reduction; and, moreover, the history of previous cases of the accident tended to show that, on the one hand, there was little hope of replacing the astragalus when the tibia and fibula were unbroken; and on the other, that in every instance in which the bone had been left in its new situation the issue had been satisfactory. As to the rotation of the astragalus, that bone could be rotated either on its antero-posterior or transverse axis. In Mr Liston's case, the astragalus was rotated in the latter direction; and in Mr Turner's it was rotated on its antero-posterior axis outwards, the superior articular surface presenting at the wound. In the case just read the bone had made a quarter of a revolution inwards. Professor Williams said he need not dwell upon the mechanism of the various rotations of the astragalus in dislocation; for whether the rotation was partial or complete; whether on the antero-posterior or transverse axis; or whether it occurred in dislocation forwards or backwards, the mode of its production was analogous in each case, and explicable on the

same principles. But the recognition of the existence of such a rotation was of great importance, for when it existed to any considerable extent, Professor Williams thought it rendered reduction impracticable. Now, the outlines of the astragalus are so well marked and recognizable by the touch, and the exact position was so clearly made out in this case of dislocation *backwards*, when the soft parts covering the displaced bones are so much thicker than in dislocation forwards, that he could not but think the existence and extent or non-existence of rotation could be easily determined, at all events in most cases, and especially in anterior dislocation; and thus an important guide as to the treatment to be adopted would be obtained.

Mr TUFNELL said—I met the patient the other day in the street when he was walking with one stick; he formerly required two, but latterly he said he had discontinued one of them. I asked him whether he was able to carry a basket on his arm and to work for his living, and he replied that he could not. Now, this is an important point to bear in mind. Three years ago, I visited the Curragh of Kildare to see a young soldier, whose horse fell on his side and dislocated the astragalus; I removed the latter and left the case to nature, and it ultimately did so far well that the man recovered, but with a pointed toe; and he afterwards wrote to me from England to know whether there was any contrivance which would enable him to support the weight of the body from the knee. I regretted that in that case I did not take away the foot by Syme's operation.

Mr BUTCHER—I do not think that under any circumstance the surgeon would be warranted in removing the ankle-joint in a case of this dislocation. The amputation of the ankle-joint is not so well established as Mr Syme would lead us to suppose. I performed it myself on a woman, and although the stump was a beautiful one, she found it exceedingly difficult to walk on it. I since had to amputate in another case where Syme's operation had been performed some months before. I do not think the astragalus should be removed unless the symptoms were of a very urgent character. If an operation, however, must be performed, I would prefer to remove the astragalus, and also the extremities of the malleoli by bone forceps, so as to let the tibia down on the calcis. In London a considerable number of persons are walking about every day without the astragalus, and I think that on the whole, it is better to adhere to the rule of endeavouring to save parts as much as possible.

Mr TUFNELL—When I spoke of Mr Syme's operation, I intended to confine it to cases where accidents had occurred to a healthy structure; Mr Butcher seems to refer to amputation in cases of disease. There is a distinction to be drawn between a foot that is useful to a patient, and one that can be used. If a case occurred to a person who had to labour for his bread, I would remove the foot; but on the other hand, if he belonged to the upper ranks of society, had no occasion to labour for his bread, and did not like the deformity, then I would endeavour to preserve the foot.

Professor WILLIAMS—I have no hesitation in saying that on several points the treatment of dislocation of the astragalus requires to be very carefully reviewed, and that Mr Turner's statistics are not sufficiently extended to justify some of the conclusions which have been drawn from them. I have myself collected a considerable number of cases in addition to those that are reported in his work, and I hope on some future occasion to bring the subject at greater length under the notice of this Society. As regards the question of leaving the bone *in situ*, this much is to be said, that if authority deserves to have any weight, we have in favour of doing so the great names of Sir Astley Cooper, Dupuytren, and other surgeons of eminence, who did so even in some cases where it was anticipated that the parts covering the bone were likely to slough.
—*Dublin Medical Press*, April 1855.

Part Fourth.

MEDICAL NEWS.

STATEMENT EXPLANATORY OF THE PRINCIPLES OF A DRAFT OF A BILL FOR REGULATING THE MEDICAL PROFESSION.

It is now twenty-two years since an attempt was made to introduce into Parliament a bill for regulating the Practice of Medicine. Repeated attempts have been made since then, but all have failed, for one or more of the following reasons :—

1. Because too much was attempted at one time ; viz., to regulate the practice of physicians, and surgeons, and general practitioners in one bill.
2. Because it was contemplated to suppress some existing medical institution or institutions, by withdrawing from them privileges long exercised, as they thought, for the public good.
3. By proposing to create a new medical incorporation ; which would have certainly injured or destroyed one or more of those pre-existing.
4. By proposing for the executive medical head of the profession a representative council, chosen by the several medical institutions.
5. By attempting to define too precisely the requisite qualifications and the privileges of the three ranks of the profession, and to separate them in three distinct registers.

As the medical institutions, possessing, more or less, the right of practice, of one kind or another, in one part or another of the United Kingdom, are twenty in number, it is evident that no medical bill has any chance of success without avoiding these errors, unless it be pushed through Parliament in face of a considerable dissentient minority of these bodies, by the weight and influence of Government ; which, whether rightly or wrongly, has been hitherto refused on such terms.

The accompanying draft of a bill is suggested, as being entirely free of all the preceding objections.

1. It proposes to confine legislation, in the first instance, only to the general practitioner, on the grounds—1. That this is the class in whose qualifications the public are most widely interested ; and, 2. That the regulation of the higher classes of the profession may be safely left, for the present, to the discretion and honourable rivalry of the bodies which now govern them.

2. It provides for a uniform system of qualification by education and examinations for general practice, as essential for entering any branch of professional practice in any part of the United Kingdom.

3. It proposes to confer a reciprocity of right of practice in all parts of the kingdom, by providing that any of the present medical titles, which now convey a right to practise any branch of medicine in some division, or some part of a division, of the United Kingdom, shall convey henceforth the privilege of practising medicine, in its several branches, in any part of the kingdom, provided the education required for the said title, and the examinations conducted by the said bodies, singly or conjunctly, shall comprise at least the education and examinations to be required by sect. 2 of this statement.

4. It proposes to place the nomination of the medical council of the nation, or general governing body, in the hands of the Crown, as the only way of at once limiting sufficiently the number of the council, and securing, as far as possible, an impartial nomination.

5. It contemplates the framing and publishing of a single register for the

United Kingdom, kept partly in each division thereof, presenting the several medical titles granted to each individual by the graduating or licensing bodies recognized in this draft of a bill.

The bodies to be recognised are the several universities and chartered incorporations specified in the Appendix to the Draft. These are twenty in number.

Strictly interpreted, there are certainly only three—probably, however, two more—of these bodies whose charters enable them to confer the privilege of practising every branch of the medical art; but by usage the greater number of their charters have been allowed a much more liberal interpretation.

An attempt was lately made before Parliament to deny that the Scottish Universities could confer any right of practising any branch of the medical profession in any part of the United Kingdom, even in Scotland. Without specifying the terms of royal charters, or of statutes of an early date, or going into any other detail on this subject, it is sufficient to notice that, in 1840, when a second case between the University of Glasgow and the Faculty of Physicians and Surgeons of Glasgow was finally disposed of by the House of Lords, it was held by the Lord Chancellor Cottenham, and Lord Brougham, that the decision of the Court of Session, in a prior case between these corporations, which had reference, among other things, to university degrees and to diplomas held by various parties, had “found and established,” that, in virtue thereof, “these parties are authorised to *practise* medicine;” and that the unanimous opinion of the judges of the Court of Session bears, with regard to licenses for the *practice* of physic, that “a degree from a university, or a license from the royal physicians, was sufficient for that purpose.”

At the same time an attempt was made to impress on Parliament the idea that universities were, in their own nature, intended to confer honours, not to bestow privileges, on their graduates. The example of other countries is a sufficient answer to this proposition; and, in point of fact, the legislature have already repudiated it by the act in favour of the graduates of the London University, passed at the close of last session.

The privileges of some of the bodies, which this draft of a bill proposes to recognise, at present extend over a very limited district; but, nevertheless, in such a measure as the present, no sufficient reason can be shown for similarly restricting the range of their privilege, provided it be not exclusive, and if they may be expected to do public good by sharing in the wider privilege. In fact, some of these bodies of limited privilege have done so much public good, that the title conferred by them has, by usage, been recognised as a qualification for practising medicine throughout almost the entire United Kingdom, as well as the colonies.

Competent judges have doubted whether some of these institutions have hitherto existed for the public good. The advantage of the measure now proposed is, that this consideration does not require to be taken into account. While it excludes no such body from honourable competition, it compels all, without exception, to compete, on terms of fairness and equality, for public support.

It is not impossible that, under such competition, some of the bodies recognized in this draft of a bill may sustain injury, or even suffer gradual decay; but no existing institution will suffer, unless it ceases to enjoy the public confidence; and these are not the days for upholding monopolies by partial favour, when the objects of it have ceased to confer, and possibly never did confer, any commensurate public benefit.

The annexed draft of a bill does not contemplate the confounding of all ranks of the profession under one denomination, or the establishment of what has been called a one-faculty system. It leaves the several universities and royal colleges, which have hitherto fostered the higher branches, to continue to do so uncontrolled; and there can be no doubt that their existence, their well-known desire and evident self-interest, together with the equally evident

demands of the public interest, will enable them effectually to maintain the distinctions of rank which have been hitherto preserved in this country.

It is true that the draft of a bill contemplates the mixing up of all ranks in one register. But, when every individual is distinguished in it by his proper medical titles, such a register will no more establish a one-faculty-system, than it is established by the present medical directories, against which no one has ever dreamed of bringing this charge. What, after all, is the contemplated register, but a perfected and authentic medical directory?

DRAFT BILL FOR AN ACT FOR REGULATING AND IMPROVING THE MEDICAL PROFESSION.

WHEREAS it is not expedient for the public interest that any person shall be permitted to practise the medical profession without being duly qualified :

And whereas it is not consistent with the equal privileges of the subjects of a United Kingdom that persons held qualified to practise in one part, be prohibited from doing so in another part, of such United Kingdom :

And whereas the laws now in force concerning the medical profession require to be amended :

BE IT ENACTED by the Queen's Most Excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons, in this Parliament assembled, and by authority of the same—

1. That the several acts of Parliament mentioned in Schedule A, hereunto annexed, shall be, and the same are hereby repealed, in so far as the same are not consistent with this act, and as may be requisite for giving full effect to all the provisions herein contained.

2. That the words following shall have the signification hereby given to them, unless there be anything in the context repugnant to such signification :—
“ Medical Practice,” or “ Medical Profession,” shall include and signify the practice of “ Medicine, Surgery, Midwifery, and Pharmacy ;” “ Medical ” shall include “ Surgical ;” “ Council ” shall signify the medical council constituted under this act ; “ Registrar,” “ Secretary,” and “ Treasurer,” shall signify, respectively, the registrar, secretary, and treasurer for the time to the said council in England, or in Scotland, or in Ireland, as the case may be.

3. That a council shall be established, which shall be styled, “ The Medical Council for the United Kingdom ;” which council shall consist of one of her Majesty's principal Secretaries of State, and of twelve members of the medical profession, of whom six shall be resident in England, three shall be resident in Scotland, and three shall be resident in Ireland : That all of said twelve members shall be appointed by her Majesty, with the advice of her privy council ; that four members of such council shall be a quorum ; and that the appointments of all the first members of such council shall be made within three months after the passing of this act.

4. That one of her Majesty's Secretaries of State shall always be a member of council, and be the president of the council, in virtue of his office ; and that every other member of said medical council shall be entitled to be a member thereof for three years, and shall then go out of office, but may forthwith be reappointed ; and that, upon every vacancy among these other members, such vacancy shall be forthwith filled up by her Majesty, with the advice of her privy council—the new member being chosen for England, Scotland, or Ireland respectively, in such manner as at all times to maintain the above proportion of members from these respective parts of the kingdom.

5. That to the extent hereinafter mentioned, the carrying into effect the powers and duties by this act vested in the said medical council, may be devolved by such council upon the sub-councils for England, Scotland, and Ireland respectively, hereinafter mentioned.

6. That the said medical council shall, as soon as may be after the same has been appointed, meet in London at such place and time as shall be appointed

by one of her Majesty's secretaries of state, by notice given to the various members ; that the council shall from their own body elect a vice-president ; and that besides having a vote as a member, the president, or in his absence such vice-president, shall, in the event of an equality of votes, have an additional or casting vote in all matters ; and that, subject to this provision, every question brought before the council shall be decided by a majority of votes of those present at the meeting.

7. That within three months of its appointment, the said medical council shall determine the course of study and amount of examination, without which no one shall be permitted to enter upon medical practice, and also to fix the age under which certificate or license to practise shall not be given. That the whole matters as to amount of study, examination, and age, shall be reconsidered by the medical council at least once in every three years ; and that their determination thereon shall remain in force until the same be altered by the council.

8. That the degrees, or diplomas, or licenses, as physicians, or surgeons, or apothecaries, of all the universities in Great Britain and Ireland, and other corporations within the kingdom, at present authorised to confer such (and which several universities and corporations are named in Schedule B hereunto annexed), shall be recognised by the council as qualifications for registration with a view to practice ; provided that, as to all persons entering on medical practice after the passing of this act, the requirements established by the council as to course of study, the mode and extent of examination, and age, shall have been fulfilled by the said universities and corporations, excepting in so far as the same shall have been dispensed with by the council, in favour of medical students whose medical studies were commenced before the passing of this act.

9. That it shall be lawful for the said medical council to make regulations for dispensing with such parts of the study and examination fixed under this act as to them shall seem fit, in favour of medical students whose medical studies were commenced before the passing of this act.

10. That, for the purposes of such examination, any two or more of the said corporations, whose examinations separately do not embrace the whole course of examination so to be prescribed, may, with the sanction of the said medical council, either combine and unite in making the examination, or may each make their own part of the examination ; provided that, in the one way or in the other way, the whole of the prescribed examination shall take place by corporations named in said Schedule B.

11. That, in order to ensure efficiency and equality of examination, all the members of the medical council, or any person appointed by the medical council, or by one of the said sub-councils, shall have free access to all examinations of students or candidates, and to the records of such examinations, and shall also have right to accompany them to hospitals or infirmaries.

12. That if, at any time, the council shall be satisfied that any university or corporation is not complying with and carrying into effect each and all of the requirements previously established by the council as aforesaid, or is granting, conferring, or issuing degrees, diplomas, or licenses without such requirements having been all complied with, the council shall be, and hereby is empowered and required, when and as often and so long as this occurs, to refuse to recognise as qualifications for registration each and all of the degrees, diplomas, or licenses granted, conferred, or issued by such university or corporation during the period when the requirements of the council were, or any of them was, not complied with : That all resolutions of the council with regard to these, or any of these matters, shall be entered in the minutes or record of the council, and be signed by the president of the meeting, or otherwise authenticated as the council shall direct ; and that a copy or extract, signed by the registrar, and sealed or stamped with the seal of the council, shall be evidence in all courts and places whatever, as well of such resolution and of such minute, as of all matters and facts therein embraced and stated.

13. That no degree, or diploma, or license already issued, and no degree, or diploma, or license to be hereafter issued by any body whatever, shall qualify or entitle the holder, after the 1st of June next after the passing of this act, to enter upon or continue medical practice within any part of the United Kingdom, until he shall have produced the same to the said medical council, or to one of said sub-councils, and until his name and residence, with his medical title or qualification, has been entered in the register, and a certificate, in terms of this act, has been obtained.

14. That a register shall be kept by the said medical council of the name and residence, as well as of the medical title or titles, and dates of such titles, granted by any of the universities or other corporations mentioned in Schedule B, of every medical practitioner who shall apply to be registered, and who, prior to the passing of this act, shall have obtained a degree or diploma in medicine or surgery, or license as an apothecary, from any of the aforesaid bodies authorised to confer such—and who shall produce such degree, or diploma, or license, or a duly attested certificate, or such other proof as shall be satisfactory to the said medical council or sub-council, of his having, prior to the passing of this act, obtained such degree, or diploma, or license, and of every person who shall so apply to be registered, who, after the passing of this act, shall obtain, and who shall produce the like degree, or diploma, or license, or such other proof as aforesaid, by any of the said universities or bodies mentioned in Schedule B. That such register shall be the warrant for certificates to be issued by the registrars as hereinafter provided; and that the said medical council shall provide suitable books for such registration,—which books or registers shall be the property of the council,—and be kept and preserved for them in such places, and under such rules, as they may make or appoint.

15. That three books shall always be kept for such registration—one in England, one in Scotland, and one in Ireland. That the register in England shall be kept under the superintendence and control of the members of council resident in England, who shall form a sub-council for England, and meet in London. That the register in Scotland shall be kept under the superintendence and control of the members of council resident in Scotland, who shall form a sub-council for Scotland, and meet in Edinburgh; and that the register in Ireland shall be kept under the superintendence and control of the members of council resident in Ireland, who shall form a sub-council for Ireland, and meet in Dublin.

16. That registration as aforesaid may take place in England, or in Scotland, or in Ireland, as to any duly qualified practitioner resident in any of the colonies, or elsewhere beyond the United Kingdom: But that the original and every renewed registration of every practitioner resident in England and Wales, or in Berwick-upon-Tweed, the Channel Islands, or the Isle of Man, shall take place only in England; the like registrations of every practitioner resident in Scotland shall take place only in Scotland; and the like registrations of every practitioner resident in Ireland shall take place only in Ireland;—and that registrars shall not record or issue certificates except in conformity with these provisions: that each registered medical practitioner who shall come to reside within one of the said three parts or divisions of the United Kingdom different from that in which his prior registration took place, shall, at the annual registration occurring next after such change of residence, effect his registration with and obtain his certificate from the sub-council and registrar for that part of the United Kingdom within which the new residence of such practitioner is locally situated: that in order to obtain that registration, the practitioner shall produce to such sub-council and registrar the same evidence of qualification as is by this act made requisite for original registration, and shall also pay the like fee as is hereby made payable for the original registration and certificate.

17. That the said medical council shall appoint a fit and proper person to be their registrar and secretary, and the same or a different person to be their treasurer, who, besides holding these offices under the council, shall also be

registrar, secretary, and treasurer for England under the sub-council for England: that the said medical council shall also appoint a registrar, secretary, and treasurer for Scotland, and a registrar, secretary, and treasurer for Ireland, and, for those various parts of the United Kingdom, shall also, from time to time, appoint such clerks and servants as they may deem necessary: that there shall be paid to each of such officers such yearly salary or remuneration as such council shall think fit; and each of such officers shall be removable at the pleasure of the council: that the council shall cause to be made a seal or seals for their use in the execution of this act, in each of the three divisions of the United Kingdom hereinbefore mentioned, and shall cause to be sealed or stamped therewith all certificates issued by them in pursuance of this Act. That all certificates and other documents purporting to be signed by the registrar, and sealed or stamped with any such seal, shall be received as evidence in all courts and places whatsoever.

18. That for such registration in the first instance, and for the certificate thereof, whether original or upon change of residence as before provided, each applicant shall pay to the registrar, for behoof of the council, a fee of which fee shall be paid before the registration takes place; and that, for each subsequent annual registration and certificate, each applicant shall pay a fee of five shillings, which fee shall likewise be paid to the registrar for behoof of the council before registration.

19. That to every person who shall be registered in manner above provided, the registrar shall give a certificate, impressed with the seal of the council for that division of the United Kingdom to which he is attached, according to the form in Schedule D to this act annexed, and which certificate shall require to be renewed annually—it being hereby enacted, that the certificate first issued shall be in force till the first day of February immediately following its date, and no longer.

20. That when such registration has once taken place, the registrar shall thereafter annually, upon demand, issue a certificate, also according to the form of Schedule D to this act annexed, to every person registered as aforesaid who shall apply for such certificate; and such certificate shall bear date on the day when the same shall be so issued, and shall continue in force until the first day of February then next ensuing, and no longer.

21. That each renewed or annual certificate so issued shall by the said respective registrars be duly recorded in the register, in the same way and form as the certificate first issued, but with such additional medical title or titles as the party may have subsequently acquired from any of the universities or other corporations before mentioned, stating in the register the name, residence, and medical title or titles aforesaid, with the date thereof; and that during the month of February in each year the registrar in Scotland and the registrar in Ireland shall respectively furnish to the registrar of the said medical council a copy of the said record kept by him: And the registrar of the said medical council, being also registrar in England, shall, in the month of March in every year, cause to be printed a correct register, according to the form in Schedule C to this act annexed, of the names, arranged alphabetically, with the respective residences and medical titles conferred as aforesaid, with the dates thereof, of all persons to whom certificates shall have been issued during the year then last past, according to the provisions of this act, and publish or furnish printed copies on such terms and in such way as the council may direct: And that a printed copy of the register, so published as aforesaid, shall be evidence in all courts, and before all justices of the peace and others, that the persons therein specified have obtained certificates according to the provisions of this act; and the absence of the name of any person from such printed copy shall be evidence, until the contrary be made to appear, that such person has not obtained a certificate according to this act.

22. That every person who shall be registered, and shall possess a certificate, in force according to the provisions of this act, shall be entitled to practise

medicine, surgery, midwifery, and pharmacy, throughout every part of the United Kingdom and of her Majesty's dominions. And that every person who shall be registered, and possess a certificate according to the provisions of this act shall be entitled to demand and recover in any court of law, with full costs of suit, reasonable charges for medical advice, visits, operations, and medicine, without any other license than such registry and certificate.

23. That all moneys received by the said respective registrars shall be paid over to the treasurer of the said medical council, and shall be applied to defray the expenses of carrying this act into execution, in such manner as shall be directed by said medical council, with the sanction of one of her Majesty's secretaries of state: And that from the said moneys there shall be paid to the members of said council, and to persons appointed by them to attend examinations, such reasonable expenses, incurred by them in performance of their duties under this act, not exceeding three guineas for each attendance, and also such reasonable allowance for mileage as shall from time to time be fixed by said council: And if, after paying all such expenses, any surplus of said moneys shall remain, such surplus shall be yearly applied for the founding and establishing of medical scholarships or prizes, or in promoting the advance of medical science and literature, in such manner as the medical council shall determine.

24. That the said council shall, once in every year, publish a full account of their income and expenditure for the year then last past, and shall transmit a copy of such account to one of her Majesty's principal secretaries of state.

25. That after the 1st day of June 1855, no person shall be entitled to recover any charge in any court of law for any medical advice, attendance, or for the performance of any surgical operation, or for medicine prescribed by him, unless he shall prove upon the trial either that he is in possession of a certificate in force according to the provisions of this act, or that he was legally practising in the capacity in which he claims such charge at the time when the same was incurred.

26. That after the 1st day of June 1855, no person who does not possess a certificate in force, according to the provisions of this act, shall be capable of holding any appointment in any part of her Majesty's dominions in the capacity of a physician, surgeon, or other medical officer, in any hospital, infirmary, dispensary, lunatic or other asylum, lying-in hospital, gaol, penitentiary, house of correction, house of industry, parochial or union workhouse or poor-house, parish union, or other public establishment, body, or institution, or to any friendly or other society for affording mutual relief in sickness, infirmity, or old age. And that in any case in which by law the certificate of a medical man is required, a certificate by an unregistered person shall not be received.

27. That if any person shall, after the 1st day of June 1855, act as a medical practitioner in any part of the United Kingdom, without being duly registered according to the provisions of this act, and without having a certificate as aforesaid in force at the time of his so practising or acting, he shall, on conviction before any magistrate having jurisdiction in the county, city, or place where the offence was committed, forfeit and pay a sum not exceeding five pounds, nor less than forty shillings, for every such offence, to be recoverable within six months next after the commission of the said offence.

28. That if any registrar under this act shall wilfully make or cause to be made any falsification in any matters relating to any register, certificate, or record aforesaid, every such offender shall be deemed guilty of a misdemeanour in England and Ireland, and in Scotland of a crime and offence; and shall on conviction thereof be sentenced to be imprisoned for any term not exceeding twelve months.

29. That if any person shall procure, or attempt to procure, a certificate from any registrar, by making, or producing, or causing to be made or produced, any false or fraudulent degree, diploma, or license, or shall by any false

or fraudulent means whatsoever, possess, obtain, use, or attempt to possess, obtain, or use any registration or certificate as aforesaid, every such person so offending, and every person aiding and assisting him therein, shall, upon being convicted thereof, be adjudged guilty of a misdemeanour in England and Ireland, and in Scotland of a crime and offence; and thereupon it shall be lawful for the court before whom such offender shall be tried and convicted to sentence such offender to be imprisoned, with or without hard labour, for any period not exceeding twelve months.

30. That every unregistered person who shall wilfully and falsely pretend to be, or take or use the name or title of a physician, doctor, bachelor of medicine, or surgeon, or any name, title, addition, or description, implying that he is registered under this act, or that he is recognised by law as a medical practitioner, shall, on being convicted of such offence before any magistrate, pay a fine not exceeding twenty pounds, nor less than five pounds for every such offence, which penalty shall be recoverable as hereafter described.

31. That any two justices of the peace, acting in and for the county, city, or place in which the offence had been committed, and sitting in petty sessions, or any magistrate appointed by virtue of an act passed in the second and third years of the reign of her Majesty Queen Victoria, intituled, "an Act for regulating the Police Courts of the Metropolis," or one of the sheriffs or justices of the peace courts in Scotland, may hear and determine any complaint charging any person with practising medicine without a certificate as aforesaid, or with pretending falsely to be registered, on the oath of one or more witnesses, or by the confession of the accused party, and shall award the penalty or punishment herein provided for such offence respectively, and in every case of the adjudication of a pecuniary penalty under this act, and of non-payment thereof, it shall be lawful for the said justices, sheriff, or magistrate, to commit the offender to any gaol or house of correction within his jurisdiction for a term not exceeding one calendar month when the sum does not exceed forty shillings, and for a term not exceeding six calendar months when the sum does not exceed twenty pounds, the imprisonment to cease on payment of the sum due.

32. That any sum or sums of money arising from conviction and recovery of penalties for offences committed against the authority and provisions of this act shall be paid to the treasurer of the sub-council for that part of the United Kingdom in which such conviction shall take place, and be applied to the purposes of this act; and the same shall and may be sued for either by him or by any other person authorised so to do by such sub-council.

33. That every person who shall be registered, and possess a certificate in force under the provisions of this act, shall be exempt, if he shall so desire, from serving on all juries and inquests whatsoever, and from serving any corporate, parochial, ward, hundred and township office, and that the name of such person shall not be returned in any list of persons liable to serve in any such office as aforesaid; and further, any prior act to the contrary notwithstanding, that no person shall be entitled to such exemption as aforesaid, on the ground of being a physician, or surgeon, or apothecary, who does not possess such certificate then in force as aforesaid.

34. That if any registered medical practitioner shall be convicted in England or Ireland of any felony or misdemeanour, or in Scotland of any crime or offence inferring infamy, or the punishment of death or transportation; or if it shall be found, by the judgment of any competent court, that any such medical practitioner shall have procured a certificate under this act by any fraud or false pretence, or that any such medical practitioner has wilfully and knowingly given any false statement, evidence, or certificate, in any case in which by law the evidence or certificate of a physician or surgeon is required, the registrar of the board in whose register his name is entered, on the production before him of an office copy or extract of the conviction or judgment of the court, duly certified under the hand of the proper officer of the court,

or other proof thereof, shall cause the name of such medical practitioner to be erased from the register; and every person who shall have been so erased, after such conviction or judgment as aforesaid, shall thereby forfeit and lose all the privileges of a registered medical practitioner provided by this act.

35. That this act may be amended or repealed by any act to be passed in this session of Parliament.

SCHEDULES.

SCHEDULE A.—Acts referred to in Section First of this Act, and which are hereby Repealed to the extent therein mentioned.

Date of Act.	Title of Act.
3 Henry VIII., c. 11. Anno 1511.	An act for the appointing physicians and surgeons.
14 and 15 Henry VIII., c. 5. Anno 1522-23.	The privileges and authority of physicians in London.
32 Henry VIII., c. 40. Anno 1540.	For physicians and their privilege.
32 Henry VIII., c. 42. Anno 1540.	For barbers and surgeons.
33 and 34 Henry VIII., c. 8. Anno 1542-43.	A bill, that persons being no common surgeons, may minister medicines, notwithstanding the statute.
1 Mary, sess. 2, c. 9. Anno 1553.	An act touching the corporation of physicians in London.
10 George I., c. 20. Anno 1723.	An act for the better viewing, searching, and examining of all drugs, medicines, waters, oils, compositions, used or to be used for medicines, in all places where the same shall be exposed for sale, or kept for that purpose, within the city of London or suburbs thereof, or within seven miles' circuit of the said city.
18 George II., c. 15. Anno 1745.	An act for making the surgeons of London and the barbers of London two separate and distinct corporations.
55 George III., c. 194. July 12, 1815.	An act for better regulating the practice of apothecaries throughout England and Wales.

SCHEDULE B.—Universities and other Corporations in Great Britain and Ireland whose Degrees, or Diplomas, or Licenses are to be recognised by the Medical Council appointed under this Act.

1. The University of Oxford; 2. The University of Cambridge; 3. The University of London; 4. The University of Durham; 5. The University of Edinburgh; 6. The University of Glasgow; 7. The University and King's College of Aberdeen; 8. The Marischal College and University of Aberdeen; 9. The University of St Andrews; 10. Trinity College, Dublin; 11. The Queen's University, Ireland; 12. The Royal College of Physicians of England; 13. The Royal College of Physicians of Edinburgh; 14. The Royal College of Physicians of Ireland; 15. The Royal College of Surgeons of England; 16. The Royal College of Surgeons in Edinburgh; 17. The Royal College of Surgeons in Ireland; 18. The Faculty of Physicians and Surgeons of Glasgow; 19. The Apothecaries' Society of London; 20. The Company of Apothecaries of Ireland.

SCHEDULE C.

The medical register in $\left\{ \begin{array}{c} \text{England,} \\ \text{Scotland,} \\ \text{or} \\ \text{Ireland.} \end{array} \right\}$ as the case may be.

MEDICAL REGISTRATION CERTIFICATE FOR THE UNITED KINGDOM OF
GREAT BRITAIN AND IRELAND, FOR (specify the year).

In accordance with the provisions of an Act of Parliament passed in the year of the reign of Her Majesty Queen Victoria, intituled, "An Act for regulating and improving the Medical Profession," I hereby certify that A B, residing at C, in the county of D, having produced before the Sub-Medical Council of the United Kingdom for [England, Scotland, or Ireland, as the case may be] (*Here specify some one of the diplomas or other medical titles or qualifications mentioned in the Section 13 of this Act*), has been duly registered, according to the provisions of the said Act, as a person who is qualified to practise Medicine in any part of Her Majesty's Dominions, and that he is entitled to exercise all the powers and privileges conferred by the said Act.

This certificate to remain in force until the 1st day of February 18 , and no longer.

(Signed)

Registrar in $\left\{ \begin{array}{c} \text{England,} \\ \text{Scotland,} \\ \text{or} \\ \text{Ireland.} \end{array} \right\}$ as the case may be.

Date.

of the Medical Council of the United Kingdom.

SCHEDULE D.—The Medical Register for the United Kingdom of Great Britain and Ireland, consisting of the Names and Places of Residence, with a description of the Medical Titles or Qualifications, and the Dates thereof, of all Persons legally qualified to practise Medicine in Her Majesty's Dominions, in the Year 18 .

Arranged alphabetically.—For England.

Name.	Medical Title or Qualification.	Place of Residence.
A. A.	Doctor of Medicine of the University of London, 1851.	30 Brook Street, London.
A. B.	Licentiate of the Apothecaries' Society of London, 1848 ; Member of the Royal College of Surgeons of England, 1848.	100 Clarendon St., Manchester.
A. C.	Member of the Royal College of Surgeons of Edinburgh, 1840 ; Doctor of Medicine of the University of Edinburgh, 1842.	10 George's Square, Canterbury.
A. E.	Doctor of Medicine of the University of Cambridge, 1840 ; Fellow of the Royal College of Physicians of London, 1850.	20 Dover's Street, London.
A. F.	Licentiate of the Apothecaries' Society of London, 1848.	33 Bishopgate St., London.

REPORT by the PRESIDENT'S COUNCIL of the ROYAL COLLEGE of SURGEONS of EDINBURGH, 9th March 1855, upon a document intitled "Draft Bill for an Act for regulating and improving the Medical Profession," presented on 12th March; amended and adopted by the College on 31st March 1855.

The Council having been lately informed that a "Draft Bill" for regulating and improving the medical profession had been prepared under the auspices of some of the medical professors of the University of Edinburgh, and transmitted to the Home Office, lost no time in applying for a copy of it, which was immediately sent by the Home Secretary. It was accompanied by a letter, now on the table, which invites the College to consider its provisions.

The Council have therefore given their attention to this very important matter, on which they report to the College as follows:—

They consider it their duty to premise, that they think this College, and medical men generally, have some reason to complain that a bill for regulating the interests of the whole of the medical profession should have been prepared and placed in the hands of the Government, and that by parties having interests very different in some respects from that profession, without making them aware of its nature, or even of its existence. This is the more remarkable as all former bills have been brought forward and advocated in the most public way.

The draft bill is preceded by a printed pleading in its favour, entitled an "Explanatory Statement," on which, as well as on the bill itself, the Council will have to remark.

In the first page of this "Statement," it is observed by its framers that the medical institutions possessing in some degree "the right of practice"—(by which, the Council presume, is meant, possessing *the power of conferring that right*)—in this country, are *twenty* in number, to all of which it is proposed by the draft bill (2d and 8th clauses) to grant the power of conferring the right of "medical practice," including under this term "medicine, surgery, midwifery, and pharmacy;" and by referring to a schedule at the end of the bill, it appears that *eleven* of the *twenty* are universities.

In page 3 of the "Explanatory Statement" are the following remarks:—

"An attempt was lately made before Parliament to deny that the Scottish universities could confer any right of practising any branch of the medical profession in any part of the United Kingdom, even in Scotland. The highest authorities in the country—the Court of Session in Scotland, by a unanimous decision, and the House of Lords in 1840—decided otherwise in the case which was formally tried by the Faculty of Physicians and Surgeons of Glasgow against the University of Glasgow.

"At the same time, an attempt was made to impress on Parliament the idea that universities were, in their own nature, intended to confer honours, not to bestow privileges on their graduates. The example of other countries is a sufficient answer to this proposition; and in point of fact, the Legislature have already repudiated by the act in favour of the graduates of the London University, passed at the close of last session."

Now, the Council have taken some pains to ascertain the state of the fact as to the lawsuit between the Faculty of Physicians and Surgeons of Glasgow and the University of Glasgow; and they find that the substantial question to which the decisions of the Court of Session and of the House of Lords related, was the right of the Faculty under their charter, to debar all persons (whether graduates of Glasgow University or others), not holding *their* diploma, from practising surgery and pharmacy within certain counties in the West of Scotland; and that on this, as on every other collateral point of the case, the decision of both courts was *entirely in favour of the Faculty*. The larger question as to the power of the University of Glasgow, or of the universities generally, Foreign and British (for under the reservation of "one famous university where medicine is taught," the British can take no more than the foreign

universities, to whom chiefly that reservation must have originally applied), to confer the right of practice, either in surgery, or any other branch of the medical art, was not adjudicated upon in that action, further than that a plea put forward by the Faculty in defence against the claim of the University—viz. "That a university, however highly privileged, can only grant academical honours and titles of distinction; and is not entitled to attach to those degrees the privilege of practising an art or trade, and far less to do so to the prejudice of a regularly established corporation"—was "*sustained*" by the Court of Session in their judgment, which was afterwards affirmed by the House of Lords. The assertion that a unanimous decision, or any decision at all, was pronounced upon this question in favour of the universities, either in the House of Lords or the Court of Session, is therefore utterly without foundation.

"The example of other countries," adduced in the second paragraph above quoted, and the value which may be supposed to belong to *their* university degrees, can surely be of little avail in a legal question regarding the privileges of a British institution. It is much more to the purpose to state, that although there has been no Scotch decision expressly determining the precise nature and effect of a university degree in this country, the question may be held as having been settled in the case of the London College of Physicians *versus* Dr West, in the Court of King's Bench. The point directly raised in that case (and in a previous case in the same court) was precisely analogous to that of the Glasgow faculty and the university graduates, viz., whether a graduate in medicine of Oxford was entitled to practise as a physician in London, or within a circuit of seven miles, without a license of the College of Physicians? It was solemnly adjudged by the court that he could not do so; and the report (Leitch's Modern Reports, vol. x.) thus proceeds:—

"As to the testimonials granted by the universities upon a person's taking the Doctor's degree,—

"The court was of opinion that these testimonials might have the nature of a recommendation—they might give a man a *fair reputation*, but confer no right. Consequently all those statutes which confirmed the privileges of the universities could revive or confirm *nothing but the reputation* that the testimonials might give such graduates: And whereas it has been insisted that, by the last clause of this statute [14th and 15th Henry VIII.] it is said, 'That none shall practise in the country without a license from the president and three elects, unless he be a graduate of the university,' it was said that all the inference from that would be, that possibly two licenses may be necessary where a person is not a graduate. In the case of Dr Lovett, Lord Chief Justice Holt did not think this a question worth being found specially. The College of Physicians is without doubt more competent to judge of the qualifications of a physician than the university; and there may be many good reasons for taking a particular care of those who practise in London."

This case is expressly founded on, and the principle it establishes as to the nature of university degrees adopted by the Consulted Judges of the Court of Session, in the very elaborate opinion pronounced by them in the Glasgow case, wherein the following passage occurs:—

"With regard to the privileges of the universities, it must be remembered that they were of old ecclesiastical corporations, and that their testimonial or diploma *confers no civil or municipal right, except in so far as is allowed by statute or usage*. In the words of the Court of King's Bench, in the case of *West*, 'Testimonials from the university, upon taking the Doctor's degree, have the nature of a recommendation; they may give a man a fair reputation, but confer no right.' On that ground the Court of King's Bench, both in that case and in the case of *Lovett* (1. Lord Raymond, 472; 16 Mod. 354), referred to in the pleadings, determined that a man who had taken his degree of doctor of physic at Oxford could not practise in London, or within seven miles of it, without a license from the College of Physicians which was incorporated by the 14th and 15th Henry VIII., c. 5. Those cases are the more decisive, be-

ause the statute, in erecting that college, had in express terms reserved the privileges of the universities. And we think they may with propriety be referred to as authoritative in this case, as *there is no reason to hold that the law of Scotland differs from that of England with regard to the privileges of universities, at least as to the effect of degrees.* When this court, therefore, in the case of Steel just mentioned, found that persons who had graduated at the university in physic were entitled to practise physic in Glasgow without a license from the defenders [i.e. the Glasgow Faculty], we apprehend the judgment proceeded, *not on the ground that the diploma [i.e. the university degree] per se conferred that privilege* to the exclusion of all corporate rights, but, on the contrary, that it proceeded on the ground *that the charter of the defenders contained an exception to that effect*,—an exception, as already observed, confined to medicine, and not extending to surgery."

It seems quite undeniable, therefore, that university degrees are regarded in the same light both by the law of England and by that of Scotland—viz. as "*testimonials*," conferring an honourable distinction, "*but no right*;" and; with respect to certain expressions in the speech of Lord Chancellor (Cottenham), in affirming the judgment of the Court of Session in the last Glasgow case, which have been supposed to countenance the idea that university degrees do constitute, *per se*, licences to practise medicine, it is only necessary to peruse attentively the whole speech, and to observe the distinction drawn in the concluding part of the above excerpt, to perceive clearly the limited sense in which these passages in the speech are to be understood. The matter under consideration being the nature and effect of the Glasgow Faculty's Charter (an obscurely expressed document of the sixteenth century—*more than a century before medicine was taught in the University of Glasgow, and nearly as long before it was taught in Edinburgh*), his Lordship is there reciting the terms of that charter, commenting upon the extent of the exclusive privilege thereby conferred, and distinguishing what was reserved or excepted therefrom, *within the district of country embraced in the charter*, and in another passage is rehearsing the terms of the decision of the Court of Session in the unreported case of Steel referred to by the consulted Judges—and is *not pronouncing any judgment or opinion on the abstract nature and value of university degrees.* No exception being taken to the law on that subject, so anxiously laid down in the learned opinion above quoted, his Lordship must of course be understood to have acquiesced in that opinion to the fullest extent.

Again, with regard to the act of last session, so far from repudiating the principle that the nature and purpose of universities is to confer honours, not rights of practice, the act is in reality a confirmation of that principle. The object of that act was to place the graduates in medicine of the University of London upon the same footing in regard to the right of practice as those of the Universities of Oxford and Cambridge. What that footing may be, it is not for the Council precisely to determine; but an act of Parliament having been required for the purpose, it is obvious that the powers which it conferred could not have been inherent in the "nature" of a university. It is most important also to observe that the act, which was sought for in redemption of a pledge given by the Government at the institution of the London University, was carefully limited to that particular university, and was still further limited by a clause to exclude surgery, midwifery, and pharmacy, from the departments of medical practice to which, in the case of that university, its provisions were to extend.

It appears, therefore, to the Council, that the framers of this bill have made out no case to show that the Universities of Scotland can confer rights of practice in any department, while it is clear that the legislature has, in August last, solemnly refused to sanction the claim then made by the London University in relation to three important departments, constituting the greatest part of the business of the general practitioner, by the insertion in its bill of the clause above noticed.

The Council would now invite the attention of the College to the probable effects of the concession of the claim of the universities to be constituted, for the first time, licensing bodies in *all departments of medical practice*.

And the Council must remark in the outset, that as university degrees have the advantage over ordinary medical qualifications, of conveying a certain rank and an honorary title, objects naturally desired by medical men, it follows that if universities were permitted, in addition to these, to confer that legal right of engaging in general practice which was so recently refused by the legislature, the possessors of those degrees would have no motive to seek any additional qualifications, and the entitled qualifications conferred by a variety of Boards in this country would be gradually extinguished. It is true that, in page 4 of the prefatory "statement," the authors of the bill have asserted that "no existing institution will suffer unless it ceases to enjoy the public confidence." In opposition to the opinion thus hazarded by the framers of this draft bill, the council think it quite certain that, in the very unequal competition which the bill would introduce between universities conferring both the honour and the license, and medical and surgical incorporations conferring the license only, the latter would suffer seriously; and they see no good reason why the universities, which have their own separate emoluments, should be permitted to absorb those derived from the exercise of the licensing power, which, both in this and other colleges, constitute the principal support of museums and other expensive establishments, and are absolutely necessary to the maintenance and improvement of their examinations. How small is the value of the protection alleged to arise from enjoying "the public confidence," is manifest from the known fact that worthless degrees have been disposed of largely, both in this country and in those foreign universities which the framers of the draft bill seem to regard as models for imitation; and that the institutions which now carry on, or have carried on, this traffic in times not long gone by, have conferred many such degrees on medical men now living in this country.

As one of those public bodies of professional men to which the legislature has intrusted certain definite powers of conferring rights of practice, this College may indeed be called upon to relinquish its privileges for the public good; but is bound to defend them against all encroachments which are not founded on this high consideration; and as it is obvious that the legislature ought to be guided by this principle in granting or recalling all delegated powers and privileges, the Council invite the particular attention of the College to the comparative advantages, in a public point of view, of conferring rights of practice by means of universities, or by means of the incorporated bodies of the profession itself.

In making this comparison, the Council will assume, not merely that the amount of control over the course of study proposed by clause 7, and the visitations of the examining boards proposed by clause 11 (supposing that these could be rendered practically effective), would be willingly acceded to both by the universities and by the medical incorporations, but that the latter, which now confer the license to practise, would be willing, as this College always has been, to defer to each other's just claims, and to consent to such changes as the legislature in its wisdom may suggest, in order to make them the instruments of the profession for self-government, and more effectually to identify their aggregate interests with those of the profession and of the public, under any bill of medical reform.

Now, the first remark which occurs to the Council is, that the medical profession, like the other learned professions, ought to be permitted, through the medium of some organization accessible to its members, to govern itself, and determine the conditions of admission to its ranks; and that other parties, having interests separate from the profession, ought not to be permitted to interfere in this matter, however respectable they may be, inasmuch as their interference would necessarily be subversive of this just and reasonable claim

of the members of the medical profession. This objection applies to all the universities in the kingdom, though there are important distinctions among them in the degree in which they possess the medical element ; some having as many as eight or nine practitioners of medicine and surgery in their ranks, while others have only one ; and one university, that of Durham, has *not even one*, and is believed to be almost purely an ecclesiastical institution. A proposal to confer professional qualifications on lawyers or divines, by means of universities and their degrees, and especially by means of such universities as do not include one single member of those professions respectively, would be justly derided by them ; and the Council do not see that the medical profession, so well fitted by its intelligence for self-government, ought to be asked to submit to this.

But there are other grave objections to the constitution of the senates of the universities, regarded as Boards for licensing medical practitioners, for which the bill does provide, and *can* provide, no remedy. There are universities in which a senate, to a certain extent under the influence of the Crown, but at the same time self-elected, and totally irresponsible to the medical profession, selects examiners for medical degrees. In these universities it is at least possible that improper examiners may be excluded. But in those universities in which the power of examining is claimed, in right of their professorships, by the members of the medical Faculty of each, there can be no effectual remedy against incapacity from age, or infirmity of body or mind, nor against capricious lenity or harshness, nor against the wildest extravagances of conduct on the part of individual members of the Faculty, even if they should think fit to lend the sanction of a professor's name to undisguised charlatanism, in defiance of their colleagues. Those who are acquainted with the history of the universities must admit that these evils are far from theoretical. They have in fact been loudly complained of by most efficient and capable university professors, and there is not one of them which could exist for a single season in any Board deriving its powers from a college or other incorporated body of members of the medical profession.

There is a third class of universities, of which Durham and St Andrews are examples, in which there are either no medical professors, or only one, and in which the examiners of medical graduates are selected by the professors of law, literature, theology, and general science, and brought from distant places because they cannot be had at the sites of those universities. Boards thus constituted are anomalies even in conferring mere honorary degrees ; but when it is gravely proposed to convert them into boards for conferring licences to practise, the profession has a good title to be listened to when it remonstrates against such a humiliation.

From all these objections to the exercise of the licensing power by universities, its exercise by the profession itself, or by boards chosen by such portion of the profession as is now incorporated or may in future be incorporated for that purpose under the provisions of a reform act, is altogether free. Such boards possess in their very constitution the principle of self-improvement. The examiners, selected annually by bodies accessible on easy terms to all respectable medical men, and consisting of capable judges, could not retain their position if they became liable to any serious objections.

There is yet one important point in this bill on which it is necessary to offer some remarks, viz., the constitution of the council (clause 3d), which is to be chosen by Her Majesty with advice of the Privy Council, and is to consist of a cabinet minister, with 12 members of the medical profession ; 6 being from England, 3 from Scotland, and 3 from Ireland. With the exception of the relative numbers, which would perhaps give too great a preponderance to the southern division of Great Britain, there can be no objection to this arrangement, if it were coupled with a provision for securing that the choice of the medical councillors should be made from among a certain number of persons selected by the different bodies of medical men throughout the kingdom. But this

seems to be necessary, to prevent the Crown from being misled in its choice by considerations of political favour, or by the advice of individuals, who however estimable and even eminent, might not happen to enjoy the confidence of their profession, or might have become estranged from it by the pursuit of partial or even selfish views and interests, condemned by the sense of the profession at large, and subversive of its just rights and respectability.

On the other clauses of this bill there is little room for remark. There are in its framework many things of which the college has on former occasions expressed its approbation. A good bill of medical reform ought, in the opinion of the President's Council, to secure uniformity of education, examination, and privilege, through the medium of those colleges and incorporated bodies of medical men which now exist in this country, and by means of examining boards chosen by them. It should effect this under the superintendence of a general council such as has been indicated in the bill, with those important modifications of its constitution suggested in the preceding paragraph. Were this done, there ought to be no second examination in order to acquire the diploma conferred by this and similar bodies, which they ought to grant on the simple report of the examiners. The fellowships of colleges ought also to be open to all who hold diplomas, by a simple ballot without additional examination. The fees for all those grades should be liable to be fixed by the general council under the reform act, with a due regard to the professional purposes to which these fees have been and may in future be applied; and the council should have the power of controlling their application. The degrees of the universities may be superadded as they now are by those who desire them as titles of distinction.

In conclusion, the President's Council regret to be obliged to express so unfavourable an opinion of this draft bill. Much as they would have preferred a good understanding with its framers on the subject of medical reform, they conceive that this college would ill discharge its duties to the medical profession if it did not offer the most determined resistance to a bill which, without being in any degree beneficial to the public, is full of provisions injurious to that profession, and in the opinion of the council, would make its condition worse instead of better.—By authority of the College,

ARCHD. INGLIS, *President*.

REPORT submitted by the COUNCIL to the FACULTY of PHYSICIANS and SURGEONS of GLASGOW, on a Draft Bill for Regulating and Improving the Medical Profession; presented on 22d March, and adopted by the FACULTY on 6th of April 1855.

The president having learned that a "Draft Bill for an Act for Regulating and Improving the Medical Profession," was under the consideration of Her Majesty's Government, lost no time in applying to the Home Secretary in the name of the Faculty for a copy, which with a prefixed explanatory statement was at once supplied. The Fellows of Faculty are thus afforded an opportunity of expressing their opinion on the provisions of this document.

The bill is understood to have emanated from certain professors of the University of Edinburgh, and before entering on its merits the council would take the liberty of remarking that it is neither likely to reconcile the differences which exist in the profession on the subject of medical reform, nor is it courteous to the various corporate bodies, who for centuries past have been invested with the legal right to grant licences for practice—that the members of any institution, having no such right, should frame in secret a measure for reforming the profession, and use their influence to get such measure taken up and adopted by the Government.

The council having attentively considered the various provisions and proposals of this bill, and the effect which, in their opinion, it would have on the

profession, beg to submit the following report thereon, for the consideration of the Faculty.

The bill provides for the appointment of a medical council for the United Kingdom, which is to consist of one of the Secretaries of State, and twelve councillors. By an interlineation with the pen in the copy forwarded from the Home Office, only eight of these twelve must necessarily be members of the medical profession—six are to be resident in England—three in Scotland, and three in Ireland, and the whole are to be appointed by the Crown.

Your council are decidedly of opinion, that the principle of representation ought to be adopted in the election of a national medical council; but this is not the place to discuss either the extent or the best plan of carrying out such representative principle. They cannot recommend you to accede to the introduction of non-medical councillors, with the exception of a Secretary of State, as chairman. They can see no reason why the medical profession should not be allowed to regulate its own affairs, as is the custom with all other professions and callings. The members of the profession must, certainly, be better acquainted with its requirements and duties, than persons who have never devoted themselves to its studies, and who could have little community of feeling with the medical majority, on the important subjects which they would require to consider and regulate.

The interpretation clause of the bill proposes that the words "medical practice, or medical profession, shall include and signify the practice of medicine, surgery, midwifery, and pharmacy," and the 8th clause provides "that the degrees, or diplomas, or licences, as physicians, or surgeons, or apothecaries, of all the Universities in Great Britain and Ireland, and other corporations within the kingdom, at present authorised to confer such, shall be recognised by the council as qualifications for registration, with a view to practice." These clauses, coupled with the paragraphs numbered 2 and 3 in page 2 of the explanatory statement, your council consider, would enact that any person holding any medical title whatever, from any university, or corporation authorised at present to grant the same, would be entitled to practise medicine and surgery, in the most comprehensive sense of these terms, in any part of Her Majesty's dominions.

Of the twenty institutions enumerated at page 12 of this draft bill, and proposed to be recognised as licencing bodies, no fewer than eleven are universities, several of which have very incomplete schools for teaching medicine, and one of them has none at all. It is admitted that Oxford and Cambridge already possess the power to licence *their graduates* to practise in England; which power they very rarely exercise. To the other nine universities it is now proposed to grant a new privilege, viz., that of licencing general practitioners, under the appellation of doctors of medicine, on exactly the same terms as the chartered medical corporations, which have never assumed to grant a higher title than surgeon. Your council cannot for a moment believe that the universities, generally, could approve of such an enactment. It would place what has always been looked upon as the highest medical title (M.D.), which confers an honorary distinction, and implies a higher qualification, both as to general and professional education, on the same level as the licence of a surgeon or apothecary. Besides bestowing a new and most uncalled for privilege on the universities, such an enactment, with the semblance of liberality, would be most detrimental to the licencing corporations, in so far as very few candidates would apply to them for a surgical diploma, if they could obtain the degree of doctor of medicine conferring the same privileges, at the same age, and on the same curriculum of study.

An attempt is made in the explanatory statement to uphold the erroneous opinion, that a medical degree from a Scotch university confers the right to practice; and the decision in the lawsuit which this Faculty had with the University of Glasgow is quoted in confirmation of this opinion. The fact is, that the privileges, if any, conferred by a Scotch degree in medicine were not com-

prehended in the point at issue on that occasion, but simply whether the title of "Magister Chirurgie," which the Glasgow University commenced about thirty-five years ago to confer, was equivalent to the diploma of the Faculty and it was decided unanimously, both by the Court of Session and House of Lords, that it was not. In corroboration of the opinion that a Scotch degree confers no legal right to practice, Dr Christison, Professor of *Materia Medica* in the University of Edinburgh, when "particularly charged with the interest of that university," stated, in evidence before a committee of the House of Commons in 1848, that the degree of doctor of medicine from a Scotch university conferred "no legal privilege, but has been received by courtesy as qualifying for physicians' practice." (Minute of Evidence, p. 5, 1848.) Your council have also good authority for stating, that the same opinion was held by many of the medical professors in our universities.

The other clauses of this bill empowering the medical council for the United Kingdom to determine the extent of education and examination, and to fix the age under which no licence to practise shall be granted, conferring on every person legally licenced, the privilege of practising in any part of the United Kingdom or of Her Majesty's dominions, are such as any attempt at medical reform in the present day must necessarily comprehend. The system proposed for registration and the penal clauses would undoubtedly be very serviceable to the public and the profession, but it might be necessary, should the bill ever come before Parliament, to oppose the tendency to centralisation which it contemplates.

The two leading features of this proposed bill, viz., vesting the appointment of the medical council of the United Kingdom unconditionally in the Crown, and bestowing on the Scotch and other universities the right of granting licences for general practice, on the same terms as to age, education, and examination, as the incorporated bodies of the profession, creating in short no fewer than nine new licencing boards, ought in the opinion of your council to be resisted by every means in your power.—In name of the Faculty,

A. DUNLOP ANDERSON, *President*.

THE WAR.

MEDICAL EVIDENCE BEFORE THE SEBASTOPOL COMMITTEE.

(Continued from page 357).

Dr Andrew Smith continued.—He had been forty years in the service, and during that time he was nursed in, or forced into, the idea, by successive Parliaments, that he was to save money, not spend it, on the Medical Department. When this war "broke out," and he found he had actually permission for the first time to spend money, it was months before he could arrange about it. Witness did not believe the reports of the press, as he received none whatever of a like kind. When asked a leading question, if they were "half" as bad as represented, Dr Smith observed, smilingly, he could not say, as "half as bad" was an indefinite sort of expression. His reports chiefly referred to the difficulties the medical officers had to contend with in the discharge of the medical duties, changing every day. The purveyor was the chief person to blame. As early as April of last year, Dr Smith wrote to the military secretary of Lord Raglan, directing bands of men to be sent to remove nuisances, which would be sure to accumulate in the hospitals. Dr Smith could not be in two places at once; but if at Scutari he could not do more. His letters being medical, were disregarded.

On the second day of his examination, it appeared from the evidence of Dr Smith, that there *never* was a want of lint; and from his statements on this day, as well as on the previous one, it was shown that he had opposed, instead

of suggested, the broken-down pensioners being sent out; but Colonel Tulloch and Colonel Maule had so partaken of Mr Hume's economic views, that they forced him to have the old pensioners. The want of medicines after the flank march was partly due to the medicines being left behind in Turkey, at Varna, like the ambulance carts. In answer to a question as to the conveyance of the sick, witness read specific directions sent by him on the 11th of May, in which it was ordered that two ships should be fitted up for the sick, which was accordingly done. The *Severn* was one of the ships; one was arranged to be sailing forward to Balaklava, while the other was sailing back from Scutari. The chairman of the committee, who seemed particularly anxious to find fault with everything bearing on the doctors or surgeons, here suggested that many of the medical officers were unfitted by their age from adequately performing their duties; many objections were made previously by the committee as to want of experience. Dr Smith, in common, it would seem, with the general public in the committee-room, thought that the objection was as to youth and want of experience, and asked if it was "too young" Mr Roebuck intended, who replied, laughing, "no, too old!" Dr Smith said that of all his medical officers, but two were under fifty, four were thirty-nine, and as he was proceeding to go down to thirty, he was abruptly stopped. Mr Ward, a purveyor or apothecary was certainly older; but he was appointed under very peculiar circumstances; they could get no one else who had the least experience of organizing a medicine department or "druggist's shop," and, as Mr Ward had been in the Peninsula in the capacity of apothecary, he was appointed. He was subsequently, like every one else, over-worked, but Lord Raglan thought it unkind to dismiss him. The military authority is the superior authority; so as Lord Raglan did not order him home, Dr A. Smith had no power to do so. With respect to rations for the troops, Dr Smith had recommended the coffee to be first roasted or "burned," and not given green, but was overruled by Mr Sidney Herbert. Dr Smith also recommended porter, ale, preserved vegetables, and lime-juice against scurvy; also large quantities of tea, as more exhilarating and manageable for the troops than coffee; but he was overruled in these also, as more "economy" in medical articles, he was told, should be observed. As to the statements in the papers with regard to Dr Lawson, Dr Smith thought that he would be compromising the service, or rather the confidence which existed between himself and Dr Hall, Dr Lawson, and other doctors, by reading letters strictly confidential. Mr Roebuck said quite sharply, however, he should give them all up; there was nothing the committee should not have if they wished it. To this Dr A. Smith doubtfully assented.

In this day's examination, also, Dr Smith said that it was never made out who applied, if any body ever did apply, to the French for the loan of lint. The committee, too, seemed evidently to think a letter he read from Mr Read, second-class staff-surgeon, in care of the hospital at Scutari, quite refuted the writings of Mr Stafford and Mr Maedonald, who had stated the contrary. The witness here went into a statement of the medical stores left behind in Turkey; representations, he believed, had been made to the Admiralty, but he could only address that board *through the Commander-in-Chief*. All surgical appointments, he next said, were made by Dr Hall, subject to the approval of Lord Raglan. If a senior medical officer arrived, the junior would be deposed: clearly so, as otherwise there could be no discipline, or inducement in the way of promotion in the army. This partly explained also why so many officers were coming and going, as represented by the *dilettante* tourists in the public press. A medical commission was sent out for the purpose of inquiry in October or November last. Mr Sidney Herbert did not allow Dr Smith to direct them or draw up medical instructions; he saw some such directions, but that was all. Mr Arnott was to have gone out, but, after some inquiry, he disliked so troublesome a business. Dr Smith suggested that civilian surgeons should be sent to Smyrna. He thought Smyrna as healthy a place, "taken altogether," as could be decided on; but as his suggestions of one superior

military medical officer over the establishment, in place of Dr Meyer, was not followed, he asked Mr Herbert permission to give up all further responsibility in the matter. A divided and irresponsible authority could not but work mischief. All he finally arranged to do was to provide, for the time being, a sufficient supply of medicines and medical comforts at Smyrna, which he continued to supply. The hospital is now established; it was represented to him as an exceedingly healthy place—particularly favourable, in fact, for the healing of wounds, and free from epidemics, except a little intermittent fever about September. Two German physicians, in particular, represented it to the War Office as desirable in a sanitary point of view. 529 medical officers were sent out with the army; of these, 28 had died, 58 were disabled or invalided, and there are now 469 with the troops in the East. Of medical purveyors there were six or seven in the Crimea. That is enough. Sometimes the War Office appoints the purveyors, sometimes Dr A. Smith. This department was, in fact, abolished as a matter of "economy," in the year 1830; so that when the war broke out we had to take any of the old men of the Peninsular war that we could procure. The purveyors in the colonies—in Malta, and at Corfu—were also all suppressed; and simple regimental hospitals established instead. Dr Hall first went to Scutari to examine into the hospital after the battle of Alma. He reported that there were great difficulties, as Scutari was only a large barrack; but considering everything, the hospitals were in a satisfactory state. (Dr Hall's report was here given into the committee). As to Dr Hall's order to the army surgeons, relative to chloroform, Dr Smith would have done exactly the same, as he thought a man severely wounded might sink under the additional depression of chloroform. The medicines, he continued, were supplied partly by Apothecaries' Hall, and partly by two druggists. No place could be suggested at first for a medical dépôt, as it was all uncertain what would be the scene of action of the army; but when it was no longer a secret that the Crimea was the spot, he fixed at once on Scutari as a general dépôt. That was done in September last. The medicines, of course, were under the care of the chief apothecary, and the medical comforts in charge of the purveyors. Dr Hall reported that packages of medicine were lying on the beach. The vessel which brought them went to Varna in a hurry, and they were left unclaimed, but were all safe, and taken to the Commissariat Office. In answer to various other questions, Dr A. Smith said the Ordnance were responsible for this. The medicines were supplied quite correctly by the druggists, and were examined in London by a medical officer of his department, usually engaged in that manner. Next, as to the nature of cholera, in which Mr Layard seemed to possess a considerable "smattering," Dr A. Smith did not think, in answer to various questions, that the reports of the London Board of Health, on that subject, were of any use. Next, as to the ambulance carts, he designed them himself. Mr Guthrie's carts were never in the Crimea at all—they were of no use, as they continually upset with the poor soldiers! General Torrens, who was himself carried wounded from the field of Inkermann to Balaklava, spoke very highly in favour of Dr Smith's *ambulances*. Objections, in fact, appeared from people in the public press, who knew nothing at all about the matter. It was said that they were too narrow; but to put a soldier, badly wounded, with compound fractures, into a cart large enough to roll about in, would kill him. You cannot stow several men into any fancy cart of small size. His ambulances are the size of omnibuses; and several surgeons in the field had sent reports, which he could furnish the committee with, speaking highly of them: 40 lighter ones, and 60 carts, are going out; 100 pair of litters, and 100 pair of chairs, after the French pattern, were ordered. Some conversation ensued as to transport ships, of no immediate medical importance, but where, as usual, the "authorities," and the contractors for ships, in spite of the surgeons did as they pleased. The duty of providing blankets, for neglect of which Dr A. Smith was censured, was not his duty at all, but the business of the purveyor. As to the matter of "orderlies" for the hospitals, witness recommended

Mr Sydney Herbert to procure 400, but only 100 were obtained; all that could be got as volunteers were sent out by him to the hospitals, in December. He also advised that a regular corps of orderlies should be permanently embodied; this was now doing, and all the preliminaries arranged. In answer to a question by Mr Layard, as to Dr Smith's opinion of the mission of Miss Nightingale, he said he had no hesitation in believing it was highly beneficial, as in laundry-work, cooking, etc., women would discover numerous things that might escape military men. Next, as to disinfectants; charcoal, he said, was procurable in abundance, and chlorides of lime, soda, etc., were all sent out in large quantities early in the campaign. Of course, disinfectants alone could not change the state of things described by Mr Stafford, as the "necessaries" or water-closet were all rotten. The "commandant" and the "purveyors" were the responsible persons; the poor surgeons themselves cried out as much as anybody else as to the state of the "privies."

Dr Smith, in continuation, said he had no reason to consider the apothecaries' department at Scutari as not efficient. As to the want of winter clothing, every effort was made to procure fur coats, fur caps, trousers lined with flannel, etc., but they could not be got up in time. Dr Hall stated that, even after warm clothing was sent out, it could not be got up to the camp from Balaklava, or it might have saved many lives. From the state of political movements at the time, and Lord Aberdeen's peace policy, Dr Hall was led to believe the troops would not remain at all, during the winter, in the Crimea before Sebastopol. Dr A. Smith was next examined as to the proportion of medical men sent out with the army, compared to the numbers in previous wars, and replied that the number was more than double. Some of the most romantic plans—india-rubber cushions, etc.—were suggested to him, while the patients were lying in the mud, more dead than alive, only requiring common assistance. As to a reorganization of the medical service, Dr Smith had received hints, but he wished to be allowed to retire, as he could not hold the place with the mixed organization that was contemplated. Again, as to the hospital at Scutari, he said he could not believe it was so bad as represented. The moment shirts were wanted, in consequence of the "kits" being left behind, 50,000 were ordered; they were lost in knapsacks: in fact, Lord Stratford gave the medical officers everything they might require. As to the treatment of dysentery, a great deal of communication took place between our medical men and the native Turkish doctors. The Duke of Newcastle did not inform him the army would take the field at all. An ambulance corps of Turks was suggested, but it was thought they would run away when they heard the firing. The medicines, instruments, etc., for the last year had cost L.40,000, an immense sum, but the medicines were mislaid and lost by the commissariat.

Dr A. Smith was again examined on the 26th inst. After some preliminary observations as to his not being made acquainted with the division of the "War" department into two, last year, and his not knowing whether he was to address the Duke of Newcastle or Mr Sidney Herbert, it appeared, amongst other changes, he did not recommend a "general" hospital, because that followed only as a matter of course when an army takes the field. He described again the difficulty of finding a proper purveyor, and Mr Wrefford, who was sixty-four years of age, was the only one to succeed Mr Ward, but he was very efficient. [Dr A. Smith here objected to giving up "private" letters between himself and Lord Raglan, but was overruled by Mr Roebuck.] The evidence was next very desultory as to the character of the wine sent out, but it seemed the War Office had been deceived. Specimens of the wine were procured and tested by a medical board in London, and reported to be good. Next, as to bedding: he had ordered 14,000 beds, which were sent out by the Ordnance; he had a proper person "down at the ships all day," to see them properly embarked, as well as the various medical comforts. Next, as to ships: he had one to leave this country concurrently with one leaving Constantinople; but at this time all was confusion in the political world, not knowing where

the troops were to be next sent to. As to lime-juice, which, it was said, was neglected, 40,000 gallons were sent out as early as November; it was used all through the winter, after the battle of Inkerman, amongst the troops. Witness believed, also, there was quite enough of linen and lint for operations. He was next asked by the committee as to splints, when Dr A. Smith gave the following most extraordinary opinion, but, we believe, too true: "Almost every medical officer and surgeon in the army has his own specific idea about splints. Nothing but novelty will now go down: one surgeon will say the splints ought to be eleven inches long: another thinks thirteen; another twelve. I sent out 400 sets of splints of all sizes! which ought to last an army six years; but when they arrived, even, every man had his own new-fangled notion." As to the cleanliness of the hospital, that is entirely non-medical, or in the purveyor's department, as the surgeon in an hospital to-day may be hundreds of miles off in a week. 5000 hospital dresses (trousers, waistcoats, and flannel jackets) were sent out by Dr Smith, but the chief difficulty, as he explained, arose from the army being ordered from Malta to Gallipoli, then Scutari, then Varna, and back again to Scutari. However, as soon as the Crimea was decided on, that, as he said before, was fixed on as "head-quarters." In the returns from Scutari, showing the washing in December to be 14,000, in November, 5000, sheets, shirts, etc., were put in. There were no complaints of the cooking made in the reports sent to him. His information from Scutari, led him to think that there were sufficient surgeons there. Once or twice it was said the surgeons were very much over-worked, when he was induced to send out more surgeons. Next, as to the principle of private contributions to military hospitals, and whether authority should be divided amongst various persons, Dr A. Smith said, Decidedly not; and that Miss Nightingale was not allowed, and very properly, any control over the medicines. As to medical appointments, six or seven applicants offered to go out, but their services were not thought of value. One offered his services *gratuitously*! He (witness) offered young surgeons ten shillings a day salary, three shillings for rations, and L.40 for outfit, and he got from the commencement 150 to 200. He had since received most favourable reports of their conduct. Some discussion next ensued as to a want of quinine, and also as to a letter by Dr Burrows with respect to the medical head-quarters being established for the future at Scutari, with some other matters not of medical interest. Dr Smith, amongst other inaccuracies of writers, also protested against a mis-statement of Mr Osborne, that Dr Spence, drowned in the *Prince*, was his son-in-law. Dr Gordon, Assistant-Surgeon Browne, Dr Taylor, Dr M'Dermott, on the contrary—all strangers—were promoted by him for good conduct. Next, as to "panniers;" one was sent with every regiment, and twenty or thirty to spare; so the French are not singular in this matter. The witness, before withdrawing, stated he should probably have some other observation to make, on another day, as to mis-statements like those relative to Dr Spence, who was no relation whatever of his. Dr A. Smith's evidence, on both days, gave very general satisfaction.

Dr Menzies, Deputy-Inspector of Hospitals, was next examined.—He had had an hospital in India of 150 beds under his care. Went as first-class staff-surgeon to the East—to Scutari, last April. He was general superintendent of the Barrack Hospital, supplementary to the General Hospital; both were under him; the former was a "make shift," and even the latter was defective from the day they entered it. He reported to the "commandant," for the information of Dr A. Smith and Dr Hall. He told Major Sillery the rooms were in a filthy state; that was in August. From June to January he had the entire charge of the two hospitals at Scutari; then (before the battle of the Alma) the privies were cleaned out, and the wards whitewashed. He had at that time 200 patients in the General Hospital. He had a hulk hospital like the *Dreadnought* for 500 more convalescents. He had twenty-one staff-surgeons; he considered one surgeon could attend fifty or even eighty slight cases. Witness here read a letter recapitulating many of the facts we have

already noted in *The Lancet*, as to Mr Ward, etc. Bedding was daily expected at Scutari from Varna, but did not come, and he had to borrow 200 boards and trestles from the Turkish authorities. It happened just at this time that Lord Redcliffe arrived to inspect the hospital, and observed this want of beds. He took Turkish surgeons into his employ after the battle of the Alma. He sewed sheets together, and filled them with straw, for beds, after the battle of Inkermann. He reported this to Dr Hall. Dr Menzies stated that his own duties were overwhelming—in hospital all day, and frequently up the whole night; plenty of writing also, so that inspection of other hospitals was out of the question. As to clean linen for the hospitals, the purveyor had persons, he said, cleaning it; but after the battle of Inkermann these people would never do anything. Sick arrived in hundreds, and the surgeons were overwhelmed. The Barrack Hospital soon became more filthy; the purveyor lived in the hospital; but the accounts in the public press were all overdrawn. He never saw faeces at all on the floor—this was quite a mistake. He was next examined, or rather cross-examined, as to whether, as the men's pay was stopped while in hospital, they got the value of it in medicine and medical comforts; to which he made the sensible answer, "they all got such supplies as the medical officers considered necessary; but if a patient was labouring under bad inflammation, it would be improper to give him the full value in wine or brandy for which he would ask." One man might have been mistaken for another in some one solitary instance as to the number of his bed; but this was the fault of the purveyor living in the hospital, not the surgeons'; such a thing never came to his knowledge. The Commission of Inquiry came out on the 4th of November; they are going over matters yet. Miss Nightingale proposed to see to the washing, but he told her it was the purveyor's business. During all November we had plenty of medicines; during the entire time he was there he found no deficiency of medicine. The witness was asked several other questions as to the landing of the sick, which was done as well as could be under all the circumstances.

Mr W. H. Flower, surgeon of the 63d Regiment, was examined on the 27th, and corroborated many of the points already published. The strength of his regiment was 980 men. The medicine-chest was left at Varna for want of transport; the opium, though supplied in large quantities, was soon exhausted from the dysentery and diarrhoea amongst the troops; in fact, purgative medicines were left on hand; they never regained their medicines from Varna! His regiment had ten stretchers and two ambulance waggons; the men's "packs" were left on board when they landed in the Crimea, of which they complained very much; but this was a strategic move of Lord Raglan's. After Alma he had sixty sick and wounded a day. Operations were done "out of doors," and under every possible disadvantage; even the *new* hospital tents were supplied rotten, and let in the wet; the sick came in wet; all was damp and dreary. Medicines were deficient at Balaklava. The knapsacks were not given to the men, but it was a "regulation of the service." As to chloroform, he disapproved of Dr Hall's order, and used it.

Dr Menzies was here recalled, and explained some points of no immediate medical interest. He said he employed seven Turkish physicians, and in August thought all was right.

Dr Menzies, Deputy-Inspector-General of Hospitals, was re-examined. He said he had direct charge of the general but not of the barrack hospital. It was Dr McGregor's duty to report to witness the state of the hospitals. His reports at times were not satisfactory. He stated that he had not sufficient assistance in the way of orderlies, and witness supplied them as far as he could. Witness never sent away some medical men who came to offer assistance after the Alma. As early as July he wrote to Major Sillery on the offensive state of the barracks and the necessities. Major Sillery applied to the engineer department, who altered the drains; but they got out of order again, and the evils were never thoroughly remedied. On the arrival of the transports, the

worst cases were selected for removal to the general hospital. There was a want of sheets, linen, cleanliness, or attention to the sick and wounded at the Alma. A card, bearing each patient's name, was placed over the patient's bed. The sick had three meals a day. Great delay occurred in getting the patients' meals—their dinners especially; and they suffered much in consequence. He had never seen *feces* flowing over the privies into the adjoining ante-rooms. Mr Stafford never represented the state of the privies to him. About two-thirds of the deaths from operations in the hospitals arose from amputations of the lower extremities. He was quite satisfied that none of the deaths of the wounded on whom operations had been performed were to be ascribed to the bad sanitary state of the hospital. The position of the French hospital at Constantinople was one of the most healthy in that city, being on elevated ground which overlooked the Golden Horn. The reason that the English hospital was inferior in that respect was, that no better could be procured than that at Scutari. The French had better opportunities of obtaining hospital accommodation, being on the other side of the water. Lord Stratford had visited the hospital twice, and at witness's request the medical officers received him in undress uniform, and not in full uniform, as had been stated; but their being so dressed did not interfere with the discharge of their duties at the hospital. Witness considered Admiral Boxer to be the agent of transports. He always treated witness very well, and was anxious to carry out his wishes as well as he could, except when he landed the sick at improper times, and then witness remonstrated with him, and some improvement took place, but not to the extent that witness wished. The admiral was to blame for not attending to the proper time for landing the sick. He had seen Mr Macdonald, but he (witness) never asked him a question. He did not apply to the *Times*' fund for assistance, because he did not think there was a necessity to do so. The articles that Mr Macdonald furnished might have been supplied, though not perhaps to the same extent, if they had not previously been purchased by that gentleman at Constantinople. He was not aware that Mr Macdonald applied to him more than once to know if anything was wanted in the hospital, and witness might have told him that nothing was wanted.

On Wednesday (March 28th) the Committee sat again, and the examination of Dr Menzies was continued: When he said on the previous day that he "did not know his duties," he meant that his position was anomalous. When Dr Cumming came out, witness considered him his superior officer. Dr Cumming gave directions in the hospital, and witness supposed that Dr Cumming superseded him for the time being, and that he had no right to dispute Dr Cumming's orders. In the strict letter of the law, being himself responsible, he ought not to allow another to do that for which he alone was responsible; but still, witness considered that he should obey Dr Cumming as his superior officer. This was his view, from long military medical experience. Witness questioned the truth of Mr Osborne's statements about the want of tables for operations. Sometimes the operations were performed in the sight of other patients; but some allowance must be made for the state in which the hospitals then were. Men did not die in the hospital of starvation, as Mr Osborne had said. These statements were highly overdrawn. He made the best arrangements he could for the hospital. An officer was specially appointed to see the embarkation of the Russian prisoners for Kululee, and witness was present on the occasion. Witness denied that the Russian prisoners had been insulted in the way described by Mr Osborne. A Russian medical man accompanied them. He was not aware that the Russian prisoners at Kululee slept on bare boards. He did not go there to see the state of their hospital, as his duties at Scutari did not allow him. He believed that the Russian prisoners had bedding, and could not believe it to be true that they slept on bare boards. Mr Wrefford was aged about sixty-three. Mr Wrefford did not satisfy witness. He opposed witness's authority, of which witness complained to Dr A. Smith. Witness would give evidence unqualifiedly contradicting Mr Os-

borne's statements that men were miserably dying of starvation, and want of clothes, and medical aid. He had more to do than merely writing reports,—he had to go round the hospitals to see that the medical officers did their duty, and to attend all important operations. Witness informed Dr Smith that Mr Wrefford did not carry out his recommendations. Mr Wrefford questioned witness's right to interfere in his office, and when witness consulted him as to some of the sick officers, he turned round and said that witness had nothing to do with it. Witness frequently operated himself at the hospitals, but generally divided the operations with a second-class surgeon. When witness was absent, first-class staff-surgeon Carr attended at the general hospital, and Dr McGregor at the barrack hospital. There was not a sufficient number of medical officers, but the patients did not suffer to any considerable extent, as the medical officers, though short in number, did a great deal of work, night and day. Many died in the East after the operations, but that was because they were in so low a state that they would have died without the operations, and the operations were performed merely to give them a chance of life. In a private letter to Dr Hall in December, he stated how the nurses were going on, and that they were very useful in washing the faces and hands of the sick and wounded, and in washing the linen, and attending to their comforts; but that at present their number was too small to enable them to judge of their efficiency. The hospital was then in tolerable order, cleanliness, and comfort; and Miss Nightingale observed to him that she was surprised at the regularity and comfort that appeared to prevail in every ward in the general hospital. Witness applied to Captain Gordon to have the flooring of the barrack hospital repaired, and Captain Gordon undertook to attend to it. There were 2000 patients in the barrack hospital in November, and the purveyors were supplied with clean linen before Miss Nightingale's arrival. He had not heard that some of the wounded remained for days in the hospital at Scutari with the earth on their wounds which they had got in the field of battle. The returns made to witness of the deaths in the sick transports were numerical, not nominal. In some cases he never received any returns of these deaths at all.

Dr Dumbreck, Deputy-Inspector of the Hospitals at Constantinople, said he arrived there on the 15th of April. He established a general hospital at Varna. He did not choose that hospital—it was assigned to him. It was objectionable in many respects. The windows were like pigeon-holes, and the privies were indescribably filthy, and many parts of the hospital were covered with vermin. He remained in charge about a month, and was succeeded by Dr Hall. The food in the hospital and camp, and the bedding in the hospital, were satisfactory. At first, when the regiments began to arrive in Scutari, no purveyor's stores had preceded them there; and what sick there were had food and comforts from the commissariat. The comforts were not such as a purveyor would have supplied. After the battle of the Alma, at which he was present, he saw no want of medicines or surgical appliances, as far as came under his observation. He left the camp before Sebastopol on the 13th of November, up to which time there were no wants amongst the troops which were not supplied; but there was a prospective scarcity of fuel. The green coffee sent out would be certainly prejudicial to health. He sent to the apothecary at Scutari for a fresh supply of opium, and he got it. He believed he applied for 50 lbs., and he got 30 lbs. Of lint, bandages, and things of that kind, there was a superabundance. The army never suffered on that score. Witness saw much in the ambulances to approve of, though some features in the French ambulances might be advantageously engrafted on them. Witness was only thus far responsible for Dr Menzies' appointment: when Dr Hall left Scutari, Dr Menzies was on the spot, and was senior officer in the medical department, and he succeeded Dr Hall as much from being on the spot as from any other consideration. Witness made many suggestions calculated to prevent disease amongst the troops, but they were only very partially adopted; one of them

was to clothe the army in woollen from September until May. The charge that there was a want of lint, bandages, and medical comforts after the battle of Inkermann, was a calumny. An ample supply had reached the lines the day after the battle. He often visited the Balaklava hospital, and never saw it in the condition described by Mr Stafford. Witness had professionally inspected several transport ships for the sick, especially the *Etrunga* and the *Talavera*, both of which were in the best possible order. He had provided 400 mattresses and 2500 palliasses for the sick in these transports; he had intended to have procured more palliasses, but could not get hay to stuff them with. After the battle of Inkermann the bedding in the sick transports failed. Many of the ships were supplied with medical comforts according to lists ordered by witness. He also ordered wine to be placed on board each transport. He visited the hospital tents regularly. The hospital tents were well adapted for summer, but not for winter. Trained nurses (such as Miss Nightrigale had embodied) and trained orderlies were desirable, and in great numbers. The routine services the medical officers had to perform, the accounts they had to keep, and the great amount of writing they had to do, most seriously interfered with the discharge of their medical duties.

Mr Drummond asked witness if that had not been the principal cause of the neglect that had taken place in the medical departments?—Witness replied that he did not admit that any neglect had existed.

The witness considered the maintaining of regimental hospitals in time of peace to operate disadvantageously to the establishment of general hospitals in time of war.—*Lancet*.

CHLOROFORM IN THE WAR HOSPITALS.—Chloroform was always used, and it appeared to me with the greatest success,—which I attribute a good deal to the practice of using it on a handkerchief held lightly to the face instead of the plan I have seen elsewhere of using some instrument, which, whilst it secured the inhalation of the anæsthetic, excluded too much of the atmospheric air. I assisted at one very painful case, in which a branch of the femoral artery had to be taken up. There were great difficulties about it, so much so that one of the best surgeons there did not seem to me to like to attempt it; it was however done by Dr M'Ilroy, assisted by some others. I am afraid to say the length of time the patient was under the influence of chloroform; his head was on my own knee the greater part of the time, and I had to keep up the administration of this inestimable agent; at last they succeeded in getting a ligature round the vessel. I was then left with one of the surgeons to try and recover him from the torpor under which he had, without pain, borne a most severe application of the knife, etc. Our only hope, from the quantity of blood he had lost, was to get some stimulant taken as soon as we could. In vain we tried every means of rousing him—the pulsation of the heart was so weak, his whole appearance such, I had begun to despair. As a last resort, I found out his name, and had him sharply spoken to by it. So strong was the force of habit that he made just sufficient effort to waken to enable us to order him to drink the wine we gave him. Keeping up the same sharp military tone of voice, we got more and more swallowed, and he soon recovered.—*Scutari, by the Rev. S. G. Osborne*.

MEDICAL APPOINTMENTS IN THE EAST.—Dr Robert D. Lyons of Dublin has been appointed to the important scientific duty of investigating the pathological conditions of the diseases which are now making such havoc among our men in the East. Dr Lyons is already on his way to Scutari.

Dr William Aitken, Demonstrator of Anatomy in the University, and Pathologist to the Royal Infirmary, Glasgow, has been appointed assistant-commissioner at the hospitals on the shores of the Black Sea.

NEW REGULATIONS FOR THE MILITARY HOSPITALS AT SCUTARI.—The representations of the Council of Health, in respect to the improper mode of burial

practised at our military hospitals, has been attended with the best results. Drs Sutherland and Gavin, and Mr Rawlinson, have determined that no burial should take place but at the distance of 100 yards from the walls enclosing the general hospital. But one range of bodies, with an intervening space of 12 inches, is to be placed in a trench; the trench to be at least of the depth of 4 feet from the surface. A layer of coal dust to be laid in lieu of quick lime over the bodies, and all interments to take place in future in the morning early, or evening, and not during the heat of the day. In respect to the round already used up, a stratum of moist coal dust is to be thrown over it, and it must be pressed down, levelled, and scattered with grass seed.

TURKISH CONTINGENT.—Dr Macpherson, chief medical officer of the corps, has lately visited Edinburgh for the purpose of securing surgical volunteers. The terms offered were as follows:—25s a day for surgeons who must be under thirty-two years of age; 15s a day for assistant surgeons with diplomas to rank at first as first-class dressers; and 10s 6d a day for dressers in their second or third years of study, who have dressed in an infirmary, to rank as second-class dressers. To proceed to Constantinople without delay. Pay to commence on day of appointment. Free rations. Free passage out and back. Fifty-one days' pay in advance. Twelve of these various grades were required. For the inferior dresserships, however, there were 40 applicants, of whom thirteen were selected, and the full complement of surgeons was at once obtained. A few appointments for the assistant-surgeon-ships were still vacant when Dr Macpherson left town, but these were in prospect of being filled up.

LUNATIC ASYLUMS IN SCOTLAND.

The Queen has been pleased to direct letters patent to be passed under the great seal appointed by the Treaty of Union to be kept and made use of in place of the Great Seal of Scotland, appointing Samuel Gaskell, Esq., Fellow of the Royal College of Surgeons; William George Campbell, Esq., Barrister-at-Law; Alex. Earle Monteith, Esq., Advocate, Sheriff of the Shire of Fife; and James Doxe, Esq., Doctor of Medicine; to be her Majesty's Commissioners for the purpose of Inquiring into the state of the Lunatic Asylums in Scotland, and also into the present state of the law respecting Lunatics and Lunatic Asylums in that part of the United Kingdom.—*London Gazette*, April 10.

We trust that the inquiries of this commission may lead to the provision of public asylums for all the pauper lunatics of Scotland. By the act 4 and 5 Vic. c. 60, it was enacted that all parish paupers should be sent to a *public hospital or asylum*, unless under special circumstances and cause shown, the Sheriff of the county should permit any case to be sent to a private or licensed madhouse. The poor-law act, however, subsequently passed, which rendered it imperative on all parishes to send their pauper lunatics "to some establishment legally qualified to receive them," appears to have rendered the clause referred to in the former act inoperative to a great extent. In the first place, it multiplied the cases requiring to be sent to asylums so much that the existing public asylums could no longer receive them; and, in the second place, it seems to have been understood by the parochial authorities and Board of Supervision as a virtual abrogation of the clause giving a preference to public asylums. The consequence has been that during the last ten years a great number of private asylums have sprung up in Scotland, which compete eagerly with the public hospitals and with each other—sending their circulars to all the parochial boards of the country offering to take their pauper lunatics at the lowest possible rates of board. It is much to be feared that this competition has been fostered by the urgent desire of parochial boards to keep down the assessments for the poor; and that the pauper insane have, from motives of economy, been consigned to houses where they are deprived of nearly all the

means of cure and alleviation of madness, which the enlightened spirit of the age demands for this class of unfortunates. These are the causes, we believe, which have led to the appointment of this commission. It does not imply any distrust of the existing public asylums of Scotland, which have long been acknowledged to have taken a lead in all the improvements introduced in the management of the insane.

NATURAL HISTORY CHAIR.

As government has come to no decision with regard to the appointment of a successor to the late Professor Edward Forbes, Dr Traill will conduct the course during the present summer session.

VARIETIES.

THE LATE EMPEROR'S PHYSICIAN.—The *Danube*, a Viennese journal, informs us that the Emperor Nicholas' physician, Dr Mandt, has secretly quitted Russia. Among the causes of his sudden departure are mentioned his having too long concealed from his august patient the fact of his lung having become affected. The doctor is a homœopath, and has been much blamed for personally compounding the medicines for the Emperor's use, instead of allowing his prescriptions to be made up by a *pharmacien* in the usual way. Much irritation exists at St Petersburg on the subject, and the Emperor Alexander, it is said, had to advise the physician to quit the country.

GERMAN UNIVERSITIES.—During the past winter 18,201 students matriculated in the 28 universities of Germany; 847 regular professors, 253 professors *agregés*, 46 honorary professors, and 450 masters of particular subjects and languages: in all 1699 persons superintended the instructions. Considerable variation has been observed in the number of students; thus, during the winter of 1851-52 the number rose to 19,354, the summer following it was 17,810; in the winter of 1852-53, 18,596, and during the succeeding summer 17,905. The total number of strangers attending these universities is estimated at 2711.—*Cologne Gazette*.

STUDENTS ATTENDING MEDICAL LECTURES IN PHILADELPHIA.—Philadelphia may justly be termed the medical centre of the United States, so far, at least, as the great number of medical students which annually assemble there, can entitle her to that honour. The catalogues of the present term, in the several schools, exhibit the following totals—University of Pennsylvania, 350—Jefferson College, 500—Pennsylvania College, 120—Philadelphia Medical College, 100—Homœopathic Medical College, 80—Female Medical College, 50. These constitute an aggregate of twelve hundred students.

EFFECTS OF THE EXTREME COLD ON LIFE.—The season of extreme cold has now passed over; and its effects have been seen in the tables of the last six weeks, when the deaths of 9408 persons have been registered. These deaths exceed the average by 1968; which appear under various diseases, and were the indirect results of the low temperature. The temperature of the six weeks was 28.4° on an average, and the deaths were nearly 100 weekly to every degree of depression below the freezing point of water. But the cold affected persons very differently, according to their age; for in the five weeks that ended on Feb. 17, at the first age of manhood (20 to 40) the cold did not destroy 2 in 10,000; at the age of 60 to 80 it was fatal to 38 in 10,000. If the average deaths at each of the five ages are subtracted from the deaths in the five weeks of cold weather, the numbers that are left representing the deaths

by cold are 807 children and youths under 20 ; 519 young men and women of 20-40 ; 290 middle-aged persons of 40-60 ; 561 of 60-80 ; and 173 of 80 and upwards. Upon dividing these numbers by the persons living of the corresponding ages, we find that the mortality by cold in the 100,000 was at the rate of 35 under the age of 20, and 18, 64, 382, and 1749 at the four subsequent ages. The above numbers show that the power of cold on life varies according to definite laws ; thus the mortality by cold is (35) twice as great under the age of 20 as the mortality (18) at 20-40 ; but after that turning point, the power of resisting cold decreases every year, and men of 90 and men of 30 have suffered from the cold that we have experienced in the proportion of 100 to 1 (or of 1749 to 17.5). The general result is, that the danger after 30 of dying of cold is doubled every nine years of age ; for out of the same numbers living, to 1 death by cold at the age of 30, there are 2 at 39 ; 4 at the age of 48 ; 8 at the age of 57 ; 16 at the age of 66 ; 32 at the age of 75 ; and 64 at the age of 84. This series at least expresses very nearly the relative mortality by cold at the respective ages during five weeks among two and a half millions of people.

HORSE FLESH AS AN ARTICLE OF FOOD.—M. Geoffrey St Hilaire has this year devoted a portion of his lectures on zoology, to the consideration of that subject with regard to its economic applications. Hitherto this department of knowledge has been confined, for the most part, to mere theory ; and while other branches of science have been contributing towards the well-being of the community, zoology in this respect has been nearly at a stand still. Among the questions discussed by the lecturer, one which has particularly attracted attention, is that of the applicability of horse flesh as an article of food. After quoting a number of the most eminent authorities—Larrey, Cadet, Parnentier, Pariset, Parent Duchatelet, etc.,—in support of the quality of this substance in an alimental point of view, he proceeds to show that, in full conviction of its value as a wholesome nutriment, its sale is authorised by the Governments of Denmark, Belgium, and Austria ; and while its quality is such as to recommend it for use, the quantity in which it may be obtained cannot fail to arrest attention. The question then, says M. St Hilaire, resolves itself into this—are these animals to be lost, as is the case now in France, by millions, while their flesh affords a material so highly nutritive and wholesome ; and that, too, at a time when, for want of some such azotised diet, we find the poor in the same land, reduced in health and strength, of mind as well as of body, and, deprived of animal food and even of bread, miserably subsisting on chestnuts and potatoes ? M. St Hilaire concluded by stating that he did not look for this article taking a place with beef and mutton, in their present state of perfection ; but argued that it should, at least, find a place on the poor man's table, and that there its utility would be first and most fully established.

DIETETIC PROPERTIES OF FISH.—There is much nourishment in fish, little less than in butcher meat, weight for weight ;—and in effect it may be more nourishing, considering how, from its fibre, fish is more easily digested. Moreover, there is, I find, in sea fish, a substance which does not exist in the flesh of land animals, viz., iodine, a substance which may have a beneficial effect on the health and tend to prevent the production of scrofulous and tubercular disease—the latter in the form of pulmonary consumption, one of the most cruel and fatal with which civilised society, and the highly educated and refined are afflicted. Comparative trials prove that in the majority of fish the proportion of solid matter, that is, the matter which remains after perfect desiccation or the expulsion of the aqueous part, is little inferior to that of the several kinds of butcher's meat, game, or poultry. And if we give our attention to classes of people—classified as to quality of food they principally subsist on, we find that the ichthyophagous class are especially strong, healthy, and prolific. In no

class than that of fishers do we see larger families, handsomer women, or more robust and active men, or a greater exemption from the maladies just alluded to.—*Dr John Davy's Angler and his Friend.*

SISTERS OF MERCY.—Sick nurses have duties to perform which require an immense deal of patience, gentleness, and tact. It is not sufficient that they be free from positive coarseness and brutality, their office requires of them a degree of tenderness and sympathy, of which by no means all women can boast. It is not, indeed, to be expected that nurses in a public hospital should exhibit the same feeling and devotion that one may hope to find in the domestic circle, but certain qualities as a minimum they should invariably possess, and by a certain control, as an influence substituted for natural affection, they should constantly and carefully be ruled. But such checks, the very nature of "Sisterly" institutions seems to us to exclude. A paid nurse obtains her place by virtue of proved qualifications, and she holds it on condition of good behaviour. But here, the mere fact of a woman being a member of a certain community, is supposed to endow her at once, and as it were, by a miracle—so much so, indeed, that her future conduct shall be beyond all challenge—with the gift of competently tending and comforting those committed to her charge; while this is looked for, moreover, in the case of women, whom, generally speaking, personal circumstances of one kind or another (but these, in the majority of cases, of a nature too surely calculated to sour the milk of human kindness in them), have been driven to seek a refuge for themselves in those societies of supposed benevolence to others. Nor is there a check on misconduct any more than on incompetence. A great improvement it would doubtless be, that the sisters were subject to some competent, efficient, accessible, and independent authority. But such is not the case. To complain of them would be worse than useless. I doubt if the superintendent could interfere even were he willing to do so, which he certainly would not be, unless to the influence of the order he could oppose the backing of some equipotent power. The sisterhood seems altogether beyond every jurisdiction save that of their own hierarchy, and its members are very well pleased when an opportunity occurs of showing how they appreciate such a position. I have heard a sister sneeringly of a patient, and in his hearing. "Number Ten threatens to speak to M. Sandras about it; he thinks he will frighten me, poor fool that he is, as if I cared for M. Sandras!"—*Life in a Paris Hospital.*—*Hogg's Instructor.*

FLANNEL.—An essay on the action of flannel in direct contact with the skin, and the influence it exerts in a physiological, pathological, and therapeutical point of view, has been published by Dr Fiévée de Jeumont; the aim of the author being to direct attention to the indiscriminate use generally made of this material, without sufficient enquiry into the indications or contra indications for its employment. Considerable importance is attached by him to the qualities of this substance in relation to electricity, believing, as he does, that its agency in this way is sufficiently powerful to exercise an influence upon the nervous system, calculated in some instances to induce a diseased condition there. However, in the employment of an article such as this, much must be left to the judgment and discretion of the medical man in attendance on the individual case; and although the remarks of Dr Fiévée are interesting theoretically, it is questionable whether they may be of much service in practice.—*Gaz. Med.*, March 31.

MESMERISM.—A novel case has just been decided in New York, which involves a curiosity in medico-jurisprudence. A mesmeric physician sued a husband for services rendered the wife in his absence. The Supreme Court says that the law does not recognise the dreams, visions, or revelations of a woman in a mesmeric sleep as necessities for a wife, for which the husband, without his consent, can be made to pay. These are fancy articles, which

those who have money of their own to dispose of may purchase, if they think proper; but they are not necessities known to the law, for which the wife can pledge the credit of her absent husband. The law does not seem to have much respect for mesmerism and spirit-rapping as sciences.—*New York Medical Times*.

SCARCITY OF MILITARY SURGEONS IN FRANCE.—Most of the military surgeons now in Paris are ordered to proceed immediately to the army. M. le Directeur de l'Assistance Publique is to nominate several physicians and surgeons in civil practice for appointment in military hospitals.—*Gaz. Med.*

PARIS EXPOSITION.—The jury for the departments of hygiene, pharmacy, medicine, and surgery, is composed of the following members:—*Jurés titulaires*—MM. Rayer, member of the Academy of Sciences and of the Academy of Medicine; Nelaton, clinical professor in the Faculty of Medicine; Mèlier, member of the Academy of Medicine, and of the Committee of Public Health in France; Bussy, member of the Academy of Sciences and of the Academy of Medicine, director of the School of Pharmacy; Boule (Henri), professor in the Veterinary School of Alfort. *Jurés suppléants*—MM. Tardieu (Ambroise), professor agrégé of the Faculty of Medicine, member of the Committee of Public Health in France; Demarquay, surgeon of hospitals.—*Gaz. Med.*

MEDICAL HONOURS.—The following surgeons have, by an imperial decree dated March 10th, been authorised to accept and wear the order of the Médjidie, which has been conferred on them by his Highness the Sultan:—2d class, M. Michel Lévy, inspector-general of the board of health of the army in the East; 4th class, M. Perrier, chef de l'ambulance; 5th class, M. Buschaert, aide-major.—*Gaz. Med.*

BISCHOFF.—This celebrated physiologist, so well known by his researches in embryology, is about to leave the university of Giessen for that of Munich. The latter institution will thus have deprived that of Giessen of two of its greatest ornaments—viz. Baron Liebig and M. Bischoff. We also learn the nomination of M. Lange of Heidelberg to the chair of midwifery in the Faculty of Medicine at Prague.

CONFINEMENT WITH HARD LABOUR.—Dr R. M. Graham, who was convicted of manslaughter some time since in New York, was last month removed to the State prison to undergo his sentence of seven years. He will be employed as assistant physician of the prison.

A NOVEL MEDICAL FIRM.—In one of the leading thoroughfares of Paris, the following inscription, on an attractive sign-board, arrests the attention of the passers by:—"Consultations gratis: from 8 A.M. till noon, treatment conducted on the principles of ancient medicine; from noon till 4 P.M. on those of homœopathy; and from 4 P.M. till 8 P.M. on the method of M. Raspail." What follows fully explains the charitable nature of the consultations: "There is a pharmacy on the premises."

BEQUEST TO IRISH HOSPITALS.—The Right Hon. James Grattan, son of Henry Grattan, has bequeathed the interest of L.8000—one half to the Meath Hospital, Dublin, or failing it, the Cork Street Hospital, and the other half to the Queen's County Infirmary, Maryborough. The motive assigned for this benevolence is a fear that the testator's "long-neglected and unhappy country was about to be again afflicted with disease and pestilence, and that government proposes to reduce the grants to charitable institutions."

PUBLICATIONS RECEIVED.

- On the Use of Creosote in Scorbatic Camp Dysentery.** By John Bramstone Wilmot, M.D., of Caius College, Cambridge, and F.R.C.V. London. 8vo. Pp. 16.
- A New Plan of Healing Ununited Fracture.** By Henry H. Smith, M.D., Consulting Surgeon and Lecturer on Clinical Surgery in the Philadelphia Hospital. With eight Woodcuts. Philadelphia. 8vo. Pp. 20.
- Pathological and Clinical Observations respecting morbid conditions of the Stomach.** By Dr Handfield Jones, F.R.S. London. Coloured Plates. 8vo.
- Biographical Sketch of the late Dr Golding Bird, being an Address to Students, delivered at the request of the Edinburgh Medical Missionary Society.** By J. H. Balfour, M.D., F.R.S.E., Professor of Medicine and Botany in the University of Edinburgh. Edinburgh. Small 8vo.
- Memoirs of the Life and Writings of the late Dr R. J. Mackenzie, F.R.C.S.** By J. Warburton Begbie, M.D., and John Struthers, M.D. Reprinted from the Edinburgh Medical and Surgical Journal for April 1855. 8vo. Pp. 44.
- On the Nature, Signs, and Treatment of Child-bed Fever, in a series of Letters, addressed to the Students of his Class.** By C. H. Meigs, M.D. 8vo. Pp. 562. Philadelphia, 1854.
- Illustrations of Medical Evidence and Trial by Jury in Scotland.** 8vo. Pp. 64. Edinburgh.
- The Micrographic Dictionary.** By J.W. Griffith, M.D., F.L.S., and Arthur Hefrey, F.R.S., F.L.S. Part vii.
- An Expository Lexicon of the Terms, ancient and modern, in Medical and General Science, etc.** By R. G. Mayne, M.D., etc. London, 1854. 8vo. Part iv.
- On Electro-lithotrixy; or the application of the Mechanical Force of the Electrical Discharge to the disintegration of Stones in the Bladder.** By George Robinson, M.D., etc. etc. 4to. Pp. 16. London.
- The Pathology and Treatment of Leucorrhœa.** By W. Tyler Smith, M.D., etc. 8vo. Pp. 217. London.
- The Quarterly Journal of Public Health, and Record of Epidemics and Hygiene, including the Transactions of the Epidemiological Society of London.** Edited by Benjamin W. Richardson, M.D. London. No. 1.
- Report on the Results of the different methods of Treatment pursued in Epidemic Cholera presented to Parliament.** 8vo. London, 1855. Pp. 28.
- Report on the Common and Model Lodging-Houses of the Metropolis (with reference to epidemic cholera in 1854).** By George Glover, Superintending Medical Inspector, General Board of Health. Presented to Parliament. London. 8vo. Pp. 34.
- Letter of the President of the General Board of Health to Lord Palmerston, accompanying a Report from Dr Sutherland on Epidemic Cholera in the Metropolis in 1844.** Presented to Parliament. London, 1855. 8vo. Pp. 120.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Army Medical Department and the Introduction of the "Civil Element" into Military Hospitals.* By Sir GEORGE BALLINGALL, Regius Professor of Military Surgery in the University of Edinburgh.

(*From an Introductory Lecture to the Course of Military Surgery, May 1855.*)

IT is not with military men that the difficulties of the medical department have heretofore generally arisen, but with the underlings of those numerous collateral offices which have so long been permitted to impair the energies and exhaust the strength of a War Department in this country. A man who will not hesitate to storm a breach, or to head a charge of cavalry, such as that of the light brigade at Balaklava, will think twice before he opposes the opinions, or impedes the operations of an intelligent and experienced surgeon. Nay, such a man will be the very first to listen to any respectful and reasonable suggestion touching the health of his men. If such things as I have cursorily noticed can be done by a regimental medical officer,—who, if he knows his duty, and chooses to do it, may be a very independent man,—if such things can be done by a regimental surgeon, what ought to be the influence of a man of energy, experience, and decision, at the head of the department?

The trammelling of the medical department has been a growing evil, although spoken of by many as something new. The limited powers and want of independent action has been, more or less, a standing and a just cause of complaint ever since I knew anything of the service; but in spite of this, we have often had the duties of the department carried on with success, and we have had men amongst us, more than one, who, if an independent action was not conceded to them, did not hesitate to take it.

Amongst these, I am tempted to mention a name which will probably be new to most of you—the name of my late friend, Mr Young—and I do this the more willingly, because he is little beholden to posthumous fame, in consequence of never, so far as I know, having written anything for publication. This gentleman, in

his regimental days, was a predecessor of mine in the Royals, where his name was long held in respect, and he spent the evening of a long and laborious life in this neighbourhood, at Rosetta, near Peebles, where he had built himself a residence, and borrowed its name from the scene of his former labours in Egypt. He was at the head of the medical staff upon two memorable occasions, and it was said to his praise, that "the worst calamities of war had no place either amidst the swamps of Holland, or on the burning sands of Egypt." And it was said some four-and-fifty years ago, with reference to his conduct, and to the point of independent action, that, "In what concerns the health of an army, the praise or blame must, peculiarly and distinctly, belong to the medical superintendent; because the events then, whether prosperous or adverse, must depend upon causes of which professional skill alone is competent to take cognisance. The hospitals, of course, must be just as much under the Inspector-General as the arrangements of the field are under the Commander-in-Chief, and consequently any peculiarity of success in the recovery of the sick and wounded is as much to the appropriate praise of the former, as the wise array of a battle or a siege is to the distinct honour of the latter."

Mr Young, gentleman, was a man of the stamp of Larrey, to whom he was well known in Egypt, and who inquired most kindly for him, when he visited this city. He was a man who saw no obstacles in his way, who stuck at nothing for the benefit of the sick, and who suffered no inroad on the rights of his department. When chief of the staff in the West Indies, a young doctor was sent out to him as physician to the forces, with the king's commission and an Oxford or Cambridge degree in his pocket, the only ones then qualifying for that rank. Mr Young declined to receive him, telling him that he could not allow those gentlemen who had been toiling under him as staff and regimental surgeons to be superseded by one who had never before seen a sick soldier. The young man, seeing that there was no room for him in that quarter, requested the Inspector to give him an order on the paymaster for some money, and on the agent of transports, to carry him home. The reply was, "I will not acknowledge you by any official act whatever." But, said Mr Young, I happen to have some money at my credit in the paymaster's books, and whatever you want I will most willingly give you.

The gentleman found his way back to England, and Mr Young soon after followed. He was ordered to repair to the Medical Board, and there he found the physician-general and the surgeon-general (neither of whom had any previous service to lean upon), in great indignation at this insult to their authority, and the former remarked upon Mr Young's courtesy to the physicians to the forces, to which he calmly replied, that if the thing was to do over again he would just do as he had done. The surgeon-general then showed his temper, and observed that they had not made up their minds

whether they would not bring the whole proceeding before a court-martial, to which Mr Young, taking up his hat, and making his bow, said, "the sooner the better." The court-martial, however, was no more heard of; they knew that he had the feeling of the service with him, and that he had Sir Ralph Abercromby at his back. I appealed for the truth of this anecdote to Mr Young himself, stating it to him as I had heard it, and as I now repeat it to you. He quietly observed that it "was very near the truth."

This leads me naturally to say a word on the introduction of the "civil element" into the military hospitals. It will not be supposed that I, who lived and practised so long in harmony with my professional brethren in this distinguished seat of medical erudition—who have now been so long an atom in this "civil element"—who, amongst those who have closed a brilliant career, have been often in consultation with such men as Gregory, Abercrombie, and Liston—who have had the honour to rank amongst my colleagues in the University, such men as Thomson and Charles Bell, will be found wanting in respect for the civil branch of my profession. The civil members of the profession have evinced a most generous spirit in the way in which they have espoused the cause of the assistant-surgeons of the Navy; and I am sure they will sympathise with those men who have been spending toilsome days and sleepless nights under canvas in the Crimea, and are now made the scapegoats for errors committed at home. It grieves me to think that these men should find themselves, at the close of a campaign, supplanted by others who have not borne the "burden and heat of the day." Could I believe that this was for the good of the public service, I would speedily be reconciled to it. But is it to be supposed, that men who have, like myself, been accustomed to see their hospitals broken up soon after midnight, to make a march of twelve or fifteen miles, and to have their hospitals again in operation by the time they sat down to breakfast, and this from day to day for weeks in succession—is it, I say, to be supposed that men conversant with such duties as this, are less competent to the organisation of new hospitals than those who have passed perhaps an hour a day in the simple duty of prescription?

I have all along maintained that there never was a want in the army of the Crimea of men equal to the higher duties of the department; but instead of seeing those men promoted to a higher rank, which they have so well earned, and appointed to what would have been to many of them an easy duty, they are superseded by men who, whatever may be their merits in other respects—and these I have no desire to question—have never hitherto had an opportunity of giving an opinion on the position, construction, or economy of an hospital—and all this at an increased expense to the nation. How far this is calculated to attract talent to the public service, to encourage merit, or to benefit the sick soldier, it is for the Government to judge.

It is quite clear that a sufficient number of hands (to use a seaman's phrase) could not be spared from the Crimea, to man these auxiliary hospitals; but, with the diminished numbers and improved health of our army in that quarter, occasion might have been found for the promotion of some half-dozen of staff-surgeons, to be placed at the head of them; and I make no doubt that many of the young gentlemen who have volunteered for the duties of those hospitals would have preferred serving under men of rank, standing, and experience in the army. What is it, I should be glad to know, that is required from the civil hospitals? is it those limited powers often imposed upon physicians and surgeons by a close-fisted treasurer? is it those delays and impediments to improvement occurring from the necessity of a reference to the governors? it is that vexatious interference on professional points sometimes exercised by a philosophic manager? or is it that divided and imperfect responsibility under which medical men have sometimes been enabled to shelter themselves when decidedly in the wrong?

The military hospitals, in my younger days, were looked to as patterns for imitation in the organisation of similar establishments for the purposes of civil life. I have now had some experience of both, and I say advisedly, that although the military hospitals are not in all respects what they might be, there is, in these hospitals, much of that arrangement, promptitude, and self-reliance, which ought to characterise all military proceedings. The quantity of superfluous writing in the medical department has, I am glad to see, been well exposed by my friend Dr Dumbreck, in his evidence before Mr Roebuck's Committee, and I fear that this department has to answer for a large share of the L.70,000 worth of stationery said to have been sent out with the army of the Crimea.

The absurd system of checks and counterchecks, so forcibly exposed by the late Secretary-at-War, would still seem to be in full operation. Of this I recollect a very ludicrous instance, and was in some degree a party to it, when a very young man. The hospital expenditure account was "returned for correction," and the surgeon, the hospital-sergeant, and myself, set our wits to work, and mustered all our joint stock of arithmetic to discover the error, but being unsuccessful, the account was sent back to the Medical Board, and was twice again "returned for correction." As if to make the thing more ridiculous, an orderly dragoon was kept galloping backwards and forwards between the head-quarters of the district and the village where we were quartered, with this precious despatch, and the mighty error turned out to be "an ounce of oatmeal overcharged." Had the clerks in the Medical Board, who at that time checked the returns, condescended to mark, by a cross on the margin or otherwise, where the error lay, it would have saved a considerable loss of time and temper, to say nothing of the wear and tear of man and horse.

This, however, chiefly concerns the public; but there are some

cases in which I fear the medical department has assisted in forging its own fetters. I should be glad to know what has become of all those portly folios which have been accumulating in our regimental hospitals for a long series of years, at a great expense to the nation, great labour to the surgeons, and little edification to the profession. It is no doubt an object of great importance, particularly when men are brought forward to be invalided, to have an authentic record to refer to, showing how often a man has been in hospital, and for what particular complaints; but surely all this might be accomplished without allotting a page or two of those huge folios to every man admitted, compelling the surgeon to spin his brains to give a graphic description of a sprained wrist, or an ulcerated leg, or to detail with equal prolixity the case of one man with a virulent gonorrhœa, and another with a malignant typhus fever.

I know no good that comes of this compulsory writing; but there is another description of writing which I should wish to see encouraged. I know not at this precise moment what are the regulations, or what is the practice of the French army, but I know, that from the medical officers of that army have emanated more than sixty volumes of the "*Recueil de Mémoires de Médecine de Chirurgie et de Pharmacie Militaires.*" This published under the authority, and at the expense of the Government, and containing many valuable papers on subjects all important to the health of the troops. In this, I think we would do well to imitate them. In addition to all other professional competitions open to the military surgeons as well as to others, I should like to see a competition instituted within the department itself. Who will show himself most conversant with the diseases of soldiers and seamen, and with those injuries to which they are exposed in the battle-field, and on the ship's deck? Who will give us the best papers on the medical topography of our many foreign stations, and on the best sites for camps, cantonments, barracks, and hospitals at home and abroad? Who, in short, will evince the most perfect knowledge of all the *juvantia et lædientia* of a military life? A selection of such papers by an impartial committee, and published by the Government, would give encouragement to the department, and health to the army.

With reference once more to this "civil element," for which we are indebted to his Grace the Duke of Newcastle, I would observe, that the expression is somewhat indefinite; and as we are not told how far it is to be carried in the re-organisation of the medical department, I would say that if this element must be introduced into the department, it should be at the bottom, not at the top of the tree. I wonder what civil element actuated Larrey when he killed the spare horses of the officers to make soup for his men. This you will allow was a most uncivil proceeding; but for this, Napoleon made him, on the instant, a Baron of the Empire. The highest prize in the medical department ought to be accessible to the youngest assis-

tant-surgeon who enters the service; and a very paltry prize it is for this great country to hold out to the Chief Medical Officer of its army.

The experiment of introducing the "civil element" into the medical department of our army, has heretofore been eminently unfortunate. Let us revert for a moment to the calamities of Walcheren. There was at that time a respectable old gentleman from civil life at the head of the department, the late Sir Lucas Pepys, who had, I believe, been a successful apothecary, or general practitioner at Weymouth, and had made himself acceptable to George III., when resident there. When called upon to proceed to Walcheren to give his assistance to the sick, he declined to move, sat still in Berkeley Street, and declared, in an official communication, that he could be of no use, and that he knew nothing of camp and contagious diseases but what he had learned from Sir John Pringle's book. To this, it was said by Cobbett, a great political writer of the day, with all the bitter irony of which he was so great a master, that the old gentleman had only one additional declaration to make, that he was unable to draw his salary.

On another occasion, when the ophthalmia spread far and wide amongst the soldiery in this country, after the return of the army from Egypt, when the civil part of the population became alarmed for their eyesight, and when an enormous burden had been thrown upon the public by the number of men pensioned for blindness, a distinguished oculist from civil life, the late Sir William Adams, was placed at the head of a large and expensive ophthalmic hospital in the Regent's Park; what was the result? "It cannot," says Dr Vetch, "fail to surprise every impartial mind to observe, that even from the report of Sir William Adams himself, so far from effecting a national saving of L.60,000 per annum, which he had promised, by a reduction of the ophthalmic pensioners, *not one has been sufficiently benefited to admit of his pension being either reduced or taken away*; and of six soldiers included in the report, all of them cases of opaque cornea, combined with the second stage of Egyptian ophthalmia, *not one has been rendered fit for duty, and all have been added to the list of pensioners.*" It is, most assuredly, not with any idea of under-rating the attainments of my many eminent friends in civil life that I express myself thus strongly, but for the purpose of deprecating what I consider an injustice to the medical department of the army.

It is not, gentlemen, I repeat, from the want of able and intelligent men among the staff and regimental surgeons serving in the army of the Crimea, that that army has in any degree suffered. The want of that army, as of many others, has been in the inferior ranks, indeed in the very lowest grade of the attachés to the medical department—the want of a numerous and efficient hospital corps. In so far as some of the duties of such a corps have been zealously, kindly, and successfully discharged by Miss Night-

ingale and her female followers, I most willingly acknowledge the civil element; and in so far as these benevolent ladies have made up for the want of numerous orderlies, and thus spared the effective force of regiments, I am sure that every commanding officer will feel grateful to them.

Touching the alleged failures in the Crimea, "the medical department failed," says a public writer, "not because Surgeon Brown could not dress a wound, or Dr Jones prescribe for a case of dysentery, but because no adequate preparation had been made for the reception of sick and wounded; because medical stores were sent to one port, while invalids were sent to another; and because purveyors were left to squabble for authority with inspectors, while patients were dying." And pray, whose fault was this? I have the best authority for saying, that had the resources and transport of the medical department been at its own disposal, much of the misery of that army would have been obviated.

One gentleman, I regret to think, has been most severely handled by the public press, not for any want of professional talent, but for apathy and want of interest, with which he was charged by the gallant officer commanding the troops. How far Dr Lawson's health may have been impaired, and his energies prostrated, by a protracted residence on the coast of Africa, for which (with a spirit most becoming in an army surgeon) he volunteered his services, I am unable to say. It is many years since I have seen him, but it is due to this gentleman, and to the memory of his excellent uncle, the late staff-surgeon Badenach, to say, that when a pupil of this class, some twenty years ago, a more steady, correct, industrious, intelligent, and promising student never sat on these benches. Of this you may judge by the following extract from the *Edinburgh Medical and Surgical Journal* for 1835:—"On Monday, the 16th day of April, at the last meeting of the class of Military Surgery in the University of this city, in the presence of a considerable number of the professors of the medical faculty, and most of the medical officers of the Army, Navy, and East India Company's Service, resident here, the professor, after concluding the lecture, proceeded to announce the names of the gentlemen who had obtained prizes.

"By nearly the unanimous votes of the class, after a competition conducted in the presence of the Principal of the University, and numerous professional gentlemen, the individual selected as most distinguished by a knowledge of the subjects of military medicine and surgery, was Mr Robert Lawson, from Perthshire, in which decision the professor concurred, and Mr Lawson was accordingly recommended to the Director-General. We understand that he has since received the appointment so justly due to his merits."

It was, I think, upon that occasion that my predecessor Dr Thomson, in congratulating me on the appearance of my pupils, observed, that Sir James M'Grigor would require to extend his

patronage—enough to show that Dr Lawson did not win his honour without a formidable competition.

There is only one other point, gentlemen, on which I should wish to be indulged with a few words; but as I have already trespassed upon your time, and have elsewhere expressed myself on the subject, I will endeavour to be brief. I have been very sorry to find anything said which is calculated to disparage our regimental hospitals, or any countenance given by gentlemen whose judgment I respect, to the opinion that they are only adapted to peaceable times, and that a forty years' peace has disqualified the medical officers of the army from expanding their views to the management of a general hospital.

This seems to me to be altogether a gratuitous assumption; and I would observe, that it was not in a time of peace, but of war, that the advantages of our regimental hospitals became fully developed. Hear what the late venerable Director-General says upon this subject! In a letter to his friend the late Dr Chisholm, written at the termination of the Peninsular war; after expressing his surprise at the extent and success of the regimental hospitals, Sir James M'Grigor goes on to say,—“However short a time a battalion or a corps rested in one place, a regimental hospital was established. It was frequently established in the face of an enemy, and nearly within reach of his guns. By making every corps constantly keep up an establishment for itself, we could prevent the general hospitals from being crowded. Much severe and acute disease was treated in its early and only curable stage, and no slight wounds or ailments were ever sent off from the regiments; by which means the effective force of the army was kept up, or perhaps increased by several thousand men, and this was effected by the joint exertions of the medical officers who served in the Peninsula, the result of medical science, and their experience of soldiers, their habits, and their aptitude to particular diseases.” Dr Chisholm adds for himself,—“In regimental hospitals, health and economy are united—in general hospitals, death and a destructive waste of money. My own experience, which has been tolerably extensive, justifies this.”

It were superfluous, and it were idle to make farther quotations upon this subject, because I believe that every experienced man conversant with these hospitals, who has committed his opinion to paper, has expressed himself to the same effect. But on an occasion like the present, when the state of our hospitals has been so frequently and so unfavourably contrasted with those of our allies the French, it may not be out of place to refer to the sentiments of Baron Larrey. I had the pleasure of conducting that distinguished surgeon over the establishments of this city, both civil and military, now nearly thirty years ago, and I shall not forget the admiration which he expressed with the state of the regimental hospital in Piershill Barracks, then occupied by the 7th Hussars, and under the charge of an assistant-surgeon, Dr Moffit. Not satisfied with this, he

repeated his commendations to Sir James M'Grigor when he went to London, and wrote back to me to say that he had done so, and that he had recommended to him the gentleman whom he was pleased to term my protégé. Dr Moffit's promotion took place soon after, and he considered it hastened by this kind recommendation of the Baron.

General hospitals, however, are indispensable on every extended scale of warfare, and I believe they never can be more advantageously conducted than by assimilating them as far as possible to our regimental establishments. General the hospital may be, general, as much as you will, in so far as the provisions, the cooking, the washing, the bedding, and the clothing of the sick are concerned, but let us, if possible, have their own surgeons to attend their own men. This may be carried to a great extent by classing the patients according to the divisions, brigades, or regiments to which they belong, having the medical staff of those divisions, brigades, or regiments to attend them, assisted by those non-commissioned officers and good conduct men of every regiment who may happen to be patients in the hospital, and who take an interest in their comrades, which strangers cannot be expected to do. This is a classification, as regards military hospitals, of equal, if not greater importance than some of those usually adopted on purely professional grounds; and the general hospital, whether under one or more roofs, thus becomes, as it were, a congeries of regimental hospitals.

I have already pointed out the difference between the province of the purveyor and the surgeon; and it is remarkable, that it is precisely at the point where the general and regimental hospitals meet, that the duties of a purveyor become paramount and indispensable, while the duties of a medical officer are in no degree changed, except in so far as he has to treat a disease which, having been acute in a regimental hospital, may probably have become chronic in a general one—a change for which surely every medical man is prepared. The purveying of a regimental hospital is for the most part a simple affair, and conducted successfully by the hospital sergeant, under the direction of the surgeon; but when serving in a general hospital, within the reach of daily or hourly communication with a purveyor, the surgeon is happily relieved of this.

So much are my old-fashioned notions in favour of the regimental principle, that I cannot help thinking it might with great advantage have been extended farther in the recent operations in the Crimea. Had the large addition, so strenuously recommended by Mr Guthrie, been made to the regimental, instead of the general medical staff, I see nothing to have prevented the assistant-surgeons of regiments, from having been detached in succession with the sick and wounded, just as the numbers of these increased, and as the number of fighting men diminished; to have succoured and assisted those men on their stormy passage across the Euxine, to have afterwards attended them

in the hospitals on the Bosphorus, and to have returned to their regiments with such of them as might again have become fit to take the field. I am here only referring to what has repeatedly happened to myself. I have, over and again, been detached from my regiment with parties of sick, and it has happened to me to have served, more than once, in general and garrison hospitals, and to have sometimes had a portion of one of them given up to me for a regimental establishment, according to an arrangement which may, I believe, at this moment, be seen in the King's Infirmary, in the Phoenix Park, at Dublin, or at least was to be seen when I last visited that establishment some few years ago.

I know of no duty of a staff assistant-surgeon to which a regimental assistant is not competent, but I do not hold that the converse of this proposition is equally true. I have the highest opinion of that "esprit de corps," which is fostered by regimental intercourse, and those "ties of regimental discipline," which, as Dr Millingen says, "constitute the superiority of battalion hospitals." I know well how much that knowledge of character acquired by a regimental surgeon—that interest on the one hand, and that confidence on the other—engendered between him and his patients, contributes to the successful treatment of disease. I have, myself, been sent for to amputate the limb of a soldier, lying in a garrison hospital, a few miles distant from the spot where I happened to be encamped. This young man, finding that his limb must come off, asked as a special favour that his own surgeon might be asked to operate. This the garrison surgeon kindly consented to, and the young man speedily recovered.

The successful discharge of regimental duties were always looked upon in my day, and, I believe, very justly looked upon, as the best preparation for the duties of the staff, whether military or medical. Where, I should be glad to know, except in the exercise of regimental duties, were such men as Jackson, M'Grigor, Hennen, Guthrie, French, Franklin, and many others prepared for those general duties which they have so successfully discharged in all quarters of the world? Of the last two named gentlemen, the former went to China as surgeon of the 49th regiment, and was placed at the head of the department as the senior medical officer of the Queen's troops employed in the Chinese War. The other was Inspector of Hospitals to the Queen's troops at Chillianwallah and Goojerat, those conflicts in the Punjaub which have given peace to that part of India for many years past. No! Gentlemen, I will not believe that the surgeons of the army are unequal to the conduct of general hospitals.

ARTICLE II.—*On the Effects of the Baths of Creuznach in Female Complaints, especially Hypertrophy and Tumours of the Organs of Generation.* By CHARLES ENGELMANN, M.D., Physician at Creuznach.

(Communicated to the Obstetric Society by Professor Simpson.)

SINCE Creuznach has enjoyed the reputation of a bathing-place of repute, complaints, to which females are subjected, have constituted a considerable portion of cases which have been sent hither for treatment. For their reputation in such cases, our baths are chiefly indebted to Dr Outrepont, Professor of Obstetrics at Wurzburg, who has published a variety of cases that occurred in his practice, and which were either cured or greatly benefited by the use of the Creuznach waters. In fact, its celebrity is no longer confined to Germany. The efficacy of our baths is generally acknowledged abroad, and foreign countries annually send forth a considerable number of patients seeking relief at our springs. During the last ten years, a large number of patients have resorted hither from Great Britain, where the attention of the public has been especially directed to the merits of the Creuznach waters, by Dr Locock, and Professor Simpson, and, I may say, by many other physicians of repute in that country. Unfortunately, however, it frequently occurs that patients are sent here for whose cases the baths are either entirely inappropriate or no longer available; and when the patients consult our local physicians, to their great disappointment they are told that the baths are not adapted to their particular complaints. It is, however, a painful task to have to inform a sufferer that a mistake has been made in his case, and that the expense and trouble of a long journey have been undertaken in vain; but it is, I must repeat, frequently necessary to do so, for so powerful is the agency of mother-lye baths, that they are either decidedly advantageous or positively injurious. The intention of the author, in the following pages, is to endeavour to define the limits within which the Creuznach waters may be considered beneficial, and to point out in which class of complaints favourable results may be reasonably expected to follow from a course of our waters. The following observations are the result of an experience extending over a period of more than fifteen years.

When we bear in mind that the efficacy of our baths is mainly to be ascribed to the combination of muriate of lime, of bromine, and iodine, we arrive at the conclusion that their peculiar efficacy will be more especially manifested in those cases in which hypertrophic enlargement or soluble tumours exist, in which cases the grand object is to induce absorption of the tumour, by increasing the activity of the absorbing vessels.

Diseases of the Mamma.

The great diversity of structure that occurs in tumours of the

mamma, precludes the possibility of assuming that the same treatment can be applicable to all. There are some tumours in which dispersion, or at least relief, may be expected from the baths; others, in which the treatment does not disperse the tumour, but is only employed to facilitate an inevitable operation, and possibly to prevent a relapse; whereas, on the other hand, there are some kinds in which the baths would be positively injurious.

Of the class of tumours which we may hope to disperse by means of the Creuznach waters, all of them are of a non-malignant character. As a general rule, we may observe, that those cases only are sent here, which either, from neglect or other unfavourable circumstances, have failed to yield to local or other remedies, and consequently have assumed a peculiarly obstinate character. It is, nevertheless, far from my intention to assert that other appropriate and active remedies might not, in such instances, have been attended with success if duly administered. Our baths are not to be regarded as a specific, but rather as an appropriate method of treatment; and we must further observe, that it is not every kind of benignant tumour that is curable by these waters, but only such as belong to the following category.

a. *Indurations of Particular Glands*, arising from inflammation of the breast during the period of suckling, or at the time of weaning, or produced by hyperæmia, or else attributable to some mechanical injury, such as a blow or pressure, or lastly resulting from cold or anomalous menstruation. In such cases, it is a well-known fact, that a proper local treatment duly persevered in, insures success. The cases of this nature treated at Creuznach (to which place they are often sent), owed their obstinacy to an erroneous and insufficient method of treatment. The dispersion of such indurations may be confidently expected from the employment of our baths.

b. *Glandular Hypertrophy*.—(Pancreatic sarcoma of Abernethy; lobular imperfect hypertrophy of Birkett; mammary glandular tumour of Paget.) It is not always easy to distinguish affections of this kind from scirrhus, or from other non-malignant affections that are not capable of being absorbed by the baths. The features that have formed my chief guides in arriving at a diagnosis have been—a great moveableness of the tumour, a uniform but not bony hardness, a slow growth (sometimes undergoing no change for years), the simultaneous occurrence of several tumours in the breast, freedom from affection of the glands under the arm-pit, absence of pain during menstruation, and the youth and general good health of the patient. In none of the cases that have come under my own observation, did the tumour exceed the size of a hen's egg. The degree of success attending the bathing-course was in proportion to the duration, rather than the size, of the tumour. Thus, in several instances in which the induration was not above a year's standing, a single course of the baths dispelled it, whereas, other and smaller swellings, but which had existed for four years and longer, were not

cured until the second or even third summer. In many cases, after repeated courses of the baths, and with intermediate treatment between the seasons, the tumour was only diminished, and there remained behind a hard substance or nucleus, which showed distinct fluctuation, and manifested itself as a cyst. In those cases whose subsequent condition came under my own observation after the termination of the bathing-course—a period of 5 and more years—I perceived no tendency to a fresh growth.

c. *Ectasia of the Milk Vessels*.—This kind of tumour is much less frequently met with at Creuznach than the above-mentioned forms. Here, too, the diagnosis is frequently attended with considerable difficulty. The youth of the patients, the unchanged character of the tumour, the manner of its origin after lactation, the inflammatory painfulness attending its first growth, and a want of disposition to unite with the skin or the thorax, served as criteria to distinguish these cases from scirrhus; the circumscribed shape, the depth, the slight degree of moveableness of the tumour, the swelling of the axillary glands during the inflammatory stage, and which afterwards disappeared, furnished, together with the anamnesis, a distinction from partial hypertrophy. All the cases that came under my observation had existed for several years, had not undergone any change for a lengthened period, and might, with ordinary attention, have continued without medical treatment, had not the apprehensions of the patients, that the swelling would assume a cancerous character, given rise to a fear that an injurious influence might be produced on the general health. The tumour varied in dimension from the size of a bean to that of a hen's egg. In every instance, a decrease of the swelling took place, and in some cases total dispersion. It is, however, possible that these last were simple indurations, arising from hyperæmia of individual glands. In most cases the patients were satisfied with having obtained a decrease of the tumour, and relied on the assurance of their medical adviser that the decrease was in itself a proof of the benignant nature of the tumour, as scirrhi were never absorbed.

The baths were always strengthened with gradually increasing quantities of mother-lye, proportioned with the patient's constitution, susceptibility to the influence of medicines, quality of the skin, etc. Local remedies were only made use of when accidental inflammatory affections were to be combated, or when the home physician had directed the patient's attention to the favourable effect of wrappers steeped in mother-lye, or lastly, when the patient seemed dissatisfied if every possible means of remedy were not adopted. I cannot say that such local applications produced any results worth mentioning.

To the class of tumours which cannot indeed be absorbed by the use of our baths, but which otherwise derive benefit from their employments, belong

a. *Sarcoma and Cystosarcoma of the Breast*; also, *Simple Cysts and Cystoids of the same*.—In these forms the baths produce a

reduction of the size of the swelling, but only by the absorption of the cellular substance surrounding the tumour, and which had become hypertrophied. In cases where the diagnosis is certain, this result is expected by the physician, and to obtain this he sends the patient to Creuznach, in order that the unavoidable surgical operation may be materially facilitated by this absorption of the hypertrophied cellular tissue. In cases in which the diagnosis is still doubtful, the object of the treatment will be to render the tumour itself more accessible to investigation by absorption of the cellular substance, and to enable us to determine whether any benefit may be expected from medical treatment, or whether a surgical operation be imperatively necessary. That, in the early stages of this disease, absorption is possible from the use of the baths, I am not prepared to assert, inasmuch as the cases which I have hitherto treated have all been of some standing and considerable size. Neither will I venture to pronounce, as some have maintained, that the bath is a preventive against a relapse. In the cases in which an operation was performed after a course of baths, and respecting which I was enabled to obtain information, no relapse occurred. But the majority of cases were withdrawn from my observation. In two cases in which no operation took place, the tumour remained without alteration since the bathing course—a period of 4 and 6 years. These few observations are not sufficient to warrant the conclusion that our baths have this effect in every instance.

b. *Scirrhus Tumours*.—In these tumours also a decrease in the extent of the induration takes place, but only by the absorption of the cellular tissue. Real scirrhus, even in the smallest form, is as incurable by our baths, as by any other medical treatment. Where there is no doubt respecting the nature of the disease, an operation has been indispensable from the beginning, and should be performed immediately after the baths have ceased. When the diagnosis has been doubtful, another motive for the employment of these baths was to ascertain whether any diminution of the tumour would ensue, and thereby arrive at a more accurate conclusion concerning the nature of the malady. The patients, dreading the operation, had been assured it would not be necessary unless the Creuznach waters should produce no effect. Many practitioners have sent patients suffering from scirrhus here, under the impression that the baths destroyed the disposition to scirrhus, so as to secure them against a relapse. On what ground this opinion has been formed I am at a loss to explain. The physicians of this place have never expressed such an opinion, neither is it to be found in any work of authority. Were such an opinion substantiated by evidence, the importance of such a result could scarcely be too highly estimated. A series of cases, it is true, have come to my knowledge, in which an operation had been performed after a course of the waters, and no re-appearance of the disease had taken place after 10 years, or even more. These, however, were very diminutive tumours, that had not grown

together with the skin, and the general health also was not yet impaired. They bore the stamp of genuine scirrhus, beyond a doubt, and the baths had no effect on their size. But in all cases where the scirrhus had already attached itself to the skin, the nipple appeared retracted, and the axillary glands enlarged, in all such cases where I had the opportunity of following them up, after the operation relapses occurred. Any injurious influence on the scirrhus, or a more rapid growth after a course of baths, I have not observed. It is even possible that the course may produce a temporary check to the growth of the tumour; at least, several professional men of other places have communicated to me that such has been the case with patients whom they have sent here, and who have obstinately refused to allow an operation to be performed on their return.

Of tumours of the breast, on which the waters exercise an injurious influence, must be enumerated

Scirrhus, when it has passed into *cancer*. The injurious effect of the baths, when simple mineral water is used, does not manifest itself during the course; on the contrary, there is an apparent amendment. The warmth of the bath assuages the local pain; the amusements of the place, the enjoyment of the open air, and the hope of recovery, produce an evident improvement on the general health. But, when the treatment has terminated, the development of the cancer is more rapid than before. Its progress is also very apparent during the treatment, if the baths are strengthened with mother-lye.

Affections of the Ovaria.

In selecting Creuznach as a place of cure for affections of the ovaria, mistakes are of more frequent occurrence than in diseases of the mamma, and this arises generally from the difficulty of the diagnosis. The use of our baths must be considered as highly pernicious in cases of cancer, or where great exhaustion exists and hectic fever has manifested itself. Every attempt to cure must in such cases be attended with disadvantage, and the more powerful the means the more injurious will be the result. A palliative treatment can alone be adopted; our object must be to obviate the most dangerous symptoms, to maintain the strength and prolong the life of the patient to the utmost possible limits.

There are other affections of the ovaria in which a moderate use of the baths is not directly injurious upon the local complaint, but may be considered so far unsuitable, as the general constitution is affected by a medical agency having no definite object to act upon. Of such a character are *cysts* of the ovaria, with simple hydrops ovarii, the *cystoids*, *cystosarcomatous concretions*, and *alveolar degeneration* of the ovaria. The results of pathological anatomy must force on us the conviction that in these affections, as soon as dropsy supervenes, there is as little benefit to be expected from the Creuznach waters as from any other medical treatment. Nevertheless, every year patients of this class present themselves at our watering-

place. They are sent here by physicians who had observed the favourable results of our waters in other diseases of the ovaria and the uterus, and who, in the present instance, all other remedies having failed, have determined to try Creuznach again, in the hope of, at least, arresting the progress of the disease. In none of the cases that came under my hands did the bathing-course produce the slightest benefit. In all cases where the diagnosis left no doubt of the existence of cysts, puncture had been repeatedly effected; and even in the least developed cases, the induration of the ovarium was of the size of a child's head. The inefficacy of the mineral treatment was proved by the fact that, after the course, puncture became necessary at the same intervals as previously. In one instance, although the patient had been tapped shortly before proceeding to Creuznach, yet the accumulation of water was so rapid, that I was obliged to repeat the operation during the course. In the case of a Russian lady, who was under my treatment last summer, the effects of the baths were evidently unfavourable. The complaint of the ovarium was of six years' standing. In Berlin the puncture was made for the first time, and repeated after an interval of four weeks; the constitution was not suffering at that time. The patient started for Creuznach one week after the operation was performed. After having taken 20 baths, such a quantity of water formed again, that I was obliged to tap it. Immediately after the puncture, the patient was seized with a hectic fever, without inflammatory symptoms, and shortly became exhausted and emaciated. Three weeks afterwards, the dropsy was considerably increased, and the respiration in the highest degree impeded by it. More than 20 pounds of a fetid fluid was discharged by the puncture. A number of cartilaginous tumours, of the size of a walnut, were now formed in the abdomen, which seemed to rise from the portion of the peritoneum which covered the enlarged ovarium. After a fortnight the patient died. I cannot determine whether the cysts in their commencement, before the formation of water took place, were capable of absorption or prevention from growth, by the use of our baths. It is possible that some of the tumours we are going to speak of, and which were either perfectly cured or greatly improved by the use of these waters, consisted of such small cysts.

The cases in which the water treatment was attended with beneficial results all belonged to the class of *solid tumours*. From the experiments in anatomic pathology, one may conclude that all swellings of the ovaria, the causes of which are *real hypertrophy* or *effusions of blood* in its tissues, or *fibrous tumours*, are capable of absorption, if treated at a period when they are not developed to too high a degree. All cases which were cured, or at least benefited by the use of the Creuznach waters, had the character of one or other of these kind of affections. But the objective and subjective symptoms of these tumours being often very similar, it was not always evident what kind of tumour existed. It was frequently the case that I

could not distinguish the real nature of the tumour, until during, or soon after the treatment. I need not state that we cannot expect a beneficial result from the course of the baths in cases of sarcomatous cysts with or without the formation of bones and hair in the tumour; and that even an injurious result follows in the case of carcinoma of the ovaria. In cases of cysts, which include bones and hair, the diagnosis cannot be clear before the surrounding parts are opened, and the deposited matter has formed an issue; and concerning carcinoma, the suffering of the whole constitution will, in most cases, enable us to decide on the real nature of the affection, even when the local symptoms are not sufficiently apparent to render the diagnosis certain. In general we may advise the use of our baths in cases of *solid tumours* of the ovaria, where no symptoms of *hydramnia* exist, and the constitution is not suffering more in consequence of the local disease than can be accounted for by the pressure of the tumour on the surrounding organs. I could adduce a considerable number of cases in which the result of the cure answered these expectations; the number of them in the last four years was between 10 and 12 annually; in former years between 6 and 8. The good effect of the baths will naturally be in proportion to the development of the disease. If the hypertrophy be very large, or the fibrous tumour of a very great size or hardness, we cannot expect that it will be entirely absorbed, but only diminished in size. A hard nucleus will remain, but without disturbance of the constitution or injury to the adjacent parts by pressure, or exhibiting any tendency to become inflamed. Some cases required repeated courses of the waters, in order to be reduced to such a degree, that no fresh increase was to be feared. As the diagnosis of these kind of swellings is not always clear, it is a question of great importance to know how long it will be advisable to continue the treatment. I should say that in cases in which neither during the use of the baths, nor in the following 3 or 4 months, a visible diminution of the tumour takes place, one may presume it is not capable of being absorbed, and therefore it would not be proper to continue the course of the waters. On the other hand, even the slightest improvement should encourage us to continue the cure until, from the tumours' diminution making no farther progress, we may infer that the treatment can no longer be beneficial. Respecting the time when absorption of the tumour begins, it may be mentioned here, that in cases of inveterate and fully developed disease, the improvement is not to be observed during the course of the baths, but by their after-effects; and such after-effect continues for a period of three or four months from the termination of the course of the baths; therefore we cannot judge of the full effect of the waters taken in the summer, until the close of the first part of the winter. With regard to the treatment, mother-lye was always added to the bath, more or less, in proportion to the constitution, strength, development of the disease, etc. For these affections, I do

not employ external means in order to assist the absorbing effects of the baths. The application of mother-lye to the wall of the abdomen corresponding with the seat of the tumour (as practised by others), cannot have a great effect, the skin having no communication with the ovaria, while the application is troublesome, and colds are scarcely to be avoided. Neither do I approve of the use of the douche in these cases, since I heard of patients treated in other watering-places, in which the douche, unseasonably applied, produced serious injury to the nerves of the abdomen. The use of ointments may be attended with beneficial results in cases of torpid tumours; but I believe the effect to be produced rather by the friction in rubbing in, than by any virtues of the ointment itself. Instead of other local applications, I made use of injections into the rectum, prepared of mother-lye mixed with water, in so small quantities that they could be retained and absorbed; but I cannot speak of the result, as I never used them without bathing. I need not mention that local means of other kinds may be required during the course, by accidental affections, *e.g.*, leeches, and aperients. When Professor Kilian at Bonn, in his essay on the "Colloid of the Ovaria," in which he much praises the effect of the Creuznach waters in affections of this kind, suggests the avoidance of exaggeration, every physician will be of the same opinion. As soon as the first symptoms of saturation appear, the treatment must cease. If the baths have not been too strong, these symptoms of saturation appear simultaneously, with an apparent irritation of the swelling; but this feeling, which is a sign of the beginning of absorption and improvement, we must be careful not to confuse with inflammatory irritation of the tumour, produced by too strong baths during the course, or other accidental causes, *e.g.*, cold.

Affections of the Uterus.

The affections of the uterus, which can be efficaciously treated by the Creuznach waters, are—*Chronic engorgements and indurations of a benignant character*, affecting the whole uterus, or some parts of it, and *hypertrophy* of this organ, produced by continual pressure arising from *fibrous tumours*. The number of patients sent to Creuznach affected with diseases of this kind is still greater than of those with diseases of the ovaria. Of recent years I have treated annually from 14 to 16 patients afflicted with such complaints. In former years the number was not quite so great; but taking all the cases I have treated during a period of sixteen years, we obtain a series of observations, which furnish, in my opinion, accurate data for determining certain indications for the use of the Creuznach waters.

In the cases of *benignant idiopathic engorgements and indurations* of the uterus, commonly the *os uteri* was the seat of the disease; the cases in which the cervix was affected, were rare. The hardness of the swelling was in some cases so great that the diagnosis was uncertain, and when mistaken for scirrhus, the error was excusable.

But regarding the treatment, it is of the highest importance to be sure of the diagnosis. For scirrhus will not only not be benefited by the waters, but will become worse and more disposed to be changed into carcinoma. I considered it a most significant symptom of its not being scirrhus when the orifice of the os uteri was open, and the swelling became softer at the time of menstruation. To patients of a torpid constitution, and when the indurated portion was not at all sensitive, besides drinking and bathing, I prescribed the use of the uterine douche, applied either in the bath by a weak douche-machine, or out of the bath by means of a *clysopompe* or *irrigateur*. When symptoms of congestion appeared, leeches were applied to the vagina.

Of patients considered as affected with chronic idiopathic *engorgements of the whole uterus*, only in one case was the diagnosis manifest. The uterus had acquired such a size that the fundus could be felt two inches above the pubis. The enlargement followed an attack of acute metritis after confinement; it was developed to the above size in a very short time, and remained unchanged for one year, until the time I saw the patient. Symptoms of fibrous tumours could not be detected. After two courses of the baths, taken in two summers, the uterus was diminished to half its size, and remained in that state afterwards. In two other cases the diagnosis was not quite clear. The uterus had become enlarged without inflammatory symptoms, and the enlargement was still increasing; the swelling had exceeded the size of a gravid uterus in the 7th month; both patients enjoyed good health, and only experienced the slight inconvenience of a common pregnancy; the menstruation was regular, and at such times the whole swelling became softer. By the use of the baths the growth of the uterus ceased. Only one of these patients took the waters for a second time. Two months after the course the tumours were so diminished in size—as I was told by the physician—that the fundus was to be felt one inch above the pelvis.

Among the cases of idiopathic indurations, I observed two in which only the *fundus uteri* was enlarged and indurated. Both were complicated with retroversion of the uterus; the organ was retroverted when gravid; abortion took place, and afterwards metritis, with partial peritonitis. The fundus uteri adhered to the rectum, and the reposition could not succeed. By the pressure, enlargement and induration of the fundus were produced. In one of these cases the swelling of the organ was reduced to such a degree by the use of the waters, that all the symptoms became tolerable; but the reposition was not possible. In the second case, which I treated last year, the retroversion had taken place eight years ago, and the lady was, several times, during that period, affected with metritis and peritonitis. The consequences of the pressure were very shocking; the rectum paralysed; the menses attended with the most violent pains, and the whole constitution exhausted by pain and restlessness. Only weak baths could be prescribed; neverthe-

less, some months after the course, the diminution of the tumour was remarkable, and a general improvement of all symptoms, and strengthening of the constitution, took place. This patient will continue a course of the waters during the ensuing summer.

The greatest number of the affections of the uterus, efficaciously treated by the use of the Creuznach waters, consisted of *hypertrophy* of the uterus, produced by *fibrous tumours*. They appeared in various parts of the uterus—some imbedded in the tissue of this organ, or situated closer to the external surface, more or less united with the tissue of the uterus, sometimes only slightly adhering to the external wall of this organ, or even, without any connexion with it, imbedded in the portion of the peritoneum, which covers the uterus. Cases of submucous fibroids, attached to the internal wall of the uterus, and imbedded in its mass, were less frequent. On the os uteri I have never seen fibrous tumours. In cases in which it was possible to examine the tumours by manipulation, they appeared either of a round uniform shape, or of an irregular knobbed form, in the cases wherein several tumours were combined. Their size varied from that of a bean to that of a child's head; in consistence, some were elastic, others of a firm cartilaginous structure. All were accompanied with a hypertrophied condition of the uterine parietes. The hypertrophy of the uterus itself varied very much in size. It appeared the least enlarged when the fibrous tumour was imbedded in the external walls of the fundus. In cases of interstitial tumours, the uterus was the most enlarged, sometimes even to the size of an advanced pregnancy. Most patients were married, and at a climacterical age; some had had children, others had been sterile; several, however, were women of about 30 years of age, and a few even young girls, who had only menstruated a few years. In most cases of external and interstitial tumours, the constitution did not suffer; the menses were regular, but generally a little too sparing, or amenorrhoea existed. A lady, who came here affected with a swelling of the uterus as large as a child's head, and with two irregular tumours at the fundus uteri, was two months ago regularly delivered of a child. Two other ladies, both married, and with families, had an enlarged uterus of the size of a gravid uterus at the full time, without injury to their constitution. In cases in which the constitution was suffering, and even exhausted by anæmia, produced by menorrhagia or copious menstruations, submucous fibroids, or such interstitial fibrous tumours as grow towards the uterine cavity, could be presumed. Patients of this kind suffered from violent uterine colic: In no case were these colic pains so dreadful as in a lady, who had a very small fibrous tumour in the tissue of the cervix uteri. In cases of external tumours, and such interstitial tumours which grow towards the external wall of the uterus, the diagnosis presented no difficulties except when complicated with the affections of the ovaria. The uterus hypertrophied could not be mistaken for a gravid one, even in the case of a lady with amenorrhoea, because the disease had

lasted some years. It is important to distinguish enlargements of this kind from idiopathic chronic engorgement, as such affections, from their tendency to congestion, do not admit of so strong baths as fibrous tumours. No less important is it to distinguish submucous fibroids and such interstitial tumours, which grow towards the internal walls of the uterus from polypus, when still inclosed in the uterine cavity. But there are cases in which the diagnosis cannot be accurately determined, even with the aid of the uterine sound applied by the most skilful hand. A polypus cannot be absorbed by any means, and when the baths are taken, it involves only a loss of time; and, moreover, we must expect that the baths will produce a bad effect on the constitution, when no object exists on which they may act. And actually some cases of polypus have been mistaken for mucous fibroids, and sent here. In one of these cases, after 20 baths had been taken, the polypus passed through the os uteri. I need not say that the baths were immediately suspended. In another case the same circumstances occurred, but not until after the patient had returned home, having gone through a whole course of the waters.

That fibrous tumours can be absorbed, when not entirely of a cartilaginous structure, is a fact. They are found so frequently in the body and fundus of the uterus, that every physician has ample opportunity for making his own observations. All the cases we met with here, even when the tumour was small, had already existed for years, and many remedies had been resorted to in vain. The extent of the cure here was in proportion to the size, hardness, and duration of the tumour. Tumours of a cartilaginous structure were never dissolved by the waters; only a diminution of the enlargement of the uterus took place. On some tumours of the size of a walnut, whose substance was not solid, the baths acted so powerfully that they could not be felt at the end of the course. In others the diminution was not perceptible until the completion of the course, or even some months afterwards. The absorption commenced sometimes in the fibroid, when it was not of a too solid structure, sometimes in the hypertrophied tissue of the uterus. I cannot account for the difference. In two cases of tumours of a cartilaginous structure, with hypertrophy of the uterus, the development of the disease was not even arrested at the end of the course; but some months afterwards it began to cease growing, and when a course of waters was resumed the following year, a slight diminution of the swelling of the uterus could be observed. In both patients the disease remained afterwards stationary (it is now 4 and 6 years since the second course was taken), and the patients enjoy good health. When the growth of the tumour ceased, diminution of the same did not immediately begin. When a perceptible diminution of a tumour was produced by the baths, and no increase had taken place after an interval of six months, the improvement was permanent. In some cases in which the increase of the swelling was only arrested without being diminished, diminution took place years afterwards without any

treatment, on the patient arriving at the climacterical period of life. All tumours of the fundus uteri, which could be felt by the touch, and which were diminished in size by the use of the waters, began towards the end of the course to swell, and became perceptibly softer. These symptoms could always be considered as indications of the commencement of absorption. In some patients, in whose cases a submucous fibroid was presumed, a discharge of a serous fluid, combined with many flakes, appeared during the course. Two of these cases, both young women, who had, on several occasions, miscarried, became pregnant shortly after their return home, and were regularly delivered. Another young lady, who was also presumed to have a submucous fibroid, was, after having taken 20 baths, suddenly affected with uterine colic, followed by a copious discharge of a thick sanaceous matter, mixed with many small and large flakes. On the following day the discharge became more serous, and continued in small quantities for some weeks. I heard no more of her case after she left Creuznach.

Also, in all affections of this kind the addition of mother-lye to the baths was in proportion to the development of the disease and the constitution of the patient. Further, the baths could, *ceteris paribus*, be stronger in these than in other affections, and it was surprising how strong the baths could be taken by such patients, even when of a delicate constitution. There were some cases in which it was necessary gradually to strengthen the baths to such a degree, that the bath at last contained, in 400 pints of plain mineral water (the usual quantity in a bath), 23 pounds of muriate of lime, and more than 11 ounces of bromide of sodium; or, in other words, 60 pints of mother-lye in each bath. When the constitution is weakened by accidental complaints, great caution must be exercised in the addition of mother-lye to the bath, and we must endeavour to produce the wished-for result, rather by a prolonged course of the baths, than by giving too much strength to them. But exhaustion, produced by anæmia, in cases of submucous fibroids, does not necessitate weaker baths, any more than other tumours which are not attended with floodings. The hæmorrhage will only be promoted when the additions of mother-lye are injudiciously large, or the temperature of the baths too high. Anæmic patients like hot baths, and cannot therefore be too earnestly cautioned to be careful in this respect. If the temperature be higher than 92 degrees of Fahrenheit, we may expect either hæmorrhage while the patient is in the bath, or the menses will come on sooner and more copiously than at any time. The same caution is necessary with regard to hip-baths. Respecting the use of the douche and other local means, and concerning the principles, determining the duration of the course, and the addition of mother-lye to the bath, I must refer the reader to the remarks I have already made on these points, when speaking of the treatment of the affections of the ovaria.

ARTICLE III.—Diseases of the Spinal Cord and its Membranes. By EVANS REEVES, M.D., Edwards' Street, Portman Square, London.

I.—SPINAL MENINGITIS AND ITS COMPLICATIONS.

Pathology of Acute Spinal Meningitis.—The cases observed and collected for the purpose of forming a history of acute spinal meningitis and its complications amount to 39. The arachnoid was affected alone in 7 of the cases; the arachnoid and pia mater, or pia mater alone, in 11; the membranes and the cord in 7; and the membranes of the cord and those of the brain, with or without the substance of the brain or cord being implicated, in 13.

The lesions observed after death in inflammation of the arachnoid, are—

1st, Congestion with effusion of sero-sanguineolent serum. The more intense the congestion, the greater will be the quantity of serum, and the more sanguineous its hue.

2d, Congestion with opacity-thickening-false membranes — pus more or less concrete or sero-purulent fluid.

Where the pia mater is affected,

1st, Congestion, varying in intensity from a pale rose-red to a deep purple, with this, if it has been of some duration and intense, the cellular tissue between it and the arachnoid will contain serum, more or less sanguineolent, gelatinous deposit (most frequent) or pus (rare).

With these changes the membrane itself may be, or not, more or less opaque and adherent to the surface of the cord, from which it cannot be separated without bringing away portions of it.

When the cord is implicated it may present,

1st, Increased vascularity, with or without infiltration of serum.

2d, Increased consistence.

3d, Softening—varying in consistence, from cream-cheese to soft pap; and in hue, from venous red to yellow, or a cream-white.

In implication of the membranes of the brain, either of the following states exist :—

1st, Congestion of the vessels and membranes, with sero-sanguineolent effusion into the ventricles, or at its base or convexity.

2d, Thickening and opacity of the membranes, with gelatinous or purulent exudation, or the formation of false membranes.

The brain with the above changes may be congested, its substance increased in density, or more or less softened.

In inflammation, both of the arachnoid and of the pia mater of the cord, the cellular tissue between the dura mater and the walls of the spine, is frequently found infiltrated with serum, sometimes blood, and the vessels ramifying in it highly congested.

Pathology of Chronic Spinal Meningitis.—Few opportunities occur of observing the changes, which chronic inflammation induces from its rarely proving fatal, unless disease of the cord (particularly the cervical portion), the brain or its membranes, lungs, kidneys, or sloughing of the sacrum occurs.

In the arachnoid the changes observed are opacity of a greater or lesser extent, adhesions between the visceral fold and the one lining the dura mater, either direct or by cellular bands.

"Cellular bands," observes Dr Copland (*Dict. of Pract. Med., art. Spinal Cord*), "have, after they have existed sometime, a tendency, from the deposit of oil globules, to be converted into fatty tissue, a result marked by an improvement in the power of motion of the paralytic limbs." Sometimes, however, fresh accessions are made to the cellular deposits, particularly when from some cause, such as chronic disease of the vertebra or kidneys, a state of congestion is kept up or excited. When this is the case they may acquire such a magnitude as to press on the cord and induce either atrophy or softening; sometimes, however, particularly in the case of disease of the vertebræ or chronic inflammation of their ligaments, the arachnoid, although it has undergone considerable change, yet the atrophy or softening may arise from the deposit of inflammatory products in the cellular tissue, external to the dura mater, upon or under the ligaments. A state of paralysis, both of motion and sensation, more or less pronounced, of one or both of the lower extremities, may exist, and yet the nerves forming the cauda equina may be only adherent from the interposition of cellular bands.

Sometimes cartilaginous or bony deposits are formed. They may, however, arise from the general tendency which serous membranes have to ossify, and they are nearly invariably accompanied by a similar state of the arteries, and similar deposits in the serous membranes of other parts of the body. These deposits are, particularly when cartilaginous, small, varying in size from a millet seed to a silver penny piece, their number also varies.

In a case recorded by Dr Bright, 50 or 60 existed, and Velpeau found a large number floating on the surface of the arachnoid,—resembling minute pieces of white soap. In this case a tumour of an encephaloid character existed on the anterior part of the cord. Esquirol,¹ in the case of a female, who became, when 35 years of age, epileptic, and died 5 years later, asphyxiated during an access, found the whole of the external aspect of the arachnoid covered with bony plates, one or two lines in diameter. They have been occasionally observed in tetanus. Brayne, Swan, and Horner have each recorded a case. These are nearly the only instances, out of 300 recorded cases of this disease which I have examined. They cannot, therefore, be considered either as a result or a cause of tetanus.

¹ Sur l'Épilepsie, Bul. de la Facult. Tome v.

Olivier considered that they were common in those who had suffered from pains considered as rheumatismal. I have several times had an opportunity of observing them, particularly after the 40th year, and co-existing with deposits in the arteries. In one case, that of a bed-ridden man of 73, the deposit was bony and of the size of a shilling, and situated opposite the 12th dorsal vertebra, it was accompanied by a similar state of the pericardium, dura mater of the skull, and arteries generally.

Barbier (*Traité Element de Mat. Med., Tome 3, p. 479*) found an osseous plate, 3 lines broad and 2 inches long. Softening of the cord existed in this case.

Morgagni (*De Sed. et Caus. Morb. Epis. 25, Art. 9*), found in an old man the cartilaginous plates accompanied by ossification of the pleura.

Soëmmering, in his translation of Baillie's *Morbid Anatomy*, published at Berlin in 1794, at page 248, note 524, states that he has several times found cartilaginous bodies in the arachnoid of the cord.

Lobstein (*Compte Rendu, à la Fac. de Med. de Strasbourg, 1820-50*), found the arachnoid containing cartilaginous plates. Sabathier, he remarks, had also observed them.

Chaussard (*Sur l'Organisation des Vieillards, Paris, 1822*), observed them as ivory-like plates, nearly quadrilateral in form.

Dr Fletcher (*Prov. Med. Jour., vol. 14*) found some ossific plates in a man who was paraplegic. The left corpus striatum contained an old clot of blood.

Rokitansky also observed them accompanied by opacity of the arachnoid. In this case softening of the cord existed.

Changes in the pia mater are still less frequently observed, particularly without chronic disease of the cord. In these cases it may be found opaque, or with false membranes more or less organized on its surface. In a man who had suffered from spasmodic contractions of the muscles of the neck, which drew the head towards the right shoulder, the pia mater opposite the third and fourth cervical vertebra, seemed contracted and deeply injected, but the cord appeared quite healthy.¹

Termination and Duration.—Of the 8 cases where the arachnoid was alone inflamed, the termination was successful in 7 of the number.

The case which proved fatal was that of a child a few days old, affected with spina bifida, which had ulcerated, first serous and then purulent fluid escaping from the spinal cavity.

The duration of the successful cases seems to have depended greatly on the activity of the treatment adopted and the period when it was had recourse to, as well as on the severity of the symptoms.

¹ Polleti, *Annali Univ. di Med.* xxxvi, 112.

The following table will show, as nearly as possible, the influence of different plans of treatment on the duration of the disease.

Sex.	Age.	Treatment.	Duration.
F.	18	Repeated abstraction of blood,	About 20 days.
F.	15	{ Local bleeding for the first 16 days, then the exhibition of mercury to render the mouth sore,	35 days.
F.	21	{ Leeches and purgatives; the chief reliance was placed on the last, (Two years before a similar attack had yielded to five bleedings in 15 days.)	46 days.
M.	42	Bled to 1 quart, then free application of leeches,	A few days.
M.	80	Leeches and digitalis,	{ Not a great No. of days.
M.	28	Bleeding and digitalis,	13 days.
M.	24	{ A large bleeding to 30 oz., calomel and tar- tarized antimony,	{ 12 days.

In the 11 cases where the arachnoid and pia mater were inflamed (or the pia mater alone), the termination was invariably fatal. In one, a male, æt. 29, the symptoms were developed by epilepsy, to which he had been for some time subject, and which had appeared soon after his marriage; death ensued on the second or third day. In another, a male, æt. 20, it arose from tapping a cyst which communicated with the spinal canal; death ensued on the 3d day; while in a 3d case, a male, æt. 50, it arose from the effusion of blood into the spinal canal, death took place on the 12th day, in the midst of clonic convulsions. In the other 7 cases death ensued in 1, a male, æt. 29, on the 7th day; in 2 on the 9th day, in 1, a male, æt. 29, from coma, in the other, a male, æt. 28, from convulsions; in 2 on the 10th day, in 1, a male, æt. 24, from coma, in the other, a female, æt. 22, while in a warm bath; in 1, a male, æt. 19, on the 11th day; in another, a male, æt. 35, on the 20th day, from inflammation of the membranes of the base of the brain; and in a child 3 days old in 24 hours, from exhaustion after convulsions. In this case it is probable that the inflammation had existed prior to the occurrence of the convulsions, for a longer period than 24 hours seems necessary for lymph to form.

The termination was fatal in the 7 cases where the membranes and the cord were affected.

In one, æt. 19, it ensued on the 8th day after convulsions; in a 2d, æt. 18, on the 14th day, exhausted and depressed; he was sensible until 6 hours before death; in a 3d, æt. 35, retention of urine came on, 11 days after pain in the back with fever and lassitude had been experienced, and death took place on the 9th day from general paralysis; in a 4th, æt. 56, on the 21st day, from sloughing

of the sacrum; in a 5th, æt. 35, on the 23d day, from exhaustion consequent on diarrhœa and sloughing of the sacrum; in a 6th, æt. 46, on the 26th day, from diarrhœa and general paralysis; in the 7th the patient was a child, and death ensued from hydrocephalus; its duration is not stated.

The termination was fatal in the 13 cases of inflammation of the membranes of the cord and of the brain, with or without implication of the substance of the brain or cord.

The following table will show the ages of the patients, and the duration of the disease.

MALES.

Age.	Duration of the Disease.	
Adult, fatal on 4th day.		
61, " "	It occurred on the 11th day of peripneumonia, which was progressing to a cure.	
21, " 9th day.		
24, " 10th "		
3½, " 9th "	after admission to hospital.	
50, " 12th "		
5, " 16th "		
13, Sudden hemiplegia on 11th of March, death on 14th of April. Tubercles in brain.		

FEMALES.

24, Fatal on 8th day.	
26, " 9th "	
40, " 9th "	
14, Extended over a period of some weeks.	
12, " " "	

Sex and Age.—Of the 39 cases of acute inflammation and its complications 28 were males and 8 females; the remaining 3 were children, their sex was not named.

In 7 of the 8 cases where the arachnoid was inflamed, 4 of the patients were males and 3 females. The ages of the first were 24, 28, 42, and 80; of the last, 15, 18, and 21.

In 10 of the 11 cases of inflammation of the arachnoid with implication of the pia mater, or inflammation of the pia mater alone, 9 were males and 1 female. The ages of the first were, 19, 20, 24, 24, 28, 29, 29, 35, and 50; the age of the female was 22.

In 6 of the 7 cases where the membranes and the cord were both inflamed, the patients were males, and their ages were, 18, 19, 35, 35, 46, and 56.

In the 13 cases where the membranes of the brain and cord were inflamed, with or without implication of the substance of the brain or cord, 8 of the patients were males and 5 females. The ages of the first were 3½, 13, 15, 21, 24, an adult, 50, and 60; while the ages of the last were 12, 14, 14, 26, and 40.

Causes.—1. *Of Spinal Meningitis.*—Exposure to cold and wet, particularly if accompanied by fatigue, disease of the bones of the spine, or injury to the spine, seem to be the most frequent exciting

causes. In one case it was induced by blood effused into the canal consequent on a fall; in another, from the formation of an ulcer on the surface of the tumour present in spina bifida; in a 3d, from the extension of inflammation excited by tapping a large cyst which communicated with the spinal canal; in a 4th, the patient had suffered for five years from an oedematous affection of the scalp, when headache set in, which yielded to treatment, but was followed by inflammation of the eyelids, which by the use of leeches had nearly subsided, when severe pain in the back declared itself. The sudden suppression of the menstrual discharge may act occasionally either as an exciting or predisposing cause. Inflammation of the lungs, kidneys, bladder, or intestines, may act in the same manner, or by the extension (particularly in diseases of the two first organs) of the inflammation along the walls of the vessels, or by the conveyance of inflammatory products to the membranes. In some of the cases, particularly those in which the pia mater was inflamed, symptoms of a rheumatic nature, more or less intense, and of variable duration, preceded the development of the symptoms.

2. *Cerebro-Spinal Meningitis*.—Nearly the same causes which induce spinal meningitis induce inflammation of the membranes of both situations. In one case it was induced by fright; her sister had been seized with the disease a few days before. In a 2d, it set in just as a favus of long standing was nearly cured; in a 3d it was induced by a long march, made immediately after being discharged from the hospital, where he had been under treatment for intermittent fever; in a 4th, after torticollis had existed for some days, it declared itself after a long walk on a very hot day; in a 5th, disease of the spine had existed some months; in a 6th, to sleeping in the sun when tipsy; in a 7th, to sleeping on the ground in a cold winter's night; in an 8th, it supervened on hemiplegia, induced by tubercles in the brain.

The disease under this form sometimes rages epidemically, often at the same time as typhus. Boyle states that the English troops sent to Sicily suffered severely from it; and during the wars of the Directory and Empire, troops from the north, when in garrison in the towns of the south, were very liable to it.

General Symptoms of Spinal Meningitis.

1. *Of the Arachnoid*.—The close resemblance which the symptoms, indicating the existence of inflammation of the arachnoid, bear to those present in congestion of the cord, render it sometimes difficult to distinguish one from the other; not that it is of any practical importance, for the treatment to be adopted in both is essentially the same. In congestion, fever is rarely present, and the pain is of a dull weighty character, and although paralysis is more or less complete, it is seldom attended, unless it is about to pass into inflammation, by shocks of pain or convulsive jerks. In inflammation of the arachnoid, the fever is generally severe, and the pain of a burn-

ing character. In some cases, however, where the lower part of the canal is the seat of the inflammation, it may be very slight, and continue so if the inflammation does not extend. With the pain a feeling of numbness, with pain and paralysis of the muscles is experienced in the lower part of the extremities, which gradually creep upwards to the body, which at length becomes similarly affected. Sometimes the paralysis is more marked in one extremity than the other, but this difference is seldom of long continuance unless the result of the inflammation is the formation of false membranes, which press on the nervous cords of one side more than the other.

When the paralysis extends to the lower part of the body, retention of urine ensues, and with it, from paralysis of the rectum and sphincter ani, either obstinate constipation or involuntary escape of the contents of the bowels. The last state generally succeeds the first when purgatives have been given. As the inflammation extends upwards or fluid accumulates, the walls of the chest and the upper extremities become paralysed. When the first takes place, a feeling of intense oppression will be experienced, sometimes general, at others more marked at one point than another, and inducing a sensation as if a cord or band was bound tightly round it. When the paralysis of the walls of the chest is complete, respiration will be carried on solely by the diaphragm, assisted slightly by the muscles of the neck, and it will be slow, sometimes sighing and interrupted, particularly on motion, by accesses of suffocation.

The upper extremities rarely become so severely or extensively paralysed as the lower. When the inflammation affects the whole extent of the arachnoid at the same time, the pain will exist along the whole length of the spine, and the paralysis will be general, and rapidly developed. But when the inflammation is confined to one point, as for instance to some part of the dorsal region, and if the result is the formation of false membranes, or serum or pus bound in by adhesions, and not in sufficient amount to press on the cord or nerves, and interfere with the reception or transmission of impressions, paralysis will then be absent, for it is only as a mechanical cause that the effused matters in inflammation of the arachnoid produce paralysis. It is therefore common to find, in inflammation of the upper parts of the canal, from the effused matters poured out gravitating to the lower part, paralysis of the lower extremities and of the bladder and rectum.

The paralysis of motion is nearly invariably accompanied by that of sensation. In two of the cases only was it increased, and this without the cord being apparently affected. Whether this state of increased sensibility of the cutaneous surface is due to irritation of the nerves of sensation or not it is difficult to say. It is not, however, improbable but that it may be.

The parts supplied with nerves from that part of the cord where the membrane is inflamed, are sometimes the seat of transient shocks of pain or slight convulsions, and sometimes of tremulous

motions. The shocks of pain and the convulsions occur under two circumstances; in one, from motion of the parts, in this case they generally pass upwards; in the other, from impressions (such as sudden mental emotion or noise) received through the brain, in this case they pass from the centre to the periphery.

These shocks are often very troublesome, and occur just as the patient is about to fall asleep, or if when asleep, he begins to dream, in both instances awakening him.

CASE I.—A delicate female, æt. 15, not accustomed to exertion, came to London in 1851 to see the exhibition. During the first two days she walked a great deal, and on her way home to her lodgings, on the evening of the 21 day, very much fatigued, she got wet. She passed a very restless night, and felt as if her whole body was bound tightly up in a bandage, but towards morning she fell asleep, and awoke nearly free from it. But she complained of a feeling of general lassitude, and drew her mother's attention to her feet, which felt benumbed and somewhat painful. She remained in bed until the evening, when she got up for the bed to be made. Then she complained that her legs, as high as the knees, were benumbed, and she was scarcely able to stand. She passed a somewhat restless night, was feverish, and drank a great deal. In the morning, the legs, as high as the thighs, were benumbed and painful, the lower part of the back was also the seat of burning pain. She was now seen by a medical man, who gave her some medicine which opened the bowels, which were confined, and inducing diarrhoea and inability to control the escape of their contents. The urine began to dribble away from her. At length, on the 4th day, a catheter was passed, and a large quantity of urine drawn off, and from this time it was introduced daily. She was now seen by a physician. She was cupped, and leeches were applied several times. In the course of the next 12 days she regained the power of controlling the evacuation of the contents of the bladder and rectum, and some slight power over the lower extremities. She was now brought into the country, where I had an opportunity of observing the case.

The power of motion in the extremities was still limited, they could be slightly retracted and moved on the bed, but not raised. Sensation was obtuse, and this existed as high as the trochanters. Pain existed accompanied by numbness, both were increased by motion, and lasted some time; the pulse was quick, but soft and compressible, skin rather warm, tongue red, thirst, particularly towards night severe, urine high coloured, pain in the lower part of the back, but of no great severity, and unaccompanied by tenderness, but slightly increased by bending the back.

The cupping and the application of leeches were repeated several times, and small doses of calomel with Dover's powder given, with the effect of rendering her mouth slightly sore. From the time that this effect was produced, her recovery was rapid, and on the 35th day from the commencement of the attack she was able to walk about as usual.

CASE II.—A robust carter, æt. 24, on the 24th of April 1850, when wet and fatigued, threw himself on some straw in his stable, and went to sleep for several hours. On awaking he felt very cold, and made great efforts to shake it off by exerting himself, but to no purpose. He passed the night suffering severely from pains in his back and limbs, and in the morning he was unable to go to work. Some epsom salts were taken, but without effect. He was seen on the evening of the 25th. His face anxious, flushed, and covered with perspiration, as was also the rest of his body. Pulse full and strong, skin hot, thirst severe, tongue white in the centre, but red at its edges and apex. The lower extremities were motionless and insensible, and the seat of severe numb-

like pain, increased by moving or bending them. The trunk was insensible as high as the nipples. Breathing greatly oppressed, and the arms felt benumbed, although the sensation was nearly intact. The bladder was distended, and no urine had been passed for 24 hours, bowels confined. The whole spine was the seat of pain, but it was free from rigidity or tenderness on pressure, although the muscles on both sides of the spinous processes were painfully tender. If he attempted to move his lower extremities the pain was increased and painful shocks were excited, which shot up to the head. Raising him to the sitting position, caused these shocks to shoot along the sides of the chest and down the legs. The moment he fell asleep he was awake by them, their severity causing him to utter loud groans. They seemed to arise suddenly in the spine, and shoot from thence over the body. His easiest position was with the head and shoulders somewhat raised, and avoiding the slightest movement; even speaking excited pain.

Blood to the amount of 30 ounces was taken from the arm, and with marked relief to the symptoms, and 2 grains of calomel and a $\frac{1}{4}$ of a grain of tartarised antimony given every two hours. The urine was drawn off.

26th. He had obtained some sleep after the bleeding. The first dose of the antimony had excited considerable nausea. The painful shocks had ceased to occur with the same severity, except on motion. They seemed to have passed into a state of tremor, which, like the shocks, was transient. The pulse being good, the bandage was removed; and the adhesions at the opening of the vein destroyed, and blood to the amount of 10 or 12 ounces allowed to flow. From this time the improvement became manifest, the bowels acted without medicine, a large quantity of green fæces being passed. The mouth became sore on the fourth day of the exhibition of the calomel; it was then discontinued. On the twelfth day he was able to get up.

The symptoms subsided in the following manner:—first the shocks of pain, then the oppression of the chest, and the numbness of the arms, accompanied by a return of the sensation of the body as low as the pelvis, then the power of evacuating the contents of the bladder, and lastly, of the voluntary power and sensation of the legs.

2. *Of the Pia Mater.*—This membrane is rarely affected with inflammation, without the arachnoid being at the same time implicated.

The most characteristic symptoms of inflammatory congestion or inflammation of the pia mater, are, rigidity of the muscles supplied by nerves derived from that part of the cord where the membrane is affected, followed by contraction, which is generally aggravated by shocks. The voluntary power and sensation of the parts affected with rigidity are but little affected in those cases where the substance of the cord or the nerves remain unimplicated. The first will, however, be interfered with from the rigidity of the muscles, and when severe it will be altogether impossible. The patient will, even when the rigidity is not severe, guard as much as possible against exerting the power of the will from the great increase of pain which motion excites, and the tendency which it has to induce convulsive shocks, or aggravate the state of contraction. But after the disease has been established and fluid poured out from the pressure which it excites, both will be more or less destroyed. In inflammation of the upper part of the canal, particularly when the arachnoid is also affected, the gravitation of the fluid effused may induce paralysis of motion and of sensation of the lower extremities,

the muscles of the upper extremities, the neck, and back, being rigid, and more or less contracted. This state of paralysis may also be induced by the matters effused into the cellular tissue covering the pia mater, from the pressure which they exert on the cord.

The cutaneous sensibility is sometimes increased, either from the substance of the cord becoming implicated, or from irritation of the nerves of sensation. The transient shocks or convulsions sometimes present in arachnitis, are very severe in inflammation of this membrane; its intimate connection with the cord and nerves may account for this, and for the state of increased cutaneous sensibility sometimes observed. Attempts at motion will induce these transient shocks and convulsions, and when contraction exists they will increase it. Tickling the soles of the feet or the palms of the hands when the corresponding extremities are affected, will induce them. They will also occur spontaneously, and add greatly to the severe pain already existing. They may sometimes become so severe as to assume a clonic form, the parts affected being alone convulsed, or from irritation or inflammation of the cerebellum ensuing, all the muscles of voluntary motion may participate.

In tracing the influence which the implication of certain parts of the pia mater exerts on the muscles, which receive their nerves from that part of the cord where the membrane is affected, it will be found,

1st, That when the membrane of the upper part of the cervical region is implicated the muscles of the face and jaws are affected, first with stiffness, which rapidly passes into a state of complete rigidity, rendering the jaws immoveable, and the features distorted. Sometimes the muscles are convulsed, and pain, more or less severe, exists, which is greatly aggravated by any attempt to move the jaws. The head will be also, more or less drawn back, from the muscles of the upper part of the back of the neck being stiff and contracted, and the eyes will be, from their muscles sometimes being similarly affected, either fixed or distorted.

2d, When the inflammation extends to, or is seated lower down in the cervical region, then the muscles of the neck will present first a state of stiffness, then rigidity followed by contraction, by which the head will be drawn back or to one side, according as to whether one side is alone or more affected than the other, and both deglutition and respiration will be more or less interfered with.

3d, Implication of the membrane of the brachial portion of the cord will be attended by rigidity and contraction of the muscles of the arms, and of the dorsal and lumbar portions, by the same state of the muscles of the chest, abdomen, back, and lower extremities. This state of rigidity and contraction will, as has been before observed, be greatly aggravated by the convulsive pains or jerks which occur.

CASE III.—A farm labourer, æt. 29, of moderate stature and strength, after

exposure to wet while mowing, was taken in the evening with pain in the back. The next, and succeeding days, the pain was very severe, and prevented him from going to work; he took some turpentine and his back was rubbed with it, with some slight relief. On the morning of the 3d day he came under my notice as a parochial patient, suffering from what appeared to be severe lumbago. He had slight fever and thirst, bowels confined, urine scanty and high coloured. A mustard poultice was ordered to be applied to the back, and some saline purgative medicine with colchicum, with a dose of calomel and opium at bed time, given.

4th.—He had passed a very restless night, the pain in the back had increased. It was referred to the vicinity of the lower dorsal vertebrae, and was aggravated by bending the back and striking the spinous processes with the knuckles. This led me to question him as to whether he had received any injury to the back. All that could be learnt was, that in a quarrel with one of his fellow-labourers he had received a blow, the day before he complained, which had knocked him down, and he had rolled off a hay stack; but the height from which he had fallen was not great, and he had received no injury. There was an expression of intense anxiety about his face, which made me feel very anxious for the result of the case. He could walk, but said that it was difficult, from the pain in the back; and when he was in the erect position he kept his hands firmly pressed on his back. His bowels had not been opened, and no urine had been passed since the day before, but the bladder was not distended. As the state of the pulse, which was quick, but without much power, did not seem to admit of the abstraction of blood from the arm, he was cupped to 8 or 10 oz. from the seat of pain, with relief. A dose of croton oil was given, followed by some saline sedative medicine. The croton oil opened his bowels, and at the same time he passed about a pint of urine. In the evening he seemed somewhat better.

5th.—The night had been unusually restless. The pain had greatly extended, the lower half of the chest felt as if tightly surrounded by a band of iron, which rendered the breathing oppressed. The muscles of the back felt stiff, as did those of the abdomen; the lower extremities were somewhat rigid. The parts thus affected were the seat of severe pain, which was from time to time aggravated, particularly on attempting to move, by shocks or jerks. This pain was also greatly increased by motion. The power of moving the legs seemed much restricted; the cutaneous sensibility was intact, although deep pressure was painful. Heart's action tumultuous, pulse rapid and jerking. Blood to 15 oz. was taken from the arm, with temporary relief to the symptoms, and it was repeated to 10 oz. in the evening with a like result; calomel and opium were also given.

6th.—This morning the symptoms were much aggravated, the whole spine was rigid and curved, the legs were retracted, but they could be extended although it excited severe pain; the upper extremities were stiff, as was also the neck; breathing greatly oppressed, and great difficulty was experienced in swallowing; the pulse was very rapid; heart's action very tumultuous. The blood drawn presented but a very slight buffy coat. This morning a motion was passed involuntarily, but no urine had been passed since the action of the croton oil. The bladder did not feel greatly distended; a catheter was, however, passed, and about a pint of urine drawn off.

From time to time the back, neck, and extremities were affected with contractions lasting for three or four minutes, causing him to utter loud groans, from the severe aggravation of the pain which they induced, and the perspiration to ooze out in large drops on his face and body.

The tobacco enema was tried, but although not more than 15 grs. were used in the infusion, the symptoms which it induced were most alarming, the pulse became almost imperceptible, the heart's action feeble and tumultuous, and the surface of the body pallid. This state continued for twenty minutes after

the enema had been evacuated by pressing on the lower part of the colon. It did not seem to exert any influence on the disease further than to prevent the occurrence of the convulsive shocks. The same results followed the use of chloroform on the next day.

7th.—The muscles of the face and jaws were now affected, the utterance of words difficult, although they were quite appropriate.

8th.—Breathing was still more oppressed, and rather noisy. Towards the evening a state of coma set in, and he died early in the morning of the 9th day.

The body was examined 24 hours after death. The vessels of the brain and the substance of the brain were loaded with blood, the lateral ventricles contained a small quantity of serum, as did also the pia mater at the base of the brain.

The vessels of the spinal canal were loaded with blood, the cellular tissue between the dura mater and the walls of the spine was of a deep red. A large quantity of serum escaped from the canal, it was slightly turbid. In the lower part of the dorsal region the arachnoid membrane was somewhat thickened and opaque; between it and the pia mater gelatinous exudation existed, which extended up as high as the medulla oblongata, but its consistence was much less marked above the 6th dorsal vertebra than below. The pia mater was deeply injected throughout, but this state was more marked below than above the 6th dorsal vertebra.

The cord was more vascular than usual, but no other change could be observed, although it was carefully examined. The vertebræ were quite healthy.

(To be continued.)

ARTICLE IV.—*On the Peculiarities of Dentition in Man, and its Influence on Infantile Mortality.* By J. SMITH, M.D., Dentist, Edinburgh.

THIS is a subject in which, notwithstanding the researches of previous enquirers, much room still remains for investigation, and regarding which, both as to its nature and consequences, the most vague and contradictory ideas are entertained—some of them, like most other errors once established in medicine, being apparently as difficult to remove, as they are certain to exert an injurious tendency in their practical application.

Dentition, in the most common acceptance of the word, appears to be too frequently confined to that period at which the temporary teeth make their appearance above the gum; and in this restricted sense of the term, we have constant reference made to a number of affections occurring during infancy, and attributed to the morbid influence of, exclusively, the eruptive stage of this process, under the various names of teething, cutting the teeth, etc. etc., all having regard to the penetration of the tissue enclosing the tooth—as if this were effected, not according to a vital process, but by mechanical force—while the other stages, exhibiting less obvious phenomena, are never taken into account at all. It is of importance, in the consideration of this subject, to bear in mind that the process of dentition, being in all its stages a natural one, regulated by relatively the same physiological laws as other healthy actions, ought to proceed without constitutional disturbance; that it does so in all animals, so far as

we know, with the exception of man; and that, in his case, when disturbance does take place, it is to be regarded as arising from some peculiar abnormal condition of the parts concerned in, and not all as an inherent effect of the process itself.

The structures concerned in dentition are originally adapted for its occurrence, and it is inconsistent with what we know to be the case in analogous instances of development, that any of the steps essentially necessary in the process, such as the penetration of the gum, should occasion a great amount of distress. But an important source of error, and one calculated to lead to much misapprehension on this subject, exists in the fact, that from the greater susceptibility at this age to impressions of any kind, sometimes those of a very slight nature, considerable constitutional disturbance, altogether apart from this cause, is of very common occurrence; and, although certainly not in every case due to this process, yet the eruptive stage of dentition offering something like a plausible explanation of suffering, an unfavourable impression generally prevails regarding it, and affections are imputed to its agency, which might, in many cases, be traced to an entirely different origin. However, attention being arrested by what appears to be so evident a cause of uneasiness, all treatment is at once turned in that direction, and the true cause of the disorder probably overlooked.

The opinions entertained of dentition being the source of such serious results as are generally ascribed to it, appear in a great measure owing to this unfavourable prejudice existing in the minds of medical men; and while the works of the more early authors on this subject, contain statements involving such misconception and inaccuracy, as to render their confutation unnecessary, we find, at the present day, that the same inclination towards exaggerated ideas of the pernicious tendency of this process, prevails to a very considerable degree.

As an illustration of what we here state in reference to this question, we find in the "Gazette des Hôpitaux," for March 3, 1855, the following remarks: "One sixth of all infants born perish annually at the period of dentition. Already, in 1781, the Royal Society of Medicine, struck by this alarming proportion, offered a prize for the best essay on the following subject, 'What are the best means to be employed for the prevention of the accidents of dentition, common to infants at the breast, and for their successful treatment when they occur?' The question, hitherto ill-studied, has not as yet been solved, for, as the mortality tables substantiate, *the proportion of infants lost by dentition, is exactly the same at the present day as it was in 1781.*"

Not to mention the fact of the rate of infantile mortality having diminished in every way since 1781, it need only be remarked with regard to the assertion that "one-sixth of infants born perish annually at the period of dentition," that, according to the latest statistics, the total number of deaths from all causes whatever,

occurring between the third and twenty-fourth months, allowing this period to embrace that of more active dentition, does not amount annually, to one-seventh of the number born. Thus, in the last volume of the Registrar-General's Reports, the deaths in England for 1849, between the ages of 3 and 24 months inclusive, amounted to 40,847, while the number born was 295,158, making the number of deaths less than one-seventh. Dentition, taking place as it does, generally speaking, about the same time in all children, we should find, were its concomitant disorders of so fatal a character, that the infantile mortality at the period of its occurrence, if not relatively increased, would, at all events, not be materially diminished. Instead of that, however, we find, with this cause of disease superadded to those previously existing, that about this very time the mortality commences, and continues afterwards, to be to a considerable extent smaller than before. From the 4th to the 14th month, allowing for extreme cases, has been stated as the period during which the teeth commence to make their appearance. According to Fox, the average date of their eruption is the 6th, 7th, or 8th month of infant life; Hunter, the 7th, 8th, or 9th; Blake, the 6th, 7th, or 8th; and Bell the 5th, 6th, 7th, or 8th. Allowing, then, the eruption of the temporary teeth to commence during the 6th, 7th, or 8th months, we shall find, by calculating according to the data afforded by different tables of mortality for the first periods of infant life, given in "M. Quetelet's Treatise on Man," that the following ratios are obtained:—

Of 100,000 born, there remain alive at the end of the

1st month,	90,396,	or 9604 die during 1st month.			
2d	"	87,936,	"	2460	"
3d	"	86,175,	"	1761	"
4th	"	84,720,	"	1455	"
5th	"	83,571,	"	1149	"
6th	"	82,526,	"	1045	"
12th	"	77,528,	"	4998	" before 12th "

Thus, of 83,571 alive at the 5th month, 1045, or nearly $\frac{1}{80}$, die during the sixth month; while of 82,526 alive at the sixth month, there die before one year 4998, or only $\frac{5}{80}$ in the next six months.

The following ratio is given, according to the data afforded by another table of M. Quetelet:—

0 to 1 month	20,542 deaths.
1 ... 2	5,233
2 ... 3	3,748
3 ... 4	3,090
4 ... 5	2,467
5 ... 6	2,213
6 ... 8	3,525

The deaths here, during the 7th and 8th months, being 1155 less than during the 5th and 6th.

In the Registrar General's Returns for England, we find abstracts

occasionally given of the comparative mortality at different ages in different years. Taking that for the years 1839-44 inclusive, p. 214 of vol. for 1847-48, as an average example, the annexed table will be found to give the ratio as exhibited there:—

Deaths.			
Years.	Months.		
	3d to 6th.	6th to 9th.	9th to 12th.
1839	8165	6800	6172
1840	8328	7081	6320
1841	8008	6341	5573
1842	8279	6814	6141
1843	8259	6786	6123
1844	8400	6831	5927

The steady decrease in mortality exhibited by this table confirms what has been already advanced.

In the last published volume, namely for 1849, we likewise find the following table, as exhibiting the rate of mortality at different ages:—

	Total Births.	Total Deaths.	Months.			Total under one year.	One year.
			0	3	6		
England,	295,158	221,801	26,551	9,835	15,031	51,417	15,081
London,	37,168	34,167	2,936	1,365	2,366	6,667	2,981

Here we find 9,835 dying between the third and end of the sixth month, and only 15,031 between the sixth and end of the twelfth, or 4,639 less than if it continued at the same ratio as from the third to the end of the sixth, as the mortality would at that rate amount to 19,670 instead of 15,031.

While these statistics indicate so plainly the decrease of mortality during the eruption of the temporary teeth, there even appears to be a somewhat analogous circumstance connected with the evolution of the permanent set; for about the sixth year we find the degree of mortality became again suddenly reduced, and remaining extremely small until the age of puberty.—(*Quetelet*.) Whether or not the decrease in the rate of mortality depends upon, or is connected with, the conditions existing at the eruptive stage of dentition, may be difficult to determine, but the tables above quoted clearly demo-

strate, that as the period for the evolution of the teeth advances, the degree of mortality is checked, and appear to afford abundant evidence that the influence of this process is inconsiderable in the production of fatal effects. There seems to be little chance of fallacy in statistics of so general an application to their subject as these are, since the process of dentition is one which of necessity takes place, and does so within a limited period of time, so that were its effects of the unfavourable character generally represented, they could scarcely avoid displaying themselves in a more obvious manner.

While these remarks are advanced as illustrative of the misconception prevailing in reference to the danger resulting from dentition, it is not meant to dispute the fact that important symptoms may exhibit themselves in connection with this process as it occurs in man, but rather to point out that too great anxiety exists with regard to it as a necessary, an unavoidable cause of suffering; an opinion, if not arising from, at least increased by the obscurity and uncertainty involving the whole process, and extending to those principles upon which its treatment requires to be based.

It is in man alone, with the exception of a very few problematical cases, said to have occurred among the lower animals, that dentition is attended by those morbid symptoms manifested previous to and during the appearance of the temporary teeth; and the probability is, that in man alone do they ever occur, since an explanation of them is to be found in causes which exist in no other animal. In considering the nature of the symptoms attending laborious dentition, we must look principally to the organic condition of those textures implicated in the action going on; recollecting that the teeth during their development and rapid growth, while acting in one sense as foreign and partly inorganic bodies within the gum, are endowed with a very high degree of vascularity and sensation, so that whatever pressure they may exert on contiguous parts, re-acts upon themselves with still severer effect, that again being aggravated in its consequences by the condition of the whole nervous system at this period of life. Whatever complications may arise as secondary consequences of dentition, pain seems to be its most frequent as well as its most simple accompaniment, and it is to the immediate causes of this condition, as mainly productive in its results, of those maladies observed to be more common about this age, that attention has been so often directed. Locally, the inconvenience seldom exceeds mere irritation; that, however, sometimes increasing to a considerable extent; and although almost universally asserted, it seems not to be satisfactorily established, that this arises from the cutting or penetration of the teeth through the superincumbent tissues, but from other circumstances special to man alone.

That the passage of a tooth above the gums, constitutes the principal cause of the disorders attending dentition, is a theory which, however questionable as to its accuracy, is yet borne out by the statements of our highest authorities on the subject. Mr Hunter

remarks at p. 80 of his "Treatise on the Teeth",—"These twenty teeth, in cutting the gum give pain and produce many other symptoms which often prove fatal to children." And again, at p. 113, "While the operation of growth is going on, another operation is produced, which is a decay of that part of the gum and socket which covers the tooth, and which becomes the cause of the very disagreeable and even dangerous symptoms which attend this process. As the teeth advance in size they are in the same proportion pressing against the sockets or gums, from whence inflammation and ulceration are produced." Mr Fox, speaking of the same subject, at p. 73 of his work, says,—"The excitement occasioned by the passage of the tooth through the gums, often gives rise to the most alarming constitutional symptoms, which are always, with difficulty, alleviated and not unfrequently terminate in death." Mr Tomes, one of the latest authorities on this question, seems inclined to maintain the same opinion, and states at p. 128 of his work on "Dental Physiology and Surgery," that "Pressure, produced by the growing tooth upon the inflamed gum, and indirectly upon the formative pulp, are justly considered as the more immediate cause of these symptoms." Then, alluding to induration of the gum as another cause of them, a number of cases are cited in support of this opinion, where, after incising the gums the morbid symptoms disappeared, while the most serious results are adduced as the consequences of neglecting this operation. All of these authors, and many others advocating the same opinion—that the disorders accompanying dentition are produced by the pressure exercised by the teeth in a direction perpendicular to the socket at the time of their eruption from within the maxillae.

With regard to the teeth forcing a passage through the gums, being considered as a cause of so much suffering, it must appear remarkable, first, that this process should occasion so much more distress in man than is met with under the same circumstances in the lower animals; and secondly, that it is about the time of the appearance of those very teeth best calculated by their form, for making a way through the jaws and gums—namely, the incisors and canines—that the greatest amount of distress is experienced. Both instances are remarkable, and both admit of explanation in a similar manner, by the want of sufficient *lateral* space for the accommodation of the teeth within the jaw. When compared with those of the lower animals, much less space exists in the human maxillae for the accommodation of their proper complement of teeth; for, it must be recollected, that man stands alone as an exception to the rest of the animal kingdom, having a similar arrangement of teeth, in having no space vacant in the dental arch, not even for the reception of the canine tooth of the opposing jaw—and when we reflect, that in the nearest approximation among the lower animals to the closely packed teeth of man, there still exists an empty space adjoining the canine teeth, of at least nearly their own breadth, it must be obvious that all lateral pressure on these organs both before and after their eruption,

must be considerably diminished. But not only is there no vacant space in the human jaw, but there would not even be room sufficient for containing the teeth already present, were it not for the protracted duration of the rudimentary saccular condition in the canines, which persists after the ossification of the crown in all the other teeth. It must be recollected that the arrangement of the temporary teeth within the jaw, previous to their evolution, is not as in the permanent set, irregular and adapted to the accommodation existing for their lodgment, but in a regular arch, each tooth occupying its future relative position to the others, and all very nearly on a level at the crowns. In this position we find ossification commencing in all the temporary teeth, with the exception of the canines; as, for some time subsequently to this, the space between the lateral incisor and first molar being inadequate for the reception of the tooth destined to be placed there, ossification does not commence in it till a late period, and even then, in many cases, the space has become barely, if sufficiently large, for its accommodation.

The removal, upon the teeth being cut, of all symptoms of irritation, is, with much less probability, due to the process of perforation of the gums being completed, than simply to the dislodgment, so far, of the unyielding, enamel-coated wedges, constituted by these teeth, from being impacted between those still remaining within the jaw—only the root, or small end of the wedge of the teeth cut, now occupying its former situation. For, the tooth, having once perforated the gum, protrudes beyond it with great rapidity; and this does not arise from enlargement of the tooth itself, by the growth and elongation of its fang, but from its being actually lifted out of its socket by the contraction of the sac at its coronal portion; since the inner surface of the sac is attached to the fang, as far as that point where the enamel commences, and there becomes detached, forming in a manner a loose covering to the crown; so that, when this part of the sac is opened by the tooth, its cut edge having a fixed point by its attachment at its free extremity to the gum, and adhering as it does to the root by its deeper margin, the contraction of the portion of the sac, intermediate between these two points, dislodges the tooth from its socket, leaving a vacant space between the bottom of the alveolar cavity and the extremity of the root. In the case of the canine teeth, deficient amount of lateral space is still more obviously manifest; as here a positive increase in the size of the jaw must of necessity take place before they can possibly be accommodated. And at least, in this instance, when the canines commence to be ossified, while also rising from the alveolus, the probability of lateral pressure seems quite sufficient to account for the more urgent symptoms frequently accompanying the eruption of these teeth, without taking the penetration of the alveoli and gums into consideration at all.

Lateral pressure, from the arrangement of the temporary teeth within the jaw previous to their eruption, seems then a sufficient and reasonable explanation of the peculiar and exceptional symp-

toms attending the first dentition in man, and one which can be better supported by analogy than that attributing the disorders accompanying the appearance of the teeth to the mere piercing of the tissue enclosing them. This step in the process takes place in all animals, and they do not seem to suffer by it; and in the eruption of the permanent teeth in man, such difficulty is seldom if ever heard of. But while there would appear to exist in both these examples the same likelihood of disturbance from cutting the gum as there is during the first dentition, in neither of them have we the state of matters existing previous to and during this process to account for such symptoms, were they even to occur, since it is during the development of the first set of teeth alone that we have them arranged side by side in a regular linear series, and forming an arc so large, as with difficulty to be accommodated within the alveolar space, in contradistinction—1st, to the permanent set, where we find them scattered up and down, wherever there is room in the maxillæ for their accommodation, and which have as many years allowed for their eruption as there are months to the first set; and 2d, in contradistinction to what is found in the lower animals, where there is more than sufficient lateral space in the maxillæ for the number of teeth with which they are destined to be furnished.

Although the treatment of such cases might not be very different, whether based upon the principles we have advanced, or upon those we have objected to, still its mode of action would be viewed in a very different light in either case; for, in the familiar instance of cutting the gums of children at this period, allowing our theory to be correct, even if that operation did assist in opening a passage for a tooth through the gum, it would not necessarily afford the expected relief unless the tooth immediately made its way through the opening thus provided for it. When the gum, from induration or other causes, evidently opposes the egress of a tooth after it has completely passed the alveolus, an incision, dividing the bridle constituting such an obstacle, will be followed by the immediate appearance of the tooth, and the relief of those symptoms which may have at that particular period manifested themselves. Such cases occasionally present themselves, and are amenable to such a method of treatment; but, as is frequently done, an incision made in the gums, with the view of facilitating the cutting of a tooth which has not yet risen from the alveolus, cannot be expected to fulfil its purpose.

Cases are recorded where the gums have been again and again cut with this view, and the operation, although perhaps beneficial in other respects, yet unsuccessful for weeks in the anticipated result of the tooth's appearance. In reference to this matter, Mr Hunter states, at p. 121 of his treatise, "As far as my experience has taught me, to cut the gum down to the teeth, appears the only method of cure. It acts, either by taking off the tension upon the gum arising

from the growth of the tooth, or by preventing the ulceration which must otherwise take place.

"It often happens, particularly when the operation is performed early in the disease, that the gum will re-unite over the tooth; in this case the same symptoms will be produced, and they must be relieved by the same method.

"I have performed the operation above ten times upon the same teeth where the disease had recurred so often, and every time with the absolute removal of the symptoms." Here then, it is evident, that the scarification did not act by assisting the tooth in its efforts to make a way through the gum—nor by relieving the gum from the pressure of the tooth—as in either way the tooth would have been so far advanced before it could produce such symptoms, as to appear through the gums immediately upon their being cut; but the true rationale of the advantage derived from this practice, lay in the scarification acting as a local depletive, or as a means of counter irritation, and as such it would still be indicated for the alleviation of these symptoms, until they were removed by the exit of some of the teeth, or an increase in the size of the jaw. On such principles it would be equally efficacious in relieving the vessels, to make an incision, not upon the alveolar ridge, but somewhat laterally; and in doing so, there would be less risk of injuring the sacs of the advancing teeth—an occurrence which, although considered by some as of little importance, cannot but exercise an injurious influence. And only in those cases where either the local appearances positively indicate incisions in that particular situation, or where the tooth is elevating and stretching the mucous membrane, would it at all be preferable to cut down upon the crown of the enclosed tooth. At all events, whatever benefits might result from any method of treatment, they cannot be justifiably attributed to the fact of its having rendered the penetration of the gums or alveoli more easy for the tooth to effect, but merely as affording relief, in the meantime, to irritation temporarily present, and in expectation of an extra amount of space being shortly gained within the jaw by the evolution of some of the teeth in their natural order, and the accompanying increase of the maxillæ in size.

ARTICLE V.—*Case of Fatal Injury to the Orbit with a Walking stick.* By PATRICK JAMIESON, Surgeon, Peterhead.

I WAS called, in great haste, on the morning of Tuesday, February 13, 1855, at half-past ten o'clock, to see James Grant, a tinsmith, who had met with a severe accident. I found him sitting in a chair, his head reclining on the shoulder of a bystander. He was very pale, faint, and exhausted; had been retching; right eyelids much swollen, tense, protruding, and discoloured, but without any abrasion of cuticle, or other external mark of injury, with the exception of a

semilunar incised wound, parallel to the transverse diameter of the orbit, about an inch long, upon the central and external part of the lower lid, the convexity being downwards. The edges of the wound were in apposition, and no blood was issuing from it; but from out the closed eyelids blood continued to trickle down the cheek. From the swelling and tension of the parts, and the intolerable pain when touched, it was found impracticable to separate the eyelids, so as to admit of a satisfactory examination of the eye and its appendages, but enough was accomplished in this respect to afford a partial front view of the eyeball, and inner surface of the lower lid. The former was pushed upwards, forwards, and slightly inwards from its natural position, bulging out from close beneath the superciliary ridge of the frontal bone. It was fixed, embedded in blood-gorged, torn appendages, but it retained its spherical shape; had a glazed or sodden aspect, pupil fully dilated; the inner surface of lower lid was lacerated and congested. I did not think it necessary to run the risk of farther injury, by probing the wound to ascertain its true direction and depth, it being abundantly evident that the weapon, whatever it was, had entered the orbit through the wound in the lower lid, pushed the eyeball upwards and forwards, and, passing behind it, had probably reached as far as the roof of the orbit.

The patient was quite conscious, answered questions readily, distinctly, and without hesitation; swallowed a glass of wine, and retained it; his pulse was 52, regular, compressible. He could stand, but not walk, complaining of inability to move his limbs, from their being numbed or "dozened." It was observable that the left leg was more affected than the other; the upper extremities presented nothing remarkable in this respect. He was placed in bed, had bottles of hot water to his legs and feet, leeches to his right temple and brow, warm fomentations to the eye.

Three P. M.—Eyelids dark purple, swelling and discoloration extending to temple and brow. Complains of the weight of the fomenting cloths occasioning increased pain, and removes them with his own hand. Pulse 48, feeble, oppressed; breathing slow, regular; no stertor; skin moderately warm: in other respects the same.

Nine P. M.—Eyelids quite black; their distension increased, separated half an inch by protrusion of eyeball, which is fixed and glazed, but retains its natural shape; sclerotic, œdematous, and highly congested; no apparent structural change can be observed: hæmorrhage has ceased. He has been restless the last hour, frequently changing his position; continually moves his right hand over the injured eye; complains of great pain in his eye and head; wanders a little in his mind at times, and has more than once attempted to get out of bed. Pulse 54, full, sluggish; skin hot; micturition free; was bled from the arm to six ounces.

Wednesday, February 14th.—Passed, on the whole, a quiet night;

is quite collected; articulation distinct; pulse 50, regular, compressible; is drowsy, and not readily roused; dullness of hearing perceptible; breathing slow, oppressed, and stertorous; left side completely paralysed; converses with those around him, and takes whatever is offered him.

Nine P. M.—Has had frequent shivering fits throughout the day, so severe as to alarm the attendants. The last one at 4 P. M., which seemed to threaten speedy dissolution: the symptoms of disorganisation of the brain have increased in intensity, but he is still quite conscious, and converses when roused; does not complain so much of pain, as of a feeling that his head would burst.

Thursday, February 15th, 8 A.M.—Moribund; the patient continued to converse with his attendants till between five and six o'clock, A.M., gradually passing into a comatose state, and died at eleven A.M., about forty-eight hours after receiving the injury.

Post Mortem appearances twenty-four hours after death.

The body, being uncovered, presented the appearance of a fine healthy subject. There was no injury on any part of the body except in the right orbit. The right eyelid was much swollen and discoloured. The eyeball protruded, so that the iris was perceptible, notwithstanding the distended condition of the eyelid. An ichorous and bloody fluid issued from between the eyelids and ran over the cheek; while manipulating and dissecting out the eyeball, a thick, dark brown, semi-fluid substance proceeded from the orbit. The eyeball was entire. The sclerotic coat was entirely vascular. The back part of the roof of the orbit was fractured.

The scalp was reflected, and the upper portion of the cranium sawn off in the usual way. The superficial blood-vessels of the brain were highly vascular. On removing the brain, an irregular radiate fracture was found on the base of the skull, at the junction of the orbital process of the frontal bone with the small wing of the sphenoid, to the outside, and, for the most part, in front of the optic foramen. All the fractured pieces were nearly in apposition. A sanious purulent fluid lay on the fracture, and could not be removed by sponging or otherwise, so that the dura mater, at this part, did not admit of being accurately observed. The dissecting knife picked away several loose pieces of bone. The direction of the wound seemed to be a line drawn from the middle of the lower margin of the orbit to the optic foramen. A quantity of thick, dark, red fluid lay on the seat of the fracture, and a similar deposit was observed on the corresponding part of the anterior lobe of the brain. Into this part of the brain the finger could be pushed without using any force, the substance of the brain having apparently participated in the puriform degeneration. There were no coagula of blood in the brain. On laying open the ventricles, they were found more or less filled with the same fluid already mentioned. The mouth, nasal passages, pharynx, and larynx, were not examined.

The chest, externally, was well arched. The lungs and heart, with their investing membranes, were healthy. A slight deposit of a flocculent substance was seen on the apex of the heart, the result, probably, of a former slight inflammation.

The stomach contained about a breakfast-cupful of a dark brown semi-fluid substance, smelling strongly of beer. The coats of the stomach were healthy, and indicated that digestion was going on at time of death. Liver and intestines healthy. The right kidney was highly vascular, but otherwise healthy. Left kidney not examined. Bladder healthy, filled with urine, but not distended.

The writer had not an opportunity of seeing particularly what injury was done to the soft parts of the orbit, as the eyeball was removed for more careful dissection.

Remarks.—The instrument which inflicted the wound was a walking-stick, not by any means a stout one, but armed with a small iron ferrule, having a knob at the extremity, about the size of a large pea. It was thus enabled to inflict a penetrating, instead of a contused wound.

The symptoms of disturbance of the brain's functions on the second day, indicated clearly, that either the optic nerve, or the roof of the orbit had been injured; but the continued consciousness of the patient precluded the idea that the substance of the brain had been penetrated.

It is interesting, in this case, to observe, the gradual sequence of the symptoms of compression,—the appearance of paralysis on the first day—its persistence and increase of intensity on the second—the drowsiness on the second day—and, finally, the coma on the morning of the third; and although these and similar symptoms increased as the case progressed, the pulse never gave indication that reaction was impending, or had come on.

ARTICLE VI.—*Case of Tape-worm which resisted treatment until the source of the disease was removed.* By R. W. CRIGHTON, M.D., Edinburgh.

IN December 1853, I was consulted by J. B., æt. 40, a calico-printer, on account of tape-worm, with which he had been troubled for six or seven years. On inquiring as to what means he had employed for his relief, I was much struck at the long list of medical practitioners under whose care he had been, but without obtaining respite for any length of time from his distressing complaint.

He stated that the remedies employed had generally been successful in bringing away portions of the parasite, but that after a greater or less time, never exceeding a few weeks, his symptoms, itching, pain, etc., had invariably returned.

The observations of Dr Nelson on the development of *Tænia*

*Solium*¹ immediately occurred to me, and led me to ask him whether he was in the habit of eating animal food uncooked. After some hesitation, he admitted that he was, that he had acquired the practice in his native county, Lancashire, and that since his removal from it to Derbyshire, his complaint had increased much, owing, he thought, to his not having fish so frequently as before.

He assured me that the practice of eating raw meat was quite common among the Lancashire operatives, and seemed quite incredulous when told that it would be the origin of his disease.

Although he used both beef and mutton, he preferred the latter, and used more of it in a raw state. When questioned as to the frequency of his taking it uncooked, he allowed that he did so at least once a-week.

He had beside him many different medicines, supplied or prescribed by various practitioners, which from time to time brought away joints of the worm. I therefore ordered him no more vermifuge remedies, but a brisk purgative, as his bowels were sluggish, and one of the preparations of iron, on account of his being weak, and anæmic. I enjoined him also, if he wished to get rid of his tedious ailment, to avoid raw flesh in future. I did not see him again for some time, but learned from one of his employers, that his health had improved much, and that he was able to attend to his duties from which he had been laid aside.

On inquiring after him during the summer of 1854, I was glad to find him nearly free of his complaint, and during the present month (May 1855), he states that he has been completely well since the end of last summer, not having seen any portions of the worm since the beginning of September, and that he has entirely abandoned the practice of eating uncooked animal food.

The case narrated is an illustration of the advantages that accrue to practical medicine from the cultivation of pure science, and shows that the importance of inquiring into the hygienic influences to which a patient may be subjected, is sometimes of greater consequence than the administration of remedies.

CHAPEL-EN-LE-FRITH, May 1855.

ARTICLE VII.—*Tabular Report of Cases treated in the Edinburgh Eye-Infirmiry during the last Five Years.* By the Medical Officers.

TO THE EDITOR.

DEAR SIR,—It has been suggested to my colleague Dr Hamilton and myself, that a quarterly list of the cases of ophthalmic disease treated at the Edinburgh Eye-Infirmiry might not be unin-

¹ Monthly Journal, June 1852, where reasons are given for the belief that the *Cysticercus Cellulose* found in sheep and other domestic animals, is transformed into *Tœnia Solium* in man.

interesting to some of your readers. In a mere statistical point of view, such lists become useful as they accumulate; but occasionally they may be accompanied with advantage by a few words of commentary.

As introductory to the first of these quarterly reports, I present the following tabular view of the cases that have occurred during the past five years; and if agreeable to you, and future opportunities are granted, the same may be reprinted, with the addition of an annual column, from year to year.—I remain, your obedient servant

BENJ. BELL.

Classification of Diseases.	During the Years				
	1850.	1851.	1852.	1853.	1854.
Tumours of Eyelids, - -	12	12	5	7	8
Wounds, Inflammation, Abscess of do.,	5	6	9	10	13
Hordeolum, - - -	2	5	7	5	6
Ophthalmia Tarsi, etc., - -	58	55	30	45	18
Inversion of Eyelids, - -	3	2	1	...	1
Eversion of Eyelids, - -	1	1	...	3	...
Trichiasis, - - -	4	3	...	2	2
Ptoſis, affection of third nerve, -	3	1	1	2	1
Epiphora, - - -	5	4	6	6	5
Do. from Everted Puncta, -	2	3	1	...	1
Inflammation of Lacrymal Sac, Fistula, etc., - -	16	1	9	10	13
Strabismus, - - -	9	11	11	9	2
Protrusion of Eyeball, - -	3
Oscillation of Eyeball, - -	2	...	2	...	1
Wounds of Conjunctiva, foreign bodies under lids, - -	14	11	9	10	12
Pterygium, fatty tumours of Conjunctiva, etc., - -	3	2	...	5	3
Xerophthalmia, - - -	1
Inflammation of Conjunctiva, acute, } Do. do., chronic, }	119	37	24	30	50
Do. Catarrhal, - -	5	9	4	1	1
Do. New-born Infants, -	5	2	6	5	8
Do. Gonorrhœal, - -	3
Do. Muco-purulent, - -	9	17	1	...	5
Do. Phlyctenular and Pustular, - -	15	25	22	35	34
Do. Strumous, - - -	23	21	21	21	9
Do. Catarrho-rheumatic, -	10	13	9	7	6
Granular Conjunctiva of Palpebræ, -	3	11	4	6	5
Wounds of Cornea, "Fires," etc., -	19	26	19	25	28
Inflammatory affections of Cornea, -	21	33	34	21	30
Abscess of Cornea, - - -	1	3	2	6	5
Ulcers, vascular speck of Cornea, -	5	22	26	31	24
Opacities of Cornea, - - -	16	27	20	29	16
Staphyloma, - - -	5	1	2	...	3

Classification of Diseases.	During the Years				
	1850.	1851.	1852.	1853.	1854.
Hydrophthalmia, - - -	1
Conical Cornea, - - -	1	1	...	1	...
Prolapsus Iridis, - - -	1	1	...	2	2
Detachment of Iris from ciliary body, -	1
Wounds of Iris, - - -	5	3	8	1	3
Fissure of Iris, - - -	1
Inflammation of Iris, - - -	4	1	4	...	4
Do. Strumous, - - -	2	1	3	2	3
Do. Rheumatic, - - -	1	8	1	6	1
Do. Syphilitic, - - -	2	2	1	4	1
Do. Aqueous Membrane, -	...	4	1	3	2
Synechia, anterior and posterior, Opacities of Capsule, - - -	9	3	4	13	6
Cataract, idiopathic, - - -	12	16	19	20	14
Do. traumatic, - - -	10	6	3	3	5
Do. congenital, - - -	2	1	2	2	1
Dislocation of Lens spontaneous, -	...	3	2	...	1
Do. from injury, - - -	1
Myopia, - - -	1	3	...	4	2
Presbyopia, - - -	2	...	1	1	4
Asthenopia, - - -	8	11	6	7	9
Nyctalopia, or Day-blindness, -	1	...	1
Hemeralopia, or Night-blindness, -	2
Amaurosis, Glaucoma, Retinitis, etc.,	42	35	38	34	38
Cases not classified, - - -	20	25	38	12	...
	525	545	473	504	472

These figures represent the number of Patients, the more prominent disease alone being specified in each case, although the numbers might have been considerably augmented, by enumerating all the affections which were frequently associated in the same individual.

Quarterly Report of Cases from January 1st to March 21st, 1855.

Tumours of Eyelids, - - -	-	-	1
Wounds, Inflammation, Abscess, of do., -	-	-	2
Ophthalmic Tarsi, - - -	-	-	19
Trichiasis, - - -	-	-	2
Epiphora, - - -	-	-	2
Inflammation of Lacrymal Sac, Fistula, etc., -	-	-	4
Strabismus, - - -	-	-	2
Protrusion of Eyeball, - - -	-	-	1
Wounds of Conjunctiva, foreign bodies under lids, etc., -	-	-	3
Acute Inflammation of Conjunctiva, - - -	-	-	10
Chronic Do. do., - - -	-	-	15
Do. Do. New-born Infants, - - -	-	-	1
Do. Do. Muco-purulent, - - -	-	-	2
Do. Do. Phlyctenular and Pustular, - - -	-	-	10
Do. Do. Strumous, - - -	-	-	2
Do. Do. Catarrho-rheumatic, - - -	-	-	2

Wounds of Cornea, "Fires," etc.,	-	-	-	7
Inflammatory affections of Cornea,	-	-	-	7
Abscess of Cornea, -	-	-	-	2
Ulcers, vascular speck of Cornea,	-	-	-	6
Opacities of Cornea,	-	-	-	4
Staphyloma, -	-	-	-	2
Hydrophthalmia, -	-	-	-	1
Inflammation of Iris, rheumatic,	-	-	-	2
Do. syphilitic, -	-	-	-	1
Inflammation of Aqueous Membrane, -	-	-	-	2
Synechia, anterior and posterior, Opacities of Capsule,	-	-	-	3
Cataract-Idiopathic, -	-	-	-	3
Myopia, -	-	-	-	1
Asthenopia, -	-	-	-	2
Amaurosis, Glaucoma, Retinitis, etc., -	-	-	-	13
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ARTICLE VIII.—*Larrey and Military Surgery.*

MILITARY surgery has been brought into such prominence by the casualties in the Crimea, that the memory of Larrey is instinctively revived on all hands. Opportunely with this feeling, a sketch of his career has appeared in the *Dublin University Magazine*, bearing evident marks of proceeding from an able pen, which has enriched the pages of our own Journal; and, we have, therefore, great pleasure in making a few quotations, which will probably not be without interest at the present crisis.

THE AMBULANCE VOLANTE, WHAT IT WAS.

"The *ambulance volante*, as organised in the army of Italy, in 1797, formed a legion containing about 340 officers, sub-officers, and men, distributed into three divisions. Each division had a surgeon-major commanding, two assistant-surgeon-majors, twelve sub-assistant-surgeon-majors (two of whom acted as apothecaries), a lieutenant-providore of the division, a sub-lieutenant, a *marechal des logis en chef* (equivalent to serjeant-major of cavalry), two brigadiers (equivalent to corporals of cavalry), a trumpeter (bearer of the surgical instruments), twelve mounted hospital men, including a farrier, boot-maker, and saddler, a serjeant-major, two fourriers, three corporals, a drummer (*garçon d'appareils de chirurgie*), twenty-five infantry hospital men. To each division were attached twelve light and four heavy carriages, manned by a *marechal des logis en chef*, a *marechal des logis sous-chef*, two brigadiers, one being a farrier, a trumpeter, and twenty drivers. It will be seen that each of these divisions was, in fact, a corps complete within itself. The medical officers were mounted, and all, officers and men, were suitably dressed and armed with light swords. The holsters and portmanteaus of the officers were furnished with the most necessary surgical appliances; and the men, mounted and dismounted, carried knapsacks containing reserve supplies of surgical munitions. The legion was under the orders of the surgeon-in-chief of the army: its administration was conducted by a board composed of the medical and administrative officers of the three divisions; and its discipline and manœuvres were regulated by a special code of instructions. Its duty was to take up the wounded from the field, after having given them immediate surgical assistance,

and to carry them to the hospitals of the first line. The sub-lieutenants of the ambulance and the infantry hospital men were also charged with the duty of burying the dead; and the former were authorised to require such levies of the inhabitants as might be necessary for that purpose. The carriages were two-wheeled or four-wheeled, and by their form and weight they were adapted to varieties of country. They could follow the most rapid movements of the advanced guard, and divide when requisite; so that a single medical officer, with an orderly carrying all necessaries, and attended by a carriage, could repair to any spot where assistance was required. There can be no doubt that this field-hospital-train conferred the most essential benefits upon the army into which it was introduced; but it would be a very grave mistake to attempt the introduction of a servile copy of it into our own service. What gave life and energy to the French institution was the soldierly spirit, intelligence, and zeal of Larrey; and these qualities are not the products of mere material arrangements. The organisation of the *ambulance volante* became easy when the medical officer, feeling his responsibility, and animated with the military love of distinction, put forth the powers of his will. Nor was he ever content with using a mere machine, even when he had brought it to a state which he considered perfect. When he found himself engaged among mountains of difficult access, bat-horses or mules with panniers were substituted for carriages. In the Egyptian campaign the difficulties of the desert were met and overcome by the employment of camels, bearing cradles for the wounded slung across their backs. In an unforeseen emergency, the vitality of the system proved itself in the manner shown in an incident of the battle of Eylau, when, upon the occasion of a panic created by a sudden movement of the enemy in the direction of the ambulance, Larrey, having hastened the amputation of a leg with which he was engaged, 'expressed, with force, his resolution not to abandon his post; and all his juniors, rallying around him, swore they would never quit him. In this difficult conjuncture,' he continues, 'Mr Pelchet, *officier directeur* of the ambulance, knew how to display the resources of his character, his ardent zeal, and his rare intelligence.' The surgeon-in-chief, in truth, knew how to draw out, and to foster those qualities, which, after all, are common enough among men; and his own superiors knew the value of his abilities for such work, and at what price—no very exorbitant one in the end—they could secure the use of them for the public service. The market of intelligence, zeal, and ingenuity, is not worse provided nor dearer in Britain than in France."

LARREY'S IMPROVEMENT OF INTERVALS, AND PRELIMINARY PREPARATIONS.

"At Nice, he held an examination of the young medical officers for promotion, and, as he tells us, distinguished le jeune Gouraud, who subsequently justified his judgment by attaining to the first rank. Here a remarkable feature of Larrey's system was developed into great activity by the position in which he found himself among able colleagues and zealous pupils—his juniors he always calls his pupils—advantageously placed for the observation of the note-worthy phenomena offered by a great number of internal and external ailments. He opened school, as he always did, at every moment of leisure, and gave lessons in pathological anatomy, producing, among other results, a special memoir on drowning. This course of life was prolonged for some time by the audacity of the English cruisers. And Britannia still continuing to rule the waves of the Gulf of Jouan, Larrey was invited by the representatives of the people with the army of eastern Spain, to take the direction of the surgical service of that army. He accordingly joined the head-quarters of General Dugommier before the lines of Figueras, where he arrived on the 25th Brumaire, an. III. (1794) two days before a general assault was delivered upon the Spanish fortified position. 'Stimulated by the evidences of the confidence placed in him by that illustrious general,' Larrey employed the interval in preparing apparatus of all kinds necessary for his service, and he had abundant occasion for them."

A MODEL SPANISH HOSPITAL.

"The Spaniards fought like furies, and two redoubts, which they blew up at the moment when they were entered by the French soldiers, produced a *tableau* than which it was impossible to imagine anything more frightful and more horrible. The general was struck by a shell, which caused him to share the lot of the brave who gloriously terminated their career on that day. There were seven hundred men wounded, a third of them very severely. They were all operated upon and dressed within the first twelve hours. Subsequent events were more fortunate for the French arms, and the fortress of Figueras, a *chef d'œuvre* of Vauban, fell. The provisions *de guerre et de bouche* found in the magazines were immense; 'I never saw,' says Larrey, 'such beautiful hospital stores: the bandages were like batiste, and the lint was as fine as *byssus*, the silk of which the mantles of the Roman emperors were formerly made. It was made up in little packets, tied with favours of different colours, by the Queen of Spain and the ladies of her court.'"

PREPARATION BEFORE EXPEDITION FOR EGYPT.

"I wrote to the schools of medicine of Montpellier and Toulouse, to request them to send me, with the least possible delay, a certain number of surgeons, well-instructed, courageous, and capable of bearing painful and tedious campaigns. Scarcely was my invitation known in the schools, when the honour of sharing in our perils and our glory became an object of warm contention, and shortly a hundred and eight surgeons (exclusive of the regimental medical officers) were united under my orders. I employed those who were at Toulon, during our short stay there, in preparing thirty chests of dressing materials, fit to be carried on the backs of animals in the rear of the divisions. The surgeons, at the same time, were exercised in the practice of their art, in the military hospital of instruction of the place. I had a complete collection made of instruments and utensils of surgery, and a sufficient number of flexible litters, easy to be carried into all sorts of places. Desgenettes directed the preparation and reception of medicines. The other branches of the medical service were equally provided for by the administrators in chief of the army.' We are not informed as to how many acres of lint, gallons of balsam of capivi, or tons of sticking-plaster, were embarked; but we are assured, that everything necessary or likely to be useful was provided, and that the medical stores were separated and distributed among the ships, ready for use in case of an action at sea, or at the moment of disembarkation. Doubtless they were not packed by hundreds-weight in bales, at some wholesale drug warehouse, and shot from waggons into the hold of a transport, there to be buried under a cairn of shot and shell. The surgeons were also disposed of among the transports, and so distributed, that no vessel of above a hundred men was unprovided with a medical officer."

SUFFERINGS OF THE FRENCH IN EGYPT, AND HOW THEY WERE MET.

"It was not until the fifth day that the French arrived at Damanhour, the first spot offering them any resource; and never did army experience so great vicissitudes, and so painful privations, as during the march. Struck with the rays of a burning sun, marching, all on foot, over a sand more burning still, traversing immense plains frightfully arid, where they barely found a few ditches of muddy water, almost solid, the most vigorous soldiers, devoured by thirst and overcome by heat, sank under the weight of their arms. The manner in which death approached these sufferers was strange. 'They perished as if by extinction. This death,' says Larrey, 'appeared to me sweet and calm; for one of them said to me, at the last moment of his life, that he found himself in a state of comfort inexpressible.' They were also continually harassed by swarms of Arabs, among the first of whose victims was a surgeon of the

ambulance. Amidst these troubles, Bonaparte was kicked by an Arab horse, receiving a very severe contusion on his right leg, which threatened mischief; but the case was very soon brought to a happy conclusion by the cares of Larrey, notwithstanding the painful march and the natural activity of the patient, which forbade repose. Arrived at Cairo, Larrey lost no time in organising a sort of head-quarters for his department. He formed a school of practical surgery for the instruction of the young surgeons of the army, and addressed to his colleagues, surgeons of the first class, a memoir on the epidemic ophthalmia, which began to show itself, in a formidable manner, among the troops. The climate and the sabres of the Mamelukes provided patients in abundance, many of whom had limbs cut clean off by those terrible weapons. The repose, too, they were beginning to enjoy after the first storm had passed over, was disturbed by what the Frenchman calls a revolt of the people of Cairo, in the course of which he had himself a narrow escape, when passing through a 'horde of assassins,' in a vain attempt to assist General Dupuy, who was mortally wounded by a lance. On returning to his duty, what was his astonishment to find the bleeding corpses of two worthy comrades, Roussel and Mongin, surgeons of the first class, stretched on the threshold of the hospital, where they fell fighting with many other brave soldiers. 'They caused the asylum of the sick to be respected, but it was at the cost of their lives.'

"Larrey accompanied Bonaparte throughout the campaign of Syria, and took his part in all the important transactions of that disastrous expedition. In foreseeing and preparing to meet the new forms of danger, and the unprecedented difficulties of this warfare, he showed his accustomed penetration, and the fertility of his mind. His ambulance carriages, for example, were no longer available, and he therefore procured a hundred wicker-work cradles, which he had suspended, by pairs, by means of elastic straps, one on either side of the humps of fifty camels. In each of these baskets a wounded man could lie at full length. The means of transport were, he says, the first object of his attention. He himself mounted a dromedary, and rode hither and thither over the desert, to whatever spot was most encumbered with the sick or wounded. In the course of this service, the want of materials for broth for his patients taught Larrey the use of a dead camel, which he found to be very superior to horse-flesh, being nourishing, and very agreeable to the taste. Before St Jean d'Acre the plague showed itself among the troops with frightful violence, and there was great difficulty experienced in the establishment of hospitals. Scarcely any spot could be found safe from the sorties of the besieged. The only beds procurable for the sick were the leaves of reeds, of which there was but a scanty supply. Wine, vinegar, and medicines were wanting. There was great misery, in every form, experienced by the whole army. During the siege, Larrey never enjoyed a moment of calm and of perfect repose. The wounded amounted to about 2000, among whom were many officers of rank. Cafferelli, who had honoured Larrey with his esteem and friendship, and who had even conceived a project for the improvement of the condition of military surgery, he was, to his eternal regret, unable to save. The chief engineer, Sanson, Duroc, Eugene Beauharnois, Lannes, Arrighi, narrowly escaped. The latter, when in the breaching battery, had his carotid artery divided by a ball, and was only saved by the promptitude of a gunner, who afterwards became Monsieur Pelissier, an officer in the Imperial Guards, in thrusting his fingers into the wound, and keeping them there until Larrey arrived, and secured the bleeding vessel, in the midst of a storm of bullets and balls. At length, after thirteen successive assaults, the genius of Bonaparte yielded before the obstinacy of Sir Sydney Smith; and the siege of Acre having been raised, it was determined to retreat upon Egypt, carrying off all the wounded. For this purpose, as Larrey states, Bonaparte gave up his own horses, and marched on foot with the army. The evacuation of the wounded was accomplished with great success, and Larrey refers to it with satisfaction, as a grand triumph of

field surgery. He seems, truly, to have been very glad to get back to Cairo, near which they were met by General Dugua, who came out, at the head of the garrison, to welcome and assist their return. 'With what pleasure,' exclaims Larrey, 'did we again see our brave companions! Fatigued by the labours of a long campaign, enfeebled by continual privations, blackened by the burning sun of the desert, we embraced brothers and friends, bound to us by interest and glory, in the spot where we had created a new country, in the midst of a strange people.' "

LIKE FERTILITY OF RESOURCE AT THE PYRAMIDS.

"At the first battle of Aboukir, Larrey again evinced his aptitude in accommodating means to ends, by substituting hospital boats for carriages or camels, and in them conveying the crowd of wounded men without any accident to Alexandria. These boats were provided with flexible litters, wine, vinegar, brandy, so as to form a sort of reserve magazine of medical munitions. The routine practice of that army was manifestly for each officer, charged with a department or a post, to do the best he possibly could to advance the service, caring little for old formularies or customs. There was no waiting for orders from a departmental chief at Paris; whatever was known to be necessary and possible was resolved upon, and done at the same instant."

THE DEPARTURE FROM EGYPT INCIDENTALLY REVEALS THAT ONE ENGLISH HOSPITAL WAS WELL MANAGED DURING LAST WAR—THE CAUSE OF THIS PHENOMENON.

"At the conclusion of the Egyptian campaign 'nothing succeeded.' Sickness increased. The hospitals became over-crowded, and the material and personal means of performing the duty daily grew less. In the defence of Fort Marabou two surgeons were killed, and a third had a leg shot away; while, during the blockade of Alexandria, the cavalry horses had to be killed for food for the sick—and very good broth their flesh made, and very agreeable to eat, with some little care in the preparation, although certain pusillanimous and unenlightened persons murmured against its use. 'At least,' says Larrey, 'I was very happy by my example to establish confidence in this fresh provision, the only kind we were able to get.' At length matters arrived at an extremity, and a council of war having been held, at which the two chief medical officers assisted, it was agreed that further resistance was impossible. A capitulation was accordingly concluded, the report of the medical officers being annexed to the articles, and the army was allowed to return to France with all the honours of war. The moment the capitulation was signed, Larrey visited the English camp and hospitals, of which he gives a favourable account. The field-hospitals, he says, were well kept, and provided with everything necessary, under the direction of the inspector-general¹ (M. Yonck, as he calls him), *who had the entire medical and administrative control of the service, with no middleman between him and the General-in-Chief.* Larrey and M. Savaresi, then acting physician-in-chief, were named members of the Commission of Armament for carrying out the terms of the capitulation, and to them, in concert with Inspector-General Young, was confided the arrangements necessary for the removal of the sick and wounded. Thirteen hundred of these, not including a corps of invalids, were embarked in twelve hospital ships, and sailed with the army. Three hundred were left at Alexandria, confided to the care and kindness of Inspector-General Young, and two months later they all returned cured to France."

¹ The reader will find allusions made to Mr Young in the interesting lecture of Sir George Ballingall, at pp. 474-5 of the present Number.

THEORY AS TO OPERATIONS.

"It is a canon of Larrey's, that in order to afford important operations a fair chance of success, they should be performed within the first twenty-four hours after the shock of the wound that renders them necessary. The proper method is, he says, to place the field hospitals as near as possible to the line of battle, and to form head-quarters, to which all the wounded requiring operations should be brought to be operated upon by the surgeon-in-chief, & under his immediate observation. One ought always to begin with those most dangerously hurt, without regard to rank or distinctions. Those less injured can wait till their brothers-in-arms, horribly mutilated, have been treated, otherwise these will cease to exist in a few hours, or not live beyond the morrow. It is easy for those slightly wounded to repair to the hospitals of the first or second line, especially for officers, who commonly do not want means of transport."

THE RUSSIAN FIELD.

"The march from Posen to Pulstusk and back to Warsaw, accomplished by the French Imperial Guard in nineteen days, was one of extreme difficulty. In many parts the men marched through thick mud, reaching to their waists and to the bellies of the horses; yet the field hospital kept its place, and the light spring waggons, on two wheels, were found to work better than four-wheeled carriages, or even bat-horses. The sick list grew larger accordingly, a circumstance which Larrey turned to advantage, by devoting one day in each week to a clinical conference at his hospital at Warsaw, where he was about to open a complete course of military surgery, when the trumpet again sounded, and, on 1st of February 1807, he was obliged to follow the army. There was about three feet of snow on the ground, and the thermometer was six or seven degrees below zero R. when they left Warsaw. At the battle of Eylau, fought on the 7th of the same month, Larrey, being the only inspector-general present, had the direction of the medical service; and his account of his work is truly terrible. The army bivouaked on the night of the 6th; the thermometer that morning having fallen to thirteen or fourteen degrees below zero. The field hospital was in open barns, from the roof of which the straw had been taken for the use of the horses. The wounded were laid upon the refuse of this straw, covered with snow. The cold was so extreme that the instruments often fell from the hands of the assistants. Larrey happily retained a supernatural strength, excited, no doubt, by the grand interest with which those honourable victims inspired him. 'The ardent desire,' he continues, 'that we felt to save the lives of these brave men made us persevere. The night arrived, and we had not had a moment's time to satisfy the wants of nature. And in the midst of what torturing scenes had we to discharge our sad, but useful duty! While I operated I heard my services called for from all sides, with the most pressing entreaties. It is true that the moans of these intrepid soldiers were succeeded, after the operations, by a prodigious calm and a sort of internal satisfaction which they expressed by demonstrations of the most lively gratitude. They no longer seemed to be occupied by their personal sufferings; they prayed for the preservation of our Emperor, and the success of our arms.' It was upon this occasion that the alarm was given, by the advance of the enemy, to which we adverted in a previous page. It was quieted by a successful charge of the cavalry of the guard, made in the midst of whirlwinds of the thickest snow. All the severe wounds of the guards, and most of those of the soldiers of the army, were dressed within the first twelve hours, and then only had the medical officers a moment of rest. 'We passed the night,' says Larrey, 'on the frozen snow, around our bivouac fires. Never did I pass through a day so painful; never was my soul so deeply moved. I could not restrain my tears even when I strove to sustain the courage of my wounded.' Another of Larrey's canons of military surgery ruled the proceedings of the

ensuing day; the wounded were all removed—the worst cases to Eylau, the remainder a distance of fifty-five leagues, to Inowraklaw, beyond the Vistula. Prompt evacuations of the wounded, upon the bases of military operations, Larrey considers to be necessary, in order to prevent the epidemics that always attend the crowding of a multitude of sick into one place, and also to be useful in raising the spirits of the men, and highly beneficial as regards the effects of motion upon the wounds, which heal better even when it is somewhat rough. The results justified the theory upon this occasion as upon many others; but it must not be forgotten that the operation demanded much care and forethought on the part of those who conducted it. The Assistant-Surgeon-in-Chief, Paulet, was ordered to repair immediately from Warsaw to Inowraklaw, to make the necessary preparations for the reception of the convoys, each of which was accompanied by a sufficient number of medical officers, sub-officers, and hospital men. Their quarters and soup were made ready for them, at each station, by sub-officers, who marched in advance; and M. le Commissaire Ordonnateur Dufour displayed a zeal and activity in all those administrative operations that entitled him to the gratitude of the troops. The services of Larrey and Percy in this battle were rewarded by crosses of Commanders of the Legion of Honour, and the rank of Chevalier was conferred on many of their subordinates."

EXPEDIENTS AS TO DIET AND SURGICAL APPLIANCES.

"The wounded collected in the isle of Lobau suffered much from a 'grand penury' of commissariat supplies. An excellent soup was, however, made of horse-flesh, and seasoned with gunpowder, the latter of which, we are assured, did not, as might be supposed, impart its black colour to the broth, which was clarified in the process of cooking.

* * * * *

"At Witpsk, a considerable battle was fought; and there the sufferings which subsequently reached so unparalleled a height, began. The surgeons were obliged to use their shirts for the first dressing of the wounded; and the misery of the disabled Russians, who had been abandoned or forgotten, was extreme. Four hospitals were established at Witpsk. At Smolensko, where the grand army had 1,200 killed, and 6,000 wounded, the appropriate penalty of its grandeur became still more galling. It was necessary to use the records found in the Archives for dressings; the paper was employed for bandages, the parchment for splints, tow and the down of the birch-tree served for lint. Forced to 'imagine the means' of supplying their wants, they bedded the sick upon heaps of paper."

HOW TO AVOID FROST-BITE.

"Owing to the deficiency of superior medical officers, Larrey himself was obliged to perform about 200 amputations in the first twenty-four hours [after Borodino], and that in despite of a bitter northerly blast, which rendered it very difficult to keep the wax torches lighted during the night. Over the horrors of Moscow, and of the retreat, we must not linger. Larrey lived and worked through them all, bravely struggling in vain efforts to do his duty, even long after the disorganisation of the army had unmistakeably commenced. Death approached him in every shape. At Dorogobouje, he barely escaped with life from the flames of a burning hospital, in which many victims perished. While his comrades sank under the influence of cold, in which Reaumur's thermometer fell to nineteen degrees below zero, the Baron, always marching on foot, and careful never to approach a fire, escaped."

DEVOTION OF THE SOLDIERS TO LARREY.

"At the passage of the Beresina, a touching proof was afforded that the misery of the wretched crowd of fugitives had not deadened their sense of honour and

gratitude. After the guard and the first and fourth corps had passed the river, one of the temporary bridges broke down under the weight of some heavy guns, and at the same moment Wittgenstein's corps of Russians attacked the rear-guard, and poured a heavy fire of shot and shell upon the immense crowd pressing in disorder towards the remaining bridge. Soldiers, camp-followers, women and children, were crushed together in a confused mass, with wagons, guns, and horses, or forced into the water. Larrey had repassed the second bridge, to seek some cases of instruments of surgery, of which he had great need for the wounded. It, too, broke down; and in his attempt to return he was at the point of perishing in the crowd, when, he says, 'happily I was recognised; instantly every one hastened to favour my efforts; passed by the soldiers from hand to hand, I found myself, to my great surprise, in a few moments on the bridge. This proof they gave me of their attachment, under such circumstances, soon made me forget both the dangers I had incurred, and the loss I had sustained.' "

WATERLOO, AND HIS FLIGHT FROM THENCE.

"Larrey performed many operations on the day of Waterloo, and continued his labours until the English cavalry charged up to his field-hospital, and the daylight failed, when he found himself obliged to follow the advice which the Emperor sent to him by an aide-de-camp, to make for the frontier by a certain cross-road. Scarcely had they proceeded a league or two, when their retreat was cut off by a body of Prussian lancers. 'I marched,' he says, 'at the head of my little company, and in the persuasion that our enemies were not numerous, I did not hesitate to force a passage, sword in hand. Having fired both my pistols upon the horsemen who stopped our way, I made a lane, through which my companions and my servant passed at full gallop, but my horse, wounded by a ball, fell, and, at the same moment, I received a double sabre cut on the head and left shoulder, which brought me senseless to the ground.' The Prussians left him, to follow his companions, and shortly afterwards, having recovered from his faint, he mounted his horse, which had also regained its feet, and riding through some corn-fields, found himself near the banks of the Sambre at break of day. There he again encountered the Prussians, and all bravery being useless, he surrendered. In spite of his submission, he was pitilessly disarmed and stripped; the officers divided the contents of his purse, some forty Napoleons, among themselves; they took his arms, ring, and watch, and, owing to his height and his grey greatcoat, mistaking him for the Emperor, they brought him before the Prussian commander of the advanced guard. By him he was sent, with his hands tied, to another general of higher rank, who, in a sudden access of rage, and believing him to be Napoleon, determined to shoot him. There was fortunately no bandage at hand to cover his eyes, and a surgeon, who was ordered to apply a piece of sticking-plaster for that purpose, recognised in the forlorn prisoner, his former teacher at Berlin. The proceedings *faire passer par les armes* were thereupon suspended; and the poor Baron, his hands tied behind his back, his head covered with bloody rags, his feet bare, and scarcely covered by his greatcoat and pantaloons, was brought before General Bulow, and subsequently sent on to Blücher. The grim old marshal received him kindly, asked him to breakfast, and having presented him with twelve gold Fredericks, he sent him, in charge of one of his aides-de-camp, to Louvain. Bread thrown upon the waters was here, after many days, found. Blücher's son, grievously wounded and a prisoner, had been tended by Larrey after one of the battles of the campaign of Austria. At Louvain the aide-de-camp asked merely for a billet for a wounded Frenchman, whose name he could not tell; and Larrey, quartered upon a poor woman, who had scarcely wherewithal to sustain herself and her children, obtained with difficulty, in exchange for one of Blücher's gold pieces, some onion soup, and the favour of having a young surgeon to dress his wounds. 'Shortly after,' he

continues, 'I saw a young medical officer come in and prepare to fulfil his mission, when suddenly he exclaimed, 'You are Baron Larrey,' and scarcely had I replied, before he rushed down the stairs and disappeared without uttering a word.' All was soon set right, the young surgeon shortly returned with a municipal officer, and the Baron was forthwith comfortably lodged in the house of a celebrated advocate, M. Yonk, from whom he received the utmost kindness."

Part Second.

REVIEWS.

THE USE OF BELLADONNA IN SCARLATINA.

1. *Travaux Thérapeutiques sur la Belladone.* Publié par A. L. J. BAYLE (Tome Seconde de "Bibliothèque de Thérapeutique.") Paris, 1830.
2. *A Treatise on the Principles and Practice of Homœopathy.* By FRANCIS BLACK, M.D. London, 1842.
3. *Homœopathy fairly Represented.* By WILLIAM HENDERSON, M.D., Professor of General Pathology in the University of Edinburgh. 2d Edition. Edinburgh, 1853.
4. *On the use of Belladonna in Scarlatina.* Article in "British and Foreign Medico-Chirurgical Review." No. 29. January 1855. By J. WARBURTON BEGBIE, M.D.
5. *Prophylactic Power of Belladonna.* Article in "British Journal of Homœopathy." No. 52. April 1855.

THE attention of the profession having lately been called to the subject of the employment of belladonna as a prophylactic in scarlatina, we take this opportunity of laying before our readers the views we ourselves entertain. In 1799 Hahnemann, then resident at Königsutter, discovered, as he supposed, the prophylactic virtue of belladonna in scarlatina, and two years thereafter, in a pamphlet published at Gotha, made known his discovery. We may here acquaint our readers with a circumstance, which, in our opinion, scarcely redounds to the credit of that "magnate," premising, however, that we should not now have referred to it, had we not lately seen attempts made to gloss over the impropriety of which Hahnemann was guilty in the interval which elapsed between his supposed discovery of the prophylactic action of belladonna, and his making

it known to the world.¹ Concealing the name of the remedy, he offered his supposed discovery for sale, and it was only when he failed to find a purchaser that he made it public. For ourselves, we are of the number who do not desire to impute dishonourable motives to Hahnemann (any more than to Drs Black and Henderson, when they so strangely mislead their readers); but had Jenner so acted; had he, concealing the source of his vaccine virus, endeavoured to enlist the interest and sympathy of the profession and of the community at large in his discovery, by subscribing it, by giving or sending little portions of the virus, and soliciting reports upon its action after trial from the recipients—while notwithstanding such a procedure, the truth of his discovery would soon have made its magnificence known; can we doubt that the immortality of Jenner would not have been so cloudless.²

Before his supposed discovery was made known, while still the now vaunted belladonna was merely Dr Samuel Hahnemann's specific,³ thirty subscribers, whom Hahnemann had begged by letter to testify to the truth, and to publish the result (be it what it might), in the *Reichs-Anzeiger*, made no reply. "Two others, Dr Jani in Gera, and Dr Müller in Plauen," writes Hahnemann, "unsolicited by me, wrote something on the subject, but good heavens! in what a spirit."⁴ Into the controversy Hahnemann had with these two physicians, we need not enter, they decided against his specific, and forthwith, rightly or wrongly, we judge not, Hahnemann assailed them in very strong language, and with a personality and invective, which strangely contrast with the celebrated words in which his

¹ "Then came his (Hahnemann's) public writings" (says Dr T. R. Leadam in the same number of the Homœopathic Journal as contains the review), "his controversies, his efforts to establish this great truth in medicine, his persecutions, his discovery that belladonna was a prophylactic against scarlet fever, his indignation at the unfair suspicions that were cast upon him in his worldly endeavours to obtain a hearing for the new fact, and his consequent magnanimous publication of it for the good of mankind; perceiving that the world would not consent to remunerate him for the information, he indignantly yielded up his knowledge and turned aside the arrows which malignity and ignorance had forged." Had these remarks come from another quarter, no doubt they would have been assailed as unfair suspicions, but emanating from the "surgeon-accoucheur to the London Homœopathic Hospital," by whom they were delivered on the important occasion of an "Introductory Lecture;" the facts they contain must be beyond suspicion. Was not the theory of the prophylactic power of belladonna tarnished in its propounder?

² Hahnemann, too, had the advantage of having seen the manner in which the English physician's discovery, not without some trials on his part, had been made known and promulgated. To Jenner's discovery his attention must have been drawn. Except to a certain extent in date, there exists no analogy between the labours of Jenner and Hahnemann, the former made a discovery, the latter a pseudo-discovery. The contrast between the conduct of Jenner and Hahnemann may, however, be advantageously studied in Baron's *Life of Jenner*, and Hahnemann's *View of Professional Liberty at the commencement of the 19th century*. (*Lesser Writings*).

³ Hahnemann *Präservativ*.

⁴ *Lesser Writings*, edited by Dr Dudgeon, p. 421.

"view" closes, "Physicians of Germany, be brothers, be fair, be just."

Made public in 1801, eleven years elapsed before any decided testimony in favour of the prophylactic power of belladonna was published. In 1812, however, there appeared from the pen of Dr Schenck, in "Hufeland's Journal," the first of a pretty long series of favourable reports, which various physicians in Germany, some of them men of great learning and reputation, as Hufeland himself and Himly, from time to time contributed. Remarkable and interesting, however, as many of these papers are, we must affirm after reading them (a feat which Dr Black¹ and Dr Henderson have yet to accomplish), that without a single exception they are sadly deficient in precision and in details. But while this character attaches more or less to all, there are some (and of these the authors names are not the least frequently quoted), which after perusal we cannot allow to have any weight in the question at issue; for not only are the facts loosely stated, but as facts they are not of the slightest importance. As an illustration, we select the testimony of Masius whom Bayle and Black quote, and whose important statistics are included by Dr Henderson, when copying the latter. But not only do Bayle, Black, and Henderson approvingly quote the statistics of Dr Masius, many authors before and at least one since they wrote, have, from their unacquaintance with Masius' original statements, fallen into the same snare. Mr Peter H. Bird's translation of the well-known work of M. Bouchut, "On the Diseases of Children," has just issued from the London press, and in it on the subject of belladonna in scarlatina, we find the following passage,—“This is one of the most curious facts of modern therapeutics, the knowledge of which is due to the illustrious Hahnemann, whose observations leave little room for scepticism. They are, moreover, confirmed by a host of physicians, amongst whom we may mention Schenk, Masius, Hufeland, etc., etc., etc.” We shall not now speak of the injustice done to the first two gentlemen in spelling their names after a fashion they never authorised,² but shall at once by a reference to the original paper of

¹ Dr Black, in commencing his translation of Bayle, thinks it necessary to apologise to his readers for being able to introduce Germans *alone* (though after all, with his usual inaccuracy, he brings in M. Meglin of Colmar—a veritable Frenchman, as an authority). How far more complimentary to the nation whom he panegyricizes, had he quoted from the original papers of these physicians, and not from a French translation.

² The Homœopathic Reviewer insists upon spelling Dr Teuffel's name, Tuffel, asserting that he found it "sic in orig." Here we are left in doubt whether Bayle's, or Black's, or the original *Teufel* is referred to. Most assuredly Dr Teuffel knows best how to spell his own name. Besides, Teufel, as almost every well-educated person knows, is the German for Devil; and Dr Black, at least, ought to have been more true to his colours than to fail in dressing up his sooty *confrère* in the approved livery. How would he like to turn out in a French quotation as *Dr Black*? We trust that hereafter, the Devil (*sic in orig.*) will always have his due, and that Dr Black may never have reason to complain of being painted less black than he is called. Many are the indig-

Masius, enable our readers to judge to what extent he has any right to be considered as one of a host of physicians, whose observations leave little room for scepticism. During an epidemic, which continued two years at Schwerin, Dr Masius, who believed himself to be constitutionally prone to the infection of scarlatina, was occupied in treating cases of a malignant type, and taking half a grain of the belladonna in four doses every day on which he visited scarlet fever patients; the consequence, we express in his own words, "und ich blieb frei." Four of his children also, who had not had the disease, and had no restriction placed on their movements, but received the belladonna, were preserved. At another time, when the disease prevailed at Rostock, Dr Masius adopting the same plan, preserved himself and his children. Upon the strength of this experience, Dr Masius declares for Dr Hahnemann's preservative, and not content with a simple expression of his own reliance in belladonna, he denounces all unbelievers, and pointing at them, he closes his remarks in the following words,—*"Ich ehre den vernünftigen scepticismus aber ich hasse den blinden Unglauben des Zeitalters."* If we do not greatly mistake, our readers will embrace the "reasonable scepticism" so politely offered, and will reject such paltry evidence, even at the risk of "blind incredulity" being laid to their charge. Reference to the original paper of Dr Masius, in Hufeland's Journal for 1813, would have spared M. Bouchut (whom we acquit from any desire to mislead), from being accessory to the propagation of such miserable twaddle, and of permitting it to pass current as scientific investigation. Bayle is less blameworthy, in so far as he furnishes his readers with very nearly Masius' exact observations, and in another part of his volume presents them as is his wont, with a resumé of his own. It is this resumé (which not unfrequently does not correspond with the observations as given in detail, and which cannot therefore be considered as always representing in a faithful manner the statements of the original observers),¹ with which Dr Black, rejecting the

nities offered to the German physicians in this particular by Drs Black and Henderson, and we only allude to them because they might and ought to have been avoided.

¹ As an example, though by no means the most marked, of the discrepancies which exist between the "recueil" and the "resumé" in Bayle's work, we shall take leave to present the two following passages to the reader. 1st, From the translation *in extenso* :—"Chez la plupart des individus soumis au traitement prophylactique, il se manifesta, au bout de quelques jours, une éruption générale, semblable à celle de la rougeole, et tous ceux chez lesquels une telle éruption s'était manifestée, demeurèrent exempts de l'effet de la contagion." 2d, From the resumé of Bayle : "Chez plusieurs des préservés, il y eut une éruption générale apyrétique, un peu analogue à la scarlatine, et qui n'était que l'effet de la belladone, observé par Hahnemann." Dr Black translates the latter. And as representing accurately the views of the original observer, in this instance Dr Dusterberg, the "Homœopathic Reviewer," has the assurance to say, "that in every instance" the views of the German physicians "are at least fairly enough rendered, and in all but one or two, with the most perfect accuracy." By whom? our readers will say, Why, by "Dr Black and Dr

original papers, and rejecting even Bayle's account of these papers *in extenso*, has rested satisfied; while Dr Henderson, who had as easy access to the papers in Hufeland's Journal as to Bayle's work, rejects both, and betaking himself to Black, adopts him for better and for worse. We must, however, present the observations of some of the other German physicians to our readers. We take Gumpert, whose paper appeared in Hufeland's Journal for 1818. This physician, who practised at Posen during an epidemic of scarlatina, administered belladonna to his four children, and they were preserved. We should like to know if there are not scores of physicians of whose families the same might be affirmed, though no belladonna was administered! Gumpert, at the same time, however, employed belladonna in about twenty families, and with similar success.¹ Nevertheless, one person took the disease during the first week of prophylactic treatment; and another, a child, after taking the belladonna for two weeks. Regarding this physician's experience, the "British and Foreign Reviewer" justly censures Bayle and Black, and Dr Henderson ought to be included for stating that he (Gumpert) preserved twenty families, amounting to *about eighty individuals*, for Gumpert mentions no such thing, and it is evidently an assumption of Bayle's own, who, being anxious to present numerically the experience of as many physicians as he possibly could, took upon himself to calculate, that each of the twenty families, like Gumpert's own, contained four individuals.² Had the "Homœopathic Reviewer" been worth censuring, or at all likely to have his mind and manners improved by such treatment, we should be tempted to let him know our candid opinion of what he has said respecting the criticism of the "British and Foreign Reviewer" upon Gumpert's experience. He labours to prove a want of open and honest dealing on Dr Warburton Begbie's part, which it is sufficient for us to say, that he has not found himself able to establish; while he finds it convenient to pass, *sub silentio*, the single charge of gross error which that gentleman endeavours, on this occasion, to substantiate against Bayle in his resumé, and after him, against Black and Henderson. Our readers shall judge how far he was warranted in so doing. Here, again, we acquit Bayle, to a certain extent; for in the account of Gumpert's experience, as given by him "in

Henderson" to be sure!! For pointing out such discrepancies, Dr Warburton Begbie surely merited thanks, instead of which, in the elegant language of the Homœopathic Journal, "he is condemned as a traducer."

¹ It could only be from a desire not to appear hypercritical, that the "British and Foreign Reviewer" did not call attention to the discrepancy between Gumpert's own statement on this subject, and Bayle's account of it. "Noch bei einigen 20 familien," is rendered "dans plus de vingt familles." "Mit gleichem Erfolg," "et toujours avec un succès constant." *In about twenty families is translated in more than twenty families; with equal success is translated, and always with constant success.* Yet to this always constant success there were two exceptional cases!

² An admirable example of what is termed "Cooking of Statistics."

extenso," there is no mention made of *eighty individuals*. It is his own suggestion in his *resumé*. He has there taken what we regard as an unwarrantable liberty; but his error is altogether less than that of either Black or Henderson. With the opportunity of quoting Gumpert accurately, they have represented that physician as stating what he did not state. It will not do to argue that Dr Black is to be excused, because, forsooth, he only professed to deal with the *resumé* of Bayle! for this is, indeed, the point on which we and our readers have just cause of complaint. If we are asked to believe the experience of certain observers, let that experience be correctly stated, or else let a simple reference be given to original authorities. The condemnation, by the "British and Foreign Reviewer," of such conduct as Dr Black's and Dr Henderson's, was not only warranted but demanded. Had he resiled from the unpleasant duty, we must have held him as in a measure answerable for their blunders. The shield of protection with which the "Homœopathic Reviewer" has endeavoured to cover Dr Black and Dr Henderson, and the manner in which, on account of the faithful discharge of a duty imposed upon him, he has chosen to speak of the writer in the "British and Foreign Review," are, to our mind, sufficient, though sad indications of the professional habits of the gentlemen whom the "Homœopathic Journal" represents.

Behr, physician at Bernbourg, administered the belladonna to forty-seven individuals, including children and adults, during an epidemic which occurred in 1820 and 1821, lasting from September to the beginning of March. Only six were attacked by the disease. Of the six cases, Behr says, "that in three the disease was of a benign character" (*gutartiges scharlachfieber*), and in regard to the other three, after describing the case of a brother who was distressed with constant hiccough, and otherwise had the disease in a very severe form (*sehr heftiges scharlachfieber*), he adds, "that they had it in a much milder degree" (*viel gelinder*). Of Behr's exceptional cases, Bayle says, "Mais chez presque tous les six la maladie fut d'une nature si benigne, qu'aucun ne succomba." We have been precise in quoting the remarks of Behr and of Bayle, in regard to these six cases, in order to allow our readers to judge of the manner in which the experience of another of the much lauded German physicians is presented by Dr Black and Dr Henderson. "Six were attacked," say these gentlemen, "in an almost insensible manner." For this glaring misrepresentation of what an original observer has stated, the "Homœopathic Reviewer" endeavours, as usual, to excuse Dr Black, because he quoted Bayle's *resumé*, "which alone he professes to deal with." We care not to accuse or to excuse Dr Black. Our readers can now judge for themselves whether, by any received canon of criticism such an excuse can be, for one moment justified. Then, as to the facts which Behr has brought forward, of what real value, notwithstanding their interest, are they? Forty-seven indivi-

duals, twenty of whom were upwards of twelve years of age, while four were thirty-four, during an epidemic of scarlet fever, which lasted upwards of six months, had belladonna exhibited to them; and while six of the forty-seven did contract the disease, forty-one did not—is this very striking and important? Would it be still more striking if the numbers were increased,—viz., the population of Bernbourg, the scarlet fever patients in that population, the number of persons to whom the belladonna was given, and even, if you will, the number who did not take scarlet fever—in short, magnify the whole experiment several fold, and what of it, after all? Is there anything in the nature of the facts, as brought forward by Dr Behr, to lead necessarily, or even tend to the conclusion, that a few drops of belladonna, given twice daily, preserved those who did not take scarlet fever? We see no reason, and feel quite as open to the conviction, that Dr Behr's six were affected by scarlet fever because they got the belladonna, as that his forty-one escaped the disease because of it.

Having indicated the reliance, in the degree of little, less, least, to be placed on the works of Bayle, Black, and Henderson, we shall now pass to the experience of one of the German physicians, which, nearer than any other, approaches to the "experimentum crucis." Before doing so, we may say, in a word, as respects the experiences of all the others, "ex uno disce omnes." The statements of Schenck, Gumpert, and Behr, differ so little in reality from those of their "confrères," as to make any further quotation needless; besides, our space forbids, and the reader is referred either to the original papers or to the article in the "British and Foreign Medico Chirurgical Review," in which these authors' views are accurately represented. The experiment just alluded to, is that of Dr Dusterberg, who, when scarlatina menaced Warbourg, selecting an individual in every family committed to his care, from whom the belladonna was withheld, has described the subsequent infection of the whole so exempted; and attributed the infection to the drug not having been administered. Any reader of Dr Dusterberg's statements will find grave cause for scepticism as to the reality of his experiments; but apart from this altogether, we are at one with the "British and Foreign Reviewer," when he says, "The exemption from the belladonna treatment of one-half of each family would have been much fairer. The exemption of only one in each family would, undoubtedly, serve as predisposing the ones so exempted to contagion in another and very marked manner, which is not even alluded to by the experimenter. The mental influence exerted over the exempted child of each family, we hesitate not to say, would be very decided, and all in favour of his or her contracting the disease."

Our readers must not suppose that all the German physicians who have written upon this subject agree with those whose statements have now been referred to. If, in numbers, the opponents do not equal the supporters of the claims of belladonna, the depen-

dence that may be placed upon their evidence is greater, because accompanied by much precision of detail. Dr Lehmann's paper is in this respect greatly superior to any one of Hahnemann's upholders; for a ready confirmation of this opinion, the reader may refer to the original article in "Rust's Magazine" for 1826, or to the "British and Foreign Medico-Chirurgical Review:" in the latter, at page 91, and 15 lines from foot of page, he will please to read *fourteenth* for *twenty-first*, an accidental mistake, for the correction of which the world is indebted to the "Homœopathic Reviewer." For once (and we would fain accept it as a token of better things to come) the original paper of an author has been referred to, and the "Homœopathic Reviewer" has been rewarded for his trouble, by detecting a slip on the part of the "Reviewed." Such good fortune should inspire him; and on this occasion truly, the indication of the error is accompanied by such a flourish of trumpets, as to satisfy us that it was the only one he could detect.

Besides Lehmann in Germany, Mierendorf, Raminski, Teuffel, etc. etc., have added their testimony against the asserted power of belladonna. It is unnecessary in this Journal to detail the evidence of a like nature which our own country has afforded. One of the most valuable and crushing testimonies against belladonna, appeared in the "Monthly Journal," by Mr Benjamin Bell. In our opinion, the facts brought forward by Mr Bell, Dr Andrew Wood, Dr Balfour of Chelsea, and Dr Balfour of Cramond,¹ have *finally settled* the question as far as scientific men have to do with it. Henceforth it can only live as a "vestige of Hahnemannism."

But the utmost that the "Homœopathic Reviewer" can say for the "divine remedy" of the "Magnate" his master, is,—"The prophylaxis of belladonna in scarlatina may have no existence, but that such is the case has not been shown by Dr J. Warburton Begbie;" and again, "notwithstanding the misrepresentations of our opponents and all our own shortcomings, we have now, as heretofore, the best of the argument and the best of the facts." Our readers, with the assistance of the works at the head of our review, have now ample opportunity of judging for themselves, of the truth of these the homœopathic "ultima," on one of the grandest dogmas of their school.

If we were asked to indicate the track by which the greatest number of errors have crept into medical literature, we should feel no hesitation in deciding, that the practice of quoting from second-hand sources has contributed more fruitfully than any other cause to this result. Extraordinary indeed, though not therefore the less blame-worthy, has been the anxiety shown by many to avoid the trouble of reference to original authorities. An evidently necessary consequence of this practice has been the stereotyping of errors,

¹ Under the title of "Notes from Practice;" see some admirable remarks by this gentleman in "Monthly Journal" for May 1853.

ot, however, in some instances, before the original blunders have step by step become lamentably augmented. In medical literature we need seek no better illustration of this truth than the history of the so-called prophylactic action of belladonna. We find, in the first place, certain loosely observed facts and testimonies recorded after a slovenly fashion, by certain German physicians, have been far from accurately represented by the French author, the title of whose work is placed at the head of our review. The next stage in the literary degeneration of these facts and testimonies is represented by a so-called English translation of Bayle (the French author in question), in which the faulty parts of that author's work are carefully enshrined, to the exclusion of the more valuable and correct portions. Finally, we have a Scottish Professor, who, among his other notorious professions, professes to quote Bayle,¹ but in reality quotes only the English translation above-mentioned, and this after an *ex cathedra* fashion, as though the whole of the evidence had passed unscathed through the fiery crucible of his eminently critical understanding; whereby the ingenuous and simple reader, little versed in

¹ Dr Henderson's words are ("Homœopathy fairly Represented," 2d edit., p. 112), "I shall adduce from an article by M. Bayle." Dr Henderson, however, does not adduce from M. Bayle, but from M. Bayle's translator; and, following him, directs his readers to page 583 of Bayle's second volume. The fact is, as pointed out by the "British and Foreign Reviewer," that Bayle's second volume contains only 532 pages. The writer in the Homœopathic Journal labours to defend the course adopted by Dr Henderson. If Dr Henderson did not intend to quote directly from Bayle, why did he not say so at the outset of his quotation, and if he intended his readers to have the idea that Dr Black, and not he himself was translating the French author for them, why did he so distinctly say "I shall adduce from an article by M. Bayle?" The subsequent note of reference to Black's book (as a consequence of the existence of which the writer in the Homœopathic Journal pronounces Dr Henderson to be the most honest, and at the same time the most abused man, while the "British and Foreign Reviewer" is likened to a fox, having previously been compared to certain other animals of the brute creation, on account of his knowledge regarding it) is evidently the circumstance which revealed to his reviewer Dr Henderson's extreme carelessness and inaccuracy, it is, moreover, an untruthful note. "For the *whole* (the italics are the Homœopathic Reviewer's) of Bayle's article on the subject, the English reader is referred to Dr Black's 'Principles and Practice of Homœopathy.'" The "whole" of Bayle's article! Why, Dr Black does not give the half of it, not even a third of it!! He is either ignorant of, or ignores the most important part of it. "English reader!" surely these words are calculated to imply, that on the occasion in question, Dr Henderson had been a French reader? As untruthful as the foregoing is the attempt to rescue Dr Henderson from a charge of gross inaccuracy, by pretending that "Bayle's compilation" is so well known, that it may be almost regarded as "common property." Our readers know better, and we happen to know that however familiar Bayle's name and observations may be, there is only one available copy of his "Compilation," for consultation, in our northern metropolis. In endeavouring to wipe out one of Dr Henderson's errors, the "Homœopathic Reviewer" has disclosed others of equal, if not greater magnitude. "Incidit in Scyllam qui vult vitare Charybdim."

the intricacies of this labyrinth of errors, is led to suppose that he has before him an immense array of solid results, from which the errors have been sifted and winnowed by a threefold process of analysis; the fact being, on the other hand, that the truth and error submitted ultimately to his judgment, stand to each other in nearly the same relation as Falstaff's "pennyworth of bread" to his "monstrous quantity of sack," or (shall we say) in the same proportion as the active (?) ingredients in a globule of arnica (30) to the saccharum lactis. In other words, Dr Henderson, affecting to quote Bayle, treats us to some homœopathic pickings out of Bayle by Dr Black; the "Homœopathic Journal" being, of course, quite clear that this proceeding is all right; in fact, that Black is white, and Henderson too. The proceeding of Dr Black is as follows: he first pumps away at honest Bayle till he has pretty nearly exhausted all the colouring matter dissolved in the Frenchman's work, and then leaves behind the dry facts, as a *caput mortuum* for those who will take the trouble to analyse them. Honest Bayle, again, labours in his vocation to clarify his muddy German sources as only a Frenchman would ever think of doing; and the straggling and sprawling heroes of "Hufeland's Journal" turn out smooth-tongued and rotund in their testimony to belladonna, just as their proper names are insensibly melted down into forms agreeable to the eye of the French printers' devils, but scarcely recognisable under a Teutonic sky. When we think of the very washy and stupid twaddle about which all this pother has arisen, we confess we begin to feel ashamed of medical literature. The story of belladonna is, by its turnings and windings, so irresistibly suggestive of the veracious and instructive narrative of the "House that Jack Built," that we trust we shall not be considered to sink below the dignity of the occasion in producing the following lines from the pen of a witty friend:—

"Here's a Professor, professing to tell
What Bayle has said, while he knows full well,
That never a line of Bayle did he see.
Thus Henderson cribs out of Black, while B.
And H. together, sooth to say,
Have treated old Bayle in a scurv'y way.

"And here's old Bayle, a blundering goose,
By Black and Henderson turned to use—
Trying to see through Germans dim,
And to turn them out *à la Français*, trim
As gay *faneurs* on a holiday,
All on the Boulevard so gay.

"And here's a noble German—so dim!
Never a Frenchman can see through him!
Sitting alone, with thought so free,
On the egg of a grand discovery,
Which came, it must be at once confessed,
Out of that very great *mare's nest*,"

The Castle in the Air that Hahnemann Built.

To be serious, we believe it impossible to present a better instance than the story of belladonna, of the manner in which errors are propagated, discreditable to the literature of our profession. Taking this tortuous history as a whole, we may picture for ourselves a genealogical tree of blunders, of which the trunk may appropriately be represented by Masius, Gumpert, Berndt, and other German physicians, of whom, without meaning anything disrespectful, we affirm, that they were loose observers; the larger branches by Monsieur Bayle, an honest, we believe, but very faulty translator, the small branches by Dr Black, and the minute twigs, by Professor Henderson, whose verdant foliage is displayed under the cover of "*Homœopathy fairly Represented*," and of whom we shall only say, that he manifests an unquestionable and vigorous vitality in assimilating and elaborating the crude juices presented to him by his predecessors. But the tree is known by its fruit; and we trust we have been successful in showing our readers the worthless, if not poisonous, character of its produce in this instance. And so, for our parts, we wash our hands of the matter.

A Manual of Botany, being an Introduction to the Study of the Structure, Physiology and Classification of Plants. By JOHN HUTTON BALFOUR, M.D., etc. Third Edition, revised and enlarged, by JOSEPH WILLIAMS, M.D., Lecturer on Botany, Original School of Medicine, Dublin. London and Glasgow: Griffin and Company. 1855.

SUCH of our readers as take any interest in botanical books are aware that, in 1849, Professor Balfour put forth an elementary work entitled "*Manual of Botany*," which was published by the Messrs Griffin of Glasgow. Somehow or other, the author and his publishers did not pull well together, and consequently when, in 1851, a new edition was called for, Dr Balfour declined having anything further to do with this manual, and betaking himself to another publisher, produced a new and much improved work under the title of "*Class Book of Botany*." It is no business of ours to inquire who was right or wrong in this controversy, suffice it to say, that the copyright of the manual remained with the Messrs Griffin, and the book now before us is a third edition thereof. Of course it is not only the right but the business of Messrs Griffin to turn their literary property to the best account. In one respect this third edition is an improvement on the second, for we have on the title-page this time the name of an actual personage acting as editor (at least, Dr Williams has specific characters assigned to him, from which we infer that he is not a myth), and therefore we know with whom we have to deal in judging of the merits of this particular edition. We fear, that in the present case we have to do with an editor who has very loose notions as to the responsibilities of his

vocation. The qualifications required for rightly editing a work are first, sufficient acquaintance with the subject to enable the editor to bring the work up to the actual state of science; and secondly, a sufficient amount of diligence and care to enable him to exclude, as far as possible, typographical errors, which are always doubly to be reprehended in books abounding in scientific and technical terms. In both these respects Dr Williams seems to us to be far from up to the mark. We fail to discover any additions to the body of the work, of those novelties which the progress of botanical science has made known to us since the appearance of the first edition of this book. Thus, at page 450, under the important subject of the Cinchonaceæ, we find the following:—

“Yellow bark China-regia, or Calisaya bark, partly flat, partly quilled, procured from Cinchona, Calisaya, which grows around Apolobamba.”

We were at first disposed to think that this involved merely an error of punctuation, arising from the same carelessness, which, in the line above, has called Cinchona micrantha C. micrantha, but that it is something more than this, is evident from the tabular view of the Cinchona barks given lower down in the same page, where we find—

“Royal, Yellow or Calisaya bark. . . . Cinchona sp.?”

The fact seems to be, that the editor has used the word Calisaya, in the first sentence quoted by us, not as the name of a species, but of a locality, for it is not printed in italics, as all the other names of species are in the same passage, and thus, in giving the tabular view below, he has merely reprinted it from the first edition, thus totally ignoring Weddell's discovery of the true species yielding the Calisaya bark—a species which has not only been, for some time, in cultivation in the botanic gardens of this country, but has actually been sent alive from this country to India, so that possibly, future generations of aguish Britons may be cured by quinine from Cinchonas grown on the slopes of the Himalayas. There thus does not seem to have been any trouble taken to keep this edition *au courant du jour*, and as small pains have been bestowed in purging it from old errors, or keeping it free from new. Errors in spelling are of constant occurrence. In page 2 molluscous is written moluscouus, in page 3 areolar, areoler; in page 8 primordial, primordeal; nor does the spelling improve as we get on, for at page 670 the amnios is said to be a “gellationous liquor.” The liquor is an *l* too long—the molluscs an *l* too short—let them have measure for measure in next edition.

With these statements before him, the reader may ask in what respect this edition is entitled to be said on the title-page to be revised and enlarged; we cannot say that we find much evidence of revision, but we at once admit enlargement to the extent of nearly 80 pages. This is chiefly in the form of a glossary, the idea of adding which probably was derived from Dr Balfour's Class Book. It is not stated that this is solely editorial, and not part of Dr Balfour's original work, but assuredly this ought to have been done, that the

merit of it may be assigned to the editor and not to Dr Balfour, who has not the slightest connection with it, and this all the more that there are very few of the items in this glossary which are not either incorrect, or unintelligible, or both, as the following example will show:—

“**ASCENDANT.** Stems united at the base are said to be ascendant. In the case of other plants, when the stems are procumbent naturally and only rise in their upper part—*Ex.* *Polygonum persicaria*. The filaments of the four long stamens of the Cruciferae are ascendant, the filaments of the two short stamens on the same are straight.”

Of the four sentences of which this passage consists, the first and fourth are simple nonsense; the second is not English, and is unintelligible; the third contains only two and a half words, and one of these misprinted, for *persicaria* ought to have a capital P.

The glossary is not the sole source of the enlargement of the present edition; there are some hints for botanical excursions and a philosophical peroration, which are so curious that we cannot resist the pleasure of quoting some passages from them—

“Anything of a more immediate personal nature must be contained in a small portable carpet bag, which must, at the same time, contain a small tin flagon of alcohol, and other articles necessary for the gathering, preparation, and study of the plants.” P. 666.

We have botanised a little in our day, and have managed to scrape together a tolerably large herbarium, but have never found alcohol necessary either for gathering, preparing, or studying, plants. We learn from certain instructions on a preceding page, that if any “of the specimens disclose moisture, all the parts must be touched lightly with a hair pencil dipped in alcohol.” But, setting aside for the present all questioning as to whether this treatment would or would not remove moisture, it is applied by our editor only to plants already in the herbarium—which, by the way, he calls a “local,” and says ought to be well aerated—as if a herbarium were a bottle of soda water—but let that pass. It is quite clear that this application of the alcohol can hardly be intended to be made during an ordinary botanical excursion, and with recently gathered plants. We can only, therefore, imagine the other alternative, that by the flagon of alcohol our editor means a flask of potheen for internal use—not *spirit. rectific.* for external application to the plants. If so, we submit that Britannia metal is better than tin, unless, to be sure, the botanist be rich and can afford silver: and farther, than for such a purpose he had better have the flask or flagon in one of the receptacles of his jacket, than in his carpet bag. But our advice to the young botanist rather is—away with all Dutch, or Prussian, or Russian courage—he is a poor botanist who could not get to the top of Saddleback, or Macgillcuddy Reeks, or even noble Ben-na-muighdui, without the aid of alcohol, and therefore we protest against this fluid being necessary either for gathering, preparing, or studying, plants. To do the editor justice, however, he seems to

have eventually come to the conclusion that a milder liquid than alcohol would be a safer companion during a botanical ramble, for waxing classical in the peroration which we have mentioned, he gives us the following parting words—"Ergo vale, lector amice: sylvas ruraque *lacte* peragra, et scientiam amabilem auge." Probably, however, the word which we have italicised is only a careless misprint for *laete*, and our satisfaction at finding that the editor has become a convert to abstinence practices, may, after all, be only an example of misplaced confidence.

We have mentioned a certain philosophical peroration—lest any of our readers should pine in ignorance, we transcribe a portion of it—

"Cui Bono? Conclusion.

"How to reply to this question so often repeated by persons strangers to the studies and pleasures of the botanist? How will you be enabled to estimate the satisfaction which the mind experiences in the research and discovery of a natural law, and in the contemplation of the wonders of nature? Will you speak of the inexhaustible and veritable happiness which to the naturalist replaces ordinary pleasures, and of that real beauty which he knows well to discriminate from conventional beauty? Of the sentiment of admiration with which he views the elegant bearing of the most common weed of our fields; the most humble *convolvulus* for example, encircling, with its spiral blossoms, the flexible stem of a grass, which he prefers to the comparatively best fabrics or articles decorated, as they generally are, with representations of the vegetable world, so incorrect and often ridiculous? Of the pearls and diamonds of the rose which sparkle on the leaves under the influence of the sun's first rays, and of which the effect appears to him in no wise inferior to the lustre of the most precious gems? The interrogator little affected by the beauty of plants, and who has never thought of them in any other point of view than that of alimentary consumption, would look at you with astonishment, and the more benevolent be scarcely able to repress a smile." P. 667.

We would fain quote more of this fine writing, but we have been seized with a feeling of horror at the idea of the existence of such a graminivorous monster as could look upon a *convolvulus* with a view to its "alimentary consumption," and are compelled to lay down our pen—merely in conclusion reminding our readers that neither the state of science nor the style of writing English in Edinburgh, are to be in any way identified with this edition of Dr Balfour's Manual.

Part Third.

PERISCOPE.

MIDWIFERY.

CASES OF CÆSAREAN SECTION.

Two cases of Cæsarean section, worthy of attention, from their different characters, have been performed during the month of January last, in the Hôpital des Cliniques, at Paris, by Professor Paul Dubois.

One of these operations, necessitated by extreme rachitic deformity of the pelvis, presents nothing, either in its performance, or in the previous history of the case, worthy of its being recorded, were it not that it may be added to the statistics of this operation, defective as these are by the inexcusable, although natural tendency to publish successful cases, and to conceal those which are less fortunate in their results. These incorrect data cannot be regarded too cautiously, as affording such erroneous presumptions of success.

Obs. I.—The following is a summary of its history, and of the chief peculiarities which merit attention. The patient, æt. thirty-two, and arrived at the full term of her first pregnancy, was of extremely small stature (one metre sixteen centimetres), the inferior extremities being proportionally more reduced than the rest of the body. This small size of the limbs arose less from their deformity than from their arrested development, and constituted a remarkable example of rachitis, in which this arrestment in the growth of the bones predominated over their ramollissement. The sacro-pubic diameter, measured with the finger, about six centimetres, but in reality, was not more than five centimetres and a-half. This appreciable difference arose from the unusual thickness of the symphysis pubis, which was ossified throughout its whole extent, and projected into the pelvis. The transverse diameter was twelve, and the oblique eleven, centimetres. This unfortunate creature, in whom pregnancy had been exempt from the usual accidents, presented a remarkable cedema of the lower extremities, accompanied with albuminous urine, which occasioned some apprehension of an attack of convulsions, and illustrated the greater tendency of rachitic patients to this condition, which was long ago pointed out by M. Dubois. On the 25th of January, at the time of her admission to the hospital, labour had commenced. The next morning the os was perceptibly dilated; in the afternoon, the membranes were ruptured, and from that time the pains became stronger. Not having a choice left between the Cæsarean section and cephalotripsy, which was rendered impracticable by the excessive narrowing of the pelvis, M. Dubois resolved upon operating immediately. Chloroform was administered, and the patient underwent the operation in the usual manner, apparently unconscious of pain, although the anæsthesia was not complete, as the patient manifested both agitation and excitement during the operation. The child, as had been expected from the sound of the heart's action previous to the operation, was alive; a fortunate result, all the more satisfactory, as the reverse was feared. In place of sutures, strong "serrefines" were employed for maintaining the edges of the wound in apposition. These clamps, which in a similar instance, had appeared serviceable, failed here. The next morning it was observed that several of them had become detached during the night; the edges of the incision had separated, and a fold of intestine was protruding through the opened wound. The hernia thus formed, was distended with air, and required to be punctured previously to its reduction, which operation being performed, the lips of the wound were this time kept together by the twisted suture. The first night was a good one; some sleep was obtained; in the afternoon, however, the patient was seized with retching and a cough, which were soon followed by fatal syncope. Upon *post mortem* examination, no traces of inflammation were found, not even injection of the peritoneum, nor any appearance of internal hæmorrhage, with the exception of a single small clot in one of the iliac fossæ, although the profuse discharge of blood, which occurred upon opening the uterine walls, and extracting the placenta, led to a suspicion that it had been otherwise.—(*Gaz. des Hôpitaux*, February 6, 1855).

In remarking on the case, the professor recalled the opinions which had prevailed relative to the circumstances demanding the performance of Cæsarean section. Until a very recent period, the established rule was, that in all cases where the pelvis was so contracted as to preclude the possibility of labour being terminated, either naturally, or by measures consistent with the safety

of the child, recourse ought to be had to an operation on the mother, having for its object the enlargement of the natural passages, or the formation of an artificial outlet for the escape of the infant; in other words, symphyotomy, or the Cæsarean operation, required to be practised. The fate of the infant was considered of much more importance than that of the mother. There, moreover, existed the illusion with regard to her chance of safety, that, according to statistics which were not subjected to a sufficient scrutiny, being supposed to be as one in three or four of the patients undergoing Cæsarean section. M. Dubois recollected that at the time of defending his "*Thèse de Concourse*," on this subject, he was under the influence of the prevailing opinion; since that time, however, experience had fully convinced him of his mistake. We had now to choose between cephalotripsy and Cæsarean section, that is, between an operation certain to destroy the child, but affording great chances of saving the mother, and an operation very likely to be fatal to the mother, without the certainty of saving the child. But, putting the question in another form, he asks, Is it justifiable to compare a life bound by so many social relations, with one not yet born, consequently without such ties, and whose existence is subject to so many risks? Considered thus, the question appears to him as beyond dispute, and he states, that wherever such difficulties presented themselves, he would have no hesitation, no scruples of conscience, in seeking the safety of the mother by sacrificing the child. He considers it all the more necessary to insist on these principles, owing to a discussion which has recently taken place on this subject, in the Academy of Medicine, in consequence of which, opinions of an opposite nature have been promulgated, and warmly supported beyond Paris, in the departments, and also in other countries, especially Germany and Belgium.

Among the ideas recently advanced on the subject of the Cæsarean section, when necessitated by deformity of the pelvis, one, which has been well received by some practitioners, is deserving of notice. Struck by the universally unfortunate result, up to the present time, of this operation, as performed in the Parisian hospitals, and with the success attending it in country practice, it has been considered desirable to remove from hospital those women upon whom it has been decided to operate, and to place them in circumstances offering a chance of success which could not otherwise be expected. This idea, praiseworthy so far, is one very difficult to carry out with success. Unsuitable in those cases which only present themselves at an hospital when labour has already commenced, and almost impracticable in other cases, on account of administrative regulations, it is, in all probability, destined to remain long merely as a desirable proposition. But it is not the less important that it should be supported, and that every practitioner should endeavour to retain these unhappy patients in their own houses, in place of removing them, as is commonly done, to an hospital, in search of that assistance which, in such circumstances, is almost invariably fatal.

The second instance is more curious, inasmuch as it affords an example of the Cæsarean operation, rendered necessary by obstacles which require to be overcome in a less dangerous manner, and which have never, until now, demanded the adoption of such extreme measures. It exhibits, in fact, as a cause of "dystocie," the simultaneous occurrence of induration of the neck of the uterus, and an emphysematous condition of the fœtus—conditions capable of relief by multiplied incisions in the neck of the uterus, by puncturing the cranium of the child and its visceral cavities, and, in the event of these means proving insufficient, by the employment of the cephalotribe. The different features of the case—the ability of the operator, and his well-known aversion to the practice of Cæsarean section, the death of the fœtus—all lead to the conviction that, in this case, it was absolutely impossible to terminate the accouchement naturally, and dangerous to carry further, than had been done, the incisions in the neck of the uterus.

Obs. II.—A woman, *æt.* 30, pregnant for the first time, was brought, on the 20th November, to the hospital by a country practitioner, who suspected a difficult accouchement, as the patient had, during infancy, sustained a depressed fracture of the pelvis. Labour commenced at the full period, on the 7th January. During the night of the 7th the membranes ruptured, and an enormous quantity of fluid escaped. On the 9th this discharge became fetid, and assumed a greenish colour, due to the presence of meconium. From this date the motions of the child could not be felt. Upon examination, it appeared that the conformation of the pelvis would not offer any obstacle to natural labour. It was not there, in fact, that the obstacle existed. Examination afforded, at first, only vague information; but, after several repetitions, it was finally discovered that the os uteri, placed at the bottom of a sinuous cavity, was small, directed backwards, and to the left, and surrounded by a ring of aponeurotic consistence. Upon introducing the finger deeply into this opening, the head of the child could be felt. On the 12th M. Dubois made, with a slightly curved and probe-pointed bistoury, four incisions at opposite points of the os. Two of these incisions were very free; and, if the diaphragm upon which they were made had been less hard and less thick, they would have enlarged, as commonly happens. Here, however, there only resulted an opening scarcely large enough to admit two fingers. On the 13th, no change having taken place, M. Dubois resolved upon perforation of the cranium, which was not fractured without difficulty, owing to the tightness of the os, and the elevation of the parts upon which the instrument had to act; and it was only by the aid of assistants, pushing the uterus downwards, that he succeeded. At the moment of withdrawing the perforation, a quantity of fetid gas escaped by the vagina, filling the ward with the most insufferable odour. M. Dubois considered that this proceeded from the cranium, and that it was, in all probability, likewise developed in the thorax, abdomen, and, perhaps, even in the cellular tissue of the infant.

The condition of this woman becoming more and more perilous, from increasing weakness, conjoined to a feverish state, and the contraction of the os and the inextensibility of the parts being apparently insurmountable, M. Dubois decided, as a last resource, to practise the Cæsarean section. Notwithstanding her feeble state, the patient was placed under the influence of chloroform, with the intention of merely diminishing sensibility—not of altogether suspending it; but the inhalation was so well borne, that it was prolonged until the anæsthesia became complete. The different steps of the operation were conducted according to the ordinary method, and with the usual precautions, without any particulars worthy of remark. The lips of the wound were brought together by strong “*serre-fines*,” assisted in their action by strips of adhesive plaster placed in the spaces between them, and by a bandage. The “*serre-fines*” were used only in the two upper thirds of the wound, the inferior third being left open for the free escape of any fluids. The loss of blood, which is generally considerable in these cases, was here so little, as to be of no moment whatever. As to the operation itself, it was of short duration, and well supported. The patient was placed in a private room, and the day passed in a satisfactory manner; but, towards evening, symptoms of agitation supervened, which increased during the night, and the next day, the 14th, the patient died from sinking, apparently produced by putrid infection—as the powerful odour, attributed to putrefaction of the fœtus, persisted after its extraction. The fœtus was of a monstrous size, and exhibited, in a high degree, the characters of the emphysema, suspected and ascertained to exist previous to the operation. At the post-mortem examination of the body, the uterus was found gangrenous throughout the internal half of its thickness. The pelvis presented a remarkable appearance: the horizontal branch of the os pubis had been fractured, during the first years of life, at a short distance from the symphysis; but direct reunion had not taken place. The fractured

ends of the bone had been united by an intermediate ligamentous tissue, of several lines in thickness. There resulted from this a remarkable mobility; and yet, although the patient had been so up to the age of twelve years, she was not lame after that period. It is probable, also, that, in consequence of this accident, there had been an arrest of development of the bone, as the bi-sciatic diameter measured, in place of 11, not more than $8\frac{1}{2}$ centimetres. At the same time, this circumstance was not sufficient to warrant the performance of Cæsarian section; as, excepting a slight narrowness of the pelvis, the patient was well-formed.—*Reported by Jacquemier in the Gaz. Hebdomadaire, April 1855.*

EXTRA-UTERINE PREGNANCY TREATED BY GASTROTOMY.

M. Rousseau, surgeon-in-chief of the hospital at Epernay, read a communication on this subject before the Academy of Medicine, at their meeting of April 17th.

During the first months of pregnancy the patient had been subject to occasional pains in the left side of the abdomen; sometime afterwards, strangury occurred and lasted for twenty-four hours. The catamenia had been suppressed. Nine months after the supposed epoch of conception, the movements of the fœtus, which in a previous pregnancy had been much earlier and more distinctly felt, became altogether imperceptible, and shortly thereafter a considerable discharge of blood took place from the vagina. The secretion of milk, which commenced simultaneously with the cessation of fœtal movements, had been for fifteen days somewhat augmented. At this period, in consequence of impairment of the general health, and the occurrence of indigestion, slight fever and emaciation, the patient was admitted into the Hospital of Epernay, October 31st 1852. In November, six successive cauterisations were made by means of the *cautére cutellaire*, over the left iliac region, where the head was perceived to lie. On the 6th December, the parts which remained to be divided in order to reach the amniotic cavity, were incised with a bistoury; the head of the fœtus was opened, and the arch of the cranium and the brain removed; the placenta and membranes were left in situ. The operation was performed without opening the peritoneum, and no peritonitis occurred, but inflammation in the veins in both arms took place. Emollient injections into the amniotic cavity were at first employed, and these were succeeded by solutions of the chlorurets. Sulphate of quinine was exhibited. The inflammation of the veins of the arms terminated in recovery, and the size of the amniotic cavity decreased daily. The placenta maintained its vitality, and partook of the cicatrization which proceeded rapidly. The external wound diminished and was very soon reduced to a small fistulous opening.

The author concluded by observing that in all cases of gastrotomy practised in consequence of extra-uterine pregnancy, wherever it was found that the placenta and membranes were not detached, they should not be removed.—*Gazette Med. April 21, 1855.*

ON THE INFLUENCE OF COMPRESSION BY THE MATERNAL SOFT PARTS DURING NATURAL LABOUR.

It occasionally happens in those accouchements where the employment of the forceps has been found necessary, that the infants born are attacked, immediately or a short time afterwards, by symptoms of congestion or compression of the brain. Fatal consequences frequently follow, and upon *post mortem* examination, there are found considerable sanguineous effusion on the surface of the brain, injection of the choroid plexus, etc.; in short, lesions indicative of the injurious pressure to which the head had been exposed during labour.

M. Charrier details three such cases, the only difference being that the labour was natural and of ordinary duration; but, from the anatomical pecu-

crities presented by the infants, he considers both orders of cases as referable to the same immediate cause, viz., pressure on the brain.

In the first of these instances, the mother was healthy and the pelvis well formed; delivery was completed in six hours, and the child appeared strong and vigorous. Some days afterwards, however, cerebral symptoms began to show themselves, and on the third day from their commencement, coma and death took place. Upon *post mortem* examination, the pia mater was found considerably injected; the sinuses were gorged with blood of a dark colour and purpy consistence; the choroid plexus was much injected; bloody serum was found in the ventricles; there was effusion of blood at the base of the brain, and the substance of both the brain and cerebellum was of a rosaceous colour. The other viscera were healthy. In removing the calvarium, the bone was found of such extreme tenuity as to be quite translucent—it could be depressed by the finger without breaking, and it regained its original form by its own elasticity.

The absence of any violence or protracted duration in the accouchement; the absence of any apparent hereditary predisposition to cerebral disease; and the fact of the mother being well-formed, all led him to the conclusion, that the death of the child resulted from the thinness and depressibility of the bones of the cranium, allowing of undue pressure on its contents during labour.

The second case was one where the infant became comatose on the second day after birth. Respiration slow, 24 inspirations in the minute: pulse small, and 115. The symptoms became more and more aggravated until the eighth day after their commencement, when death took place, proceeded by slight convulsions. At the autopsy there were observed: general congestion of the surface of the brain: serous effusions between the hemispheres; and effusion of blood at the base. The other viscera were healthy. The cranial bones were not so thin in this as in the first case, but the fontanelles were very large, and the sutures open. The diameter of the head was normal.

In the third case, the infant was seized on the sixth day with symptoms of a cerebral nature, and on the eighth a fatal termination ensued. The appearances disclosed upon *post mortem* examination were similar to those of the second case, and, as in it, the fontanelles were here of unusual size, and the sutures separated.

It is only necessary to repeat, says M. Charrier, what has been before remarked, that the compression of the skull and brain by the uterus and maternal soft parts during labour, was the cause of the symptoms observed in these cases during life, and the appearances presented upon *post mortem* examination. The lesions discovered in the three subjects examined were precisely such as are exhibited by those cases where death results from the application of the forceps, and appear quite sufficient to explain the symptoms manifesting themselves during life.

In a report upon this paper by M. Danyau, and read to the "Société de Chirurgie," it is considered doubtful whether imperfect ossification of the cranial bones can exclusively account for the production of those effects attributed by M. Charrier to this cause. M. Danyau is of opinion that other circumstances should have been taken into account, as, for example, the condition of the vascular system within the cranium; a predisposition to congestion; liability to rupture of the vessels, etc. In the absence of other indications, however, he considers death, in such cases, to be justifiably assignable to the causes laid down by M. Charrier.

Following up the inquiry on this subject, a communication has since appeared by M. Ancelon, in which he suggests that incomplete ossification of the skull not only conduces directly to pressure on the brain, but from the maternal passages in consequence, being imperfectly opened, the liver of the infant suffers by compression, arising indirectly from the same cause. He believes, moreover, that, in those cases of imperfect ossification, there generally exists,

along with it, some constitutional taint in both the mother and child; and he concludes his paper by remarking:—

1st, That owing to some peculiar diathesis the process of ossification in the foetus becomes retarded, and the power of resisting pressure during labour consequently diminished.

2d, During labour the maternal soft parts exercise a degree of pressure on the liver, proportionately greater as the ossification of the head is less complete.

3d, The diathesis alluded to, in conjunction with contusion of the liver, explains the fatal result in M. Charrier's cases.—*Gazette des Hôp.*, 24th April and 5th May 1855.

INCISION OF THE VULVÆ FOR THE PREVENTION OF RUPTURE OF THE PERINEUM.
BY M. CARPENTIER.

Two cases are described by this author, which, if they do not show the necessity of this proceeding, at least point out its efficacy as an operation. It was recommended by Michaëlis in 1810, since which time it has been practised by Weire, Eichelberg and other accoucheurs, and does not deserve the neglect into which it has fallen at the present day.

The first case related is that of a primipara where the labour had been protracted, the head of the child having appeared at the vulvæ, remained there, notwithstanding the strength of the pains, for nearly two hours. M. Carpentier then made an incision, not in the raphé, but laterally in the vulva, and this operation was scarcely completed, when a vigorous pain terminated the labour.

The other case was one requiring the employment of the forceps, which were applied without difficulty; but upon the head appearing at the vulva, the operator saw that extensive laceration of the perineum was threatened. He therefore confided the forceps to an assistant, who maintained them in position, while an incision in the vulva was made during a pain. The child was then extracted without any resistance.

It is well known that laceration of the fourchette is very common in first labours, and that in spite of all the precautions taken in supporting the perineum, it is ruptured in some cases as far as the rectum. Would then, asks M. Carpentier, incision of the vulva prevent such accidents, by substituting a surgical operation for a rent made by nature fortuitously and with violence? Insufficient statistics render any conclusion on this question difficult as yet; experience is required to show its true value as an operation.—*Revue Medico-Chirurgicale*, 1854.

ON THE CONDITION OF THE NECK OF THE UTERUS DURING PREGNANCY.

Such is the title of a memoir read by M. Cazeau before the Société de Chirurgie, and upon which M. Laborie has drawn up a report, with an interest and labour seldom bestowed upon this task, in the learned societies of the continent.

The neck of the uterus, in the pregnant female, has often been subjected to examination, as far as that can be made by touch alone; but it is only of recent date that another method of investigation, in this respect, has been afforded, in the employment of the speculum vaginæ, and it is the results obtained from researches by means of this instrument, that M. Cazeau describes. According to these observations, he states that, in primiparæ as well as in multiparæ, the vaginal portion of the neck of the uterus is of a dark red colour (lié de vin), and, in primiparæ, its whole surface presents a smooth appearance. The os uteri, of which the lips are very soft, is in general more or less rounded, and of a larger size than in the unimpregnated condition. The free portion of the neck in a very few cases exhibits ulcerations, and more frequently granulations of a cherry red colour, and bleeding easily. Among the multiparæ the neck is larger; the opening is divided into different portions; it is large, and the interior of it admits of being examined. The walls of this

part of the organ are irregular, and present a number of fungous elevations which bleed readily, in the hollows between which are occasionally observed ulcerations of a linear shape, and more or less deep. M. Cazeau regards these ulcerations as of little importance, and disapproves of any treatment for their removal.

MM. Boys de Loury, Costillies, and Coffin, consider the ulcerations of the first period of pregnancy, as having a marked influence in the production of abortions.

Admitting so far this opinion, M. Cazeau rejects it in its application to the ulcerations of the last months of utero-gestation; and, according to this view, he proposes that no treatment should be adopted in the latter cases. Thus, with the exception of specific ulcers, M. Cazeau considers that the others should not be interfered with, and, unless an excessive tendency to spread were manifested, no local means of treatment should be employed.

We give a resumé of the interesting discussion in the Society.

M. GOSSELIN does not consider ulceration of the neck of the pregnant uterus so frequent as M. Cazeau; nor does he think that it exerts any unfavourable influence on the progress of gestation. If such an opinion has been advanced, it probably arose from exclusively attending to ulcerations, and overlooking other morbid conditions existing during pregnancy, and which are the true causes of abortion: such are uterine catarrh, in which the uterus suffers as much as in the evolution of a foetus; leucorrhœa; or even constitutional syphilis. There may be added to these the repetition of examinations with the speculum, or the injudicious employment of internal remedies. In short, M. Gosselin believes that the influence of ulcerations of the neck of the uterus in the production of abortion, is very doubtful.

M. DANYAU has investigated, at the Maternity Hospital, the condition of the neck of the uterus in 22 pregnant women. Of that number, 8 presented erosions on the surface, and a fungous condition of the neck; in 6 others the erosions occupied the os tincæ; the remaining 8 exhibited no ulcerations whatever, and the neck of the uterus was merely irregular in shape and of a violet colour. These last cases had reached the final period of utero-gestation. The erosions were generally accompanied by a uterine catarrh. Only two presented this uterine catarrh without any erosion.

M. CLOQUET considers these ulcerations of the neck of the uterus as more frequent in the last month of pregnancy. He does not, however, regard them as true ulcers, but as an enlargement (*boursoufflement*) of the follicles and papillæ of the neck, or as a congested state of these papillæ, with abrasion of the epithelium. He compares them to the strawberry-like surface of certain hemorrhoidal tumours, bleeding like these upon the slightest touch, and secreting a mucous or a muco-purulent fluid. M. Cloquet has never considered it necessary to cauterise them, or to employ any other means than astringent injections in their treatment, when they occasion either discharge or hemorrhage. After delivery these vegetations disappear spontaneously, in the same manner as the hemorrhoids and varicose veins connected with pregnancy.

M. VOILLEMIER has found no ulcerations in a large number of women, arrived at the eighth month of pregnancy, who had been examined by him with the speculum.

M. GERDY has observed soft fungous erosions on the neck of the uterus in pregnant women; these erosions disappeared after the accouchement. He considers that the engorged state of the veins from the sinking of the uterus, accounts for the violaceous colour of the vulva, the vagina, and the neck, and recommends the horizontal posture, with the view of relieving this engorgement.

M. HUGUIER believes that four different affections of the uterus require to be considered separately; these are, catarrh, granulations, ulcerations, and the fungous condition of the neck.

1st, Catarrh. This occurs in nearly all women when advanced as far as the

last month of pregnancy, and, according to M. Huguier, proceeds from an increase, with or without inflammation in the mucous follicles, of the gelatiniform secretion, natural at this period.

2d, Granulations. He divides these into three varieties, one formed by an hypertrophied state of the papillæ and mucous membrane, and co-existing with granular vaginitis. Another consists of true vegetations, and co-exists with analogous vegetations on the vaginal walls or vulva. A third kind are situated round the os, and consist in an hypertrophy of the mucous follicles, or an accumulation of mucus within their cavity.

3d, Ulcerations. These he considers rare during pregnancy, but simulated by other states of the os and neck, from the muco-purulent fluid often lodging about it at this period.

4th, Fungous state of neck. This condition M. Huguier alleges to be the consequence of, and, in fact, inherent to pregnancy, and to consist in an inflamed, softened, and swollen state of the parts in the neighbourhood of the os tincæ. He considers these conditions as of trivial importance, and employs means of the most simple nature for their treatment.—*Union Med.*, April 21, 1855.

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXXIV. 1854-55.

February 4th, 1855.—WILLIAM SELLER, M.D., President, in the Chair.

ON LOCAL BLOOD-LETTING IN CHRONIC METRITIS. BY J. MATTHEWS DUNCAN, M.D.

(*This paper was inserted in the last Number.*)

IN the conversation which ensued, several members thought that Dr Duncan had overstated the amount of blood drawn by a single leech (one ounce), and the average was considered to be two to three drachms. Professor Miller, however, remarked that everything depended on the locality; that whereas externally only two drachms might be obtained, on an internal mucous surface, one ounce was a fair average, and more was likely to follow by oozing.

The President remarked that the old opinion was that the usual quantity was two ounces from an external surface, and parts favourable to their application, and he certainly thought that the smaller quantity was more frequently obtained than the larger.

Dr M. Duncan, in answer to Dr A. Wood, said that leech-tubes were still much in use in London and Edinburgh. With regard to the quantity of blood furnished by leeches he had merely mentioned the ordinary calculation, and did not speak from special observation.

Mr Struthers entirely agreed with the principles laid down in Dr Duncan's paper. Besides, there was no small risk of leeches opening some of the large veins in the vagina which possessed no valves, and would afford a large flow of blood. In his own observations on local blood-letting, he had no intention (as some thought) of making an alteration in our practice. His inquiry had been strictly an anatomical one, but its result went to confirm the established mode of treatment.

The President was inclined to believe that the application of leeches might exert a counter-irritant as well as a derivative action, and in his opinion no

theoretical view should influence our treatment of disease, unless the practical men also gave in their adherence to it.

ON THE DIFFERENTIAL DIAGNOSIS OF PNEUMONIA AND PLEURISY. BY W. T. GAIRDNER, M.D.

Dr Gairdner said that he proposed to consider how far we possessed the means of accurately distinguishing those acute diseases of the chest which were at present described as separate nosological forms of disease. The inquiry was one of great interest; pneumonia, for example, had for years been regarded as the acute disease by which we tested our systems of treatment. It was important, therefore, to know whether we could so mark it off as to be able, with sufficient accuracy, to make numerical statements as regards its cure by different remedial agents. Again, pneumonia was the disease on which homœopathy and other late systems of treatment rested their claims, and in the treatment of which they boasted their great success. Up to the eighteenth century, we found pleurisy occupying the same relation to medical literature as pneumonia does now. Did this difference of names depend on a change in the nosological type of diseases, or was it not rather a change in the ideas of the observers, who applied different names to the same disease? He (*Dr Gairdner*) believed the latter to be the true explanation. A fallacy, too, of common occurrence was, that when we have ascertained a pneumonia to be present, a series of such cases can be submitted to treatment, and the results tabulated on the supposition that the cases are similar. Now, in his opinion, pneumonia in all cases was not such a clearly definable disease, and, even if we could distinguish it, we would not be any nearer the solution of the question, as it is a disease which varies infinitely, and requires for each case a separate adaptation of treatment. That pneumonia presents, even to well-informed and honest physicians, unusual latitude for diagnostic variations, *Dr Gairdner* illustrated by a copious enumeration of the various chances of error which must be taken into account; among which, special reference was made to pulmonary collapse, cases of which, from the identity of the physical signs, might be easily classed as pneumonias; and to bronchitis, in the course of which pneumonia so frequently supervened and escaped detection. The differential diagnosis of the two diseases was dwelt upon, and it was shown that in many cases the practitioner was necessarily foiled in his differential diagnosis. Should again there be an arbitrary exclusion of cases where the two diseases were complicated, we should have a very large proportion of the most dangerous and fatal forms of pneumonia got rid of, and the value of our tables so far vitiated. Again, one man might include and another exclude cases of pneumonia modified by some peculiar constitutional tendency, as fevers of specific type, Bright's disease, syphilis and gonorrhœa, delirium tremens, diabetes and tubercle.

In the pneumonia of the old authors, we had cases of pleurisy included; and *Laennec* considered the cases of pneumonia which he tabulated as more favourable for treatment, as by his improved methods of diagnosis he was able to separate the pleurisies from the pneumonias. And yet, of late years, the opinion had gained ground that the statistics of pneumonia would be improved by the addition of a few cases of pleurisy, which was a less fatal disease. The differential diagnosis of the two diseases, pneumonia and pleurisy, was criticised in detail; in the cases where the two affections are combined, *Dr Gairdner* believed it to be extremely difficult to indicate in a satisfactory manner to what extent the pneumonic and to what extent the pleuritic elements respectively were present. From an analysis of 41 cases of pneumonia occurring in hospital practice, it appeared that in only 8 was pleurisy absent, and of these 8 cases there were 6 where there was incipient broncho-pneumonia; in one there was incipient abscess, and in one there was hæmorrhage of the lung. There was not a single case of fully formed and fatal pneumonia unaccompanied by a considerable amount of pleurisy. In 47 cases of pleurisy and fibrinous dropsy, there were only four cases of uncomplicated pleurisy. It resulted from these

considerations, that acute uncomplicated pleurisy and pneumonia occurred as seldom that any attempt to separate them in diagnosis must fail, as the elements for such a distinction did not exist in practice.

The *President* said he was glad to take this opportunity of stating his entire concurrence in the views of Dr Gairdner on collapse of the lung; this he did the more willingly, as in a paper which he (the *President*) had had the honour of reading to the Society, he had opposed the doctrines of Dr Gairdner on the subject. From Dr Gairdner's theory of emphysema he was still, however, compelled to enter his dissent.

May 2d, 1855.—WILLIAM SELLER, M.D., President, in the Chair.

CASE OF FATAL HÆMORRHAGE, AFTER AN OPENED ABSCESS OF THE NECK. BY JAMES MILLER, ESQ.

The patient was a laundry-maid, aged 41 years, and was admitted into the Royal Infirmary on the 3d of August 1854. A swelling on the right side of the neck had been first noticed about seven weeks before; and shortly thereafter Mr Miller first saw her, and found a hard indolent swelling low down in the neck, and with no impulse, which he considered to be an ordinary chronic enlarged gland. It was of firm consistence; and, some time before, supposed to be a tumour, its removal by the knife had been recommended. Mr Miller prescribed the local and internal exhibition of iodine, and sent her home. Some weeks afterwards, however, she returned; the integuments over the swelling were now red and swollen, and deep fluctuation could be made out. Formerly, the swelling lay above the clavicle, it had now extended below that bone, which almost ran midway through it. The danger of the case was fully recognised and explained to the patient. On admission, she felt weak and fatigued, complained of her breathing, and of a sick faint feeling. Beef-tea and wine in small quantity were ordered; and fomentations to the neck.

August 4.—She felt better. After careful examination with the stethoscope, the abscess was opened; Mr Miller at the time explaining, to the clerk and pupils, the risk of secondary hæmorrhage, that was inevitably incurred, by subsequent ulceration. The pus evacuated was considerable in quantity, and healthy in character. She felt relieved by the incision. Beef-tea was continued on account of difficulty in swallowing. 8 p.m.—The patient complained of pain in the abscess. There was no discharge. She was still weak and faint. She spat a good deal, but no blood. The abscess appeared to have shifted its position towards the mesial line.

August 5.—Noon. She had slept ill. The abscess was tense. By means of a probe, a quantity of foetid pus was discharged. Lint was placed in the wound, and the poultice continued. 8 p.m.—She felt easier. There was little discharge. She had spat a good deal. Nothing remarkable was observed in the sputa; there was no blood. The power of swallowing had returned.

August 6, 1.30 p.m.—She was easier, and looked better. There was healthy free discharge from the wound. At 5 p.m. (third day since the abscess was opened), she was seized with a fit of spitting of blood. Vomiting of blood ensued, and at the same time it flowed from the nose and from the wound. Her danger was imminent: she had cold sweats, involuntary discharge of feces; and her pulse was very low. Wine was administered, the poultice was removed, and lint alone applied. 8.30 p.m.—There was no more hæmorrhage. Her sputa was slightly tinged. The pulse was very low. She complained of a feeling of pain in the right arm. The wound was again closed. At 9.30 p.m. another fit of vomiting came on. Clots of blood, apparently from the stomach, were expelled; and she died, partly of weakness and partly of suffocation.

The dissection, which was hurried, revealed a healthy condition of the various cavities. A large abscess had formed on the anterior aspect of the common carotid, and had made its way down to the level of the vessels. It had opened into the common carotid, about its middle. There was also a smaller abscess

behind the vessels, which had opened into the trachea and œsophagus, both of which appeared slightly congested. The opening into the artery was plugged up with a clot of blood. The preparation, which had been carefully dissected, was handed round.

The author said, that there was no dispute as to the fact of opened abscesses subsequently communicating with arteries; the only doubt was, whether an abscess which has not been opened could exert that ulcerative power. He himself believed that both events occurred, but the latter much more rarely than the former. The history just read lent support to either view. We had two abscesses in close apposition. The larger he had himself opened; it communicated through a small recent-looking aperture with the smaller abscess, which in its turn was found to have discharged itself into the trachea, œsophagus, and the artery. Now, when were these three apertures made? Subsequently to the external incision, or had they existed for some time previous, producing the spitting and dysphagia complained of? That question he found it very difficult to answer. But, supposing that the involvement of the artery resulted from the opening of the more external abscess, the incision he (Mr Miller) made must still be regarded as the correct practice. The abscess was then so acute that it must speedily have evacuated itself, and thus a delay of a day or so might have been incurred, during which time ulceration would have extended the space of the abscess, exposing the vascular tissues to greater and and greater risk. It might be asked, why make a direct opening, and not rather draw the matter away from time to time by the Abernethian method? The answer was, that such treatment is never dreamt of, or is altogether inapplicable in the case of acute abscesses. He certainly was of opinion that delay would have been attended with increased danger to the artery, which had a chance of being avoided by the prompt incision. As to the after treatment in such cases, a doubt might arise whether it was to be strictly antiphlogistic, or whether a moderate allowance of support was to be given, on account of the probably asthenic character of the inflammation. In the present case a middle course had been followed; and he believed that the requirements of each individual case must regulate the practice in that respect. Of course the opening, when made, must be kept free and patent, so as to obviate the confinement of the pus, which was an untoward circumstance. Unfortunately, from the rapid adhesion of the lips of the incision in this case, the discharge, during a few hours, did not get free vent.

With regard to the date of the apertures, he desiderated more precise statements as to the appearance of the sputa. The report which had been read, and which was meagre on the point, was that of his house-surgeon, who had since gone abroad. The opening into the trachea might have been early produced, and supplying the abscess with air, though by a route somewhat circuitous, might account perhaps for the artery's erosion, in the opinion of those who consider the access of atmospheric influence essential to that event: or the artery might have been opened by the abscess, while the latter yet had no communication whatever with air; and then this inner abscess—for a few moments aneurismal—bursting into the outer abscess, which was open, would produce the outward channel to the hæmorrhage. The communications with the larger abscess, with the trachea, and with the œsophagus, might have occurred simultaneously. In the first crisis, we had the blood coming from three different channels at once—wound, trachea, gullet; in the second, we appeared to have it vomited alone. Could anything have been done to check the bleeding? He believed not. He was a little surprised that the carotid had been perforated so high in the neck; his impression had been, from the situation of the abscess, that the bleeding was to be expected from the subclavian or jugular veins, or the corresponding arteries in the lower cervical region. But, as it was, had the artery been cut down upon, it would have been no easy matter to have thrown a ligature around it in the universal

matting of the parts; and the ligature would have had little chance of being securing a vessel surrounded by unhealthy ulceration. It was interesting to observe, however, that, even in these desperate circumstances, nature was almost busy with her plug, which was found, on dissection, obstructing the rigid arterial aperture. The case certainly was not decisive as to whether an external opening was always necessary for the production of false aneurisms in such cases. Liston and Breschet had recorded cases of suspected abscesses, which, on the first incision, gave evidence that arterial communication had taken place. In these cases he agreed with their authors; but it was well known that, in the minds of others, they were still open to doubt and surmise. The great lesson of the present case was, in his opinion, sufficiently clear: to open early in all cases of acute abscess, in whatever situation they might be found, and more especially in those which bore closely on the neighbourhood of vascular or other important tissues. At the same time, it must be observed that, in this case, the small abscess, which did the mischief, was so situated—immediately behind the trachea and vessels—as to be inaccessible to such treatment.

Dr W. T. Gairdner addressed himself to the interesting pathological question of abscesses opening into arteries before or after incision. It had been argued that abscesses never, or very rarely, communicate with arteries before incision. The affirmative observations were certainly few in number, and still under challenge. Authors had not, to his mind, given a very satisfactory explanation why the vessel is not so apt to give way to ulceration before as after incision. It had been generally supposed that the alteration of the pus by the air, or some other circumstance acting vitally on the coats of the artery, was the cause of the altered condition after the abscess was opened. Dr Gairdner believed that the real solution of the difficulty was, on the contrary, mechanical. An artery in contact with a closed abscess was supported on every side by equable hydrostatic pressure; and however much its coats might have been previously softened, they were therefore in the most favourable condition for withstanding the pressure of blood from within. On the abscess being opened, on the other hand, the equilibrium between the external and internal pressure was destroyed, and the weakened arterial tunics were exposed to the whole force of the circulation. The opening in the vessel in the present case, instead of being minute, and with inverted edges, as might have been expected had it been produced *ab extra* by ulceration, presented an everted appearance, and supported his (Dr Gairdner's) opinion, which was also borne out by direct experiments. Virchow and others had shown that, when the exterior of an artery is subjected to chemical irritants, the internal coats are found to have undergone no ulcerative action, but lie like a necrosed mass in the very middle of the surrounding pus. They are more amenable, therefore, to forces of a mechanical than to those of a vital character. The present case presented many points of difficulty. On closely examining the preparation, the œsophagus bore marks of ulceration on the side opposite to, as well as near, the seat of the aperture: and it was possible that the ulcerative action in the œsophagus had led to its perforation, and thus to inflammation of the neighbouring cellular tissue.

Dr John Struthers referred to the somewhat similar case in which the late Dr McKenzie had tied the subclavian artery. With regard to the sequence of events in the interesting case which had been read, he was inclined to take a different view from those advanced by Mr Miller and Dr Gairdner. He (Dr Struthers) thought it probable the smaller abscess had formed first, and forced its way into the œsophagus and trachea. Escaping externally, it led to the formation of the external swelling; and, lastly, the coats of the artery gave way after the external opening had been made; thus accounting for the blood escaping in three different directions at the same time.

Dr Andrew Wood thought that there was no evidence to show that the smaller abscess had first burst into the œsophagus and trachea: indeed, had any air entered by that route, the pus which Mr Miller evacuated would have been more or less fetid. Now, in the record of the case, it was particularly

noted, that the incision let out pus of a healthy quality. He differed from Mr Miller, however, in opinion as to the simultaneity of the apertures in the smaller abscess. He (Dr Wood) thought it more probable that they had formed at different times.

Mr Miller, in reply, could not concur in the theory advanced by Dr Struthers. When the patient was first seen, weeks before she entered the hospital, there was no difficulty of swallowing or cough complained of. The swelling, too, was distinctly lateral. As to the character of the expectoration, nothing particular was noted in the house-surgeon's report; and he (Mr Miller) believed that, if it had been purulent, that circumstance would have been mentioned. Again, the aperture of communication between the two abscesses was not only minute, but also had all the appearance of being recently formed. With regard to the œsophagus being the seat of the primary lesion, he could only state that a swelling in the neck was from the first complained of, and at a very early stage was thought a fit subject for excision. At that time there were no symptoms referable to the œsophagus or wind-pipe. The case obviously originated in chronic glandular enlargement, external to the canals and vessels.

CASE OF UNSUCCESSFUL TREATMENT OF LOOSE CARTILAGE IN THE KNEE JOINT.

BY JAMES MILLER, ESQ.

His object in bringing the unsuccessful issue of this case before the Society, was to make a recantation of a principle of practice which he had inculcated in his text-book on Surgery. Mr Miller read the extract from his work, and then gave the following particulars from the hospital case-book. The patient, a young man aged 14, was admitted March 7th, 1854, with a loose cartilage in his right knee-joint. The symptoms were characteristic, and had existed for six months. After careful preparatory treatment, two hare-lip needles were inserted over the external condyle, and the body was transfixed; not, however, without great difficulty, from the smoothness of its surface.

March 18.—The needles, having now been inserted for a week, were withdrawn to-day. They had given rise to no pain or constitutional disturbance. A little exudation was observed at their points of exit.

March 24.—The loose cartilage had slipped from its position, in consequence of the knee having been incautiously handled by a student. It was, however, again transfixed, but with only one needle.

April 14.—(Twenty-one days since the date of the last transfixion). The needle was removed. The patient got up, was in the habit of moving about the ward, and expressed a wish to return home.

May 2.—The cartilage had now been fixed for eighteen days, when this morning, on the patient pulling on his boots preparatory to his leaving the hospital, it started from its place, and again entered the cavity of the joint.

May 3.—Next day Mr Miller once more transfixed it; but, on the 5th, violent inflammation set in, resisting the most active and prompt local and general treatment; and, after a narrow swim for his life, the poor lad left the hospital with an amputated limb.

This was the first case in which he had put the principle he had advocated to the test of practice. The theory was feasible enough, and for a while, no doubt, all goes well; but to the questions, Is the operation a safe one? Is it efficient, even supposing it somewhat dangerous? a decided negative must be given; though, as had been stated, the needles were again and again introduced, and retained with perfect impunity.

Dr John Struthers said that the needle, however little annoyance it might occasion, could only be regarded as a foreign body. With regard to the difficulty experienced in retaining the loose cartilage in a fixed position, the late Dr Mackenzie had made many experiments upon the subject, and found it a matter of extreme difficulty to force the cartilage into the wound of the synovial membrane, which had been made by the ordinary subcutaneous operation. And this Dr Mackenzie accounted for by the extreme laxity of that membrane. Mr Syme's

latest plan, which had been found so successful, consisted in making a wound in the synovial membrane, and fixing the body in contact with it. This plan was found so safe and simple, that it was unnecessary to have recourse to any other.

Dr Alexander Wood was of opinion that the thanks of the Society should be awarded to Mr Miller, as a tribute to his candour in bringing this unsuccessful case before their notice. He (Dr Wood) only wished that the practice of giving unsuccessful as well as successful results was a little more common, not only among surgeons, but also among physicians. Nothing, he believed, would tend more to the establishment of true principles, and the advancement of their common profession.

UPON THE USES, MEDICAL AND SURGICAL, OF THE BARK OF THE AMERICAN SLIPPERY ELM. BY HORATIO R. STORER, M.D., OF BOSTON.

This elm, the *ulmus fulva* of Michaux, grows in great abundance throughout the northern and north-western states of America. The bark, when of good quality, is perfectly loaded with mucilage, which it readily parts with to water, and as a demulcent is much used by our trans-Atlantic neighbours in dysentery, diarrhoea, and diseases of the throat and urinary organs. As an external emollient it is also very useful, the bark or its powder being formed into a poultice with hot water. So long ago as 1837, in a paper entitled "Elm-bark Surgery," Dr Macdowall, of Virginia, had drawn the attention of the profession to the application of the bark to the manufacture of surgical instruments, as bougies, catheters, tents in fistulæ, etc., etc.; frankly, however, stating a danger attendant on the use of such instruments, when seasoned and in a dry state—namely, their liability to break from their brittleness. Such an accident, occurring in the urethra or bladder, would be very troublesome; and the fear of such occurring prevented the general use of the bark for such purposes, though Dr Macdowall's paper received due notice in the *British and Foreign Medical Review* for July 1838. This objection to its use in the formation of catheters was, in Dr Storer's opinion, fatal, and as yet had not been surmounted. Dr Storer, however, proposed to use it as a tent, either for opening up the os uteri, or for preserving the patency of the cervical canal. By bruising the bark, an abundant supply of fibrous tissue was obtained, of great flexibility and toughness, and admitting of being moulded into any shape or size. The tent, with a little glazing, was complete. Not only on the score of cheapness did it compete successfully with the sponge-tent; but, from the important property which the bark possessed, of freely parting with its mucilage, it applied a bland lubricating fluid to textures which were often from disease destitute of any such protection, and which resented the contact of such an irritating substance as sponge. Again, the expansion of these tents, though not so speedy as that of sponge, was not so annoying: it was entirely lateral, and not productive of any recoil. Specimens of the tents and of the bark were handed round.

Dr Priestley had used the tents, and could speak favourably of them. He had no reason to complain of their brittleness; they had been generally expelled as a mass of mucilage. He had stated to Dr Storer that tents of this material would be more useful in cases of mechanical dysmenorrhœa, where there was great contraction of the cervix.

Dr Matthews Duncan was inclined to think very favourably of elm-bark tents. He only feared that their power of expansion was not great; and he should like to hear from Dr Storer if he had made any experiments upon the subject. This expansion, however, was of little importance in the class of cases—viz., of mechanical dysmenorrhœa—in which Dr Priestley had suggested their employment, as a great dilating power was not required, but merely some innocuous substance, sufficiently coherent to maintain a free channel of exit, and by its presence to induce enlargement of the canal by vital dilatation.

Dr Douglas MacLagan said, that the bark was well known to him. From the specimens before the Society, he doubted whether the substance, in the

form of uterine bougies or tents, could be obtained of sufficient strength to admit of forcible insertion into a tight stricture.

Dr Storer stated that the bark expanded to five or six times its size. He was still engaged in experimenting on the subject.

The *President* thanked *Dr Storer*, in the name of the Society, for his very interesting communication, and expressed his opinion that the introduction into common use of a mild demulcent, which would sit light upon the stomach, would be attended with great benefit in the treatment of disease; and such a desideratum he hoped might be supplied by the bark of the slippery elm.

VARIETIES.

DEATH OF DR MARTIN BARRY.—It is with regret that we make the above announcement, which has taken the scientific world by surprise. *Dr Barry* died at Beccles, in the house of his brother-in-law, on the 27th April. In our next number we hope to present our readers with a short account of his life and labours.

ASSISTANT-SURGEONS IN THE ARMY.—The Royal College of Surgeons of Edinburgh, at a meeting on May 16, unanimously adopted the following resolution, and directed a copy of it to be transmitted to Lord Panmure:—"That the appointment by Government to the local rank of assistant-surgeon in the army, of persons not possessing a surgical diploma, or any other public testimonial of fitness for the office, and who have merely passed through the rudiments of medical education, is calculated to prevent the individuals so employed from acquiring a proper knowledge of their profession, to compromise the interest of the sick and wounded committed to their charge, and to lower the character of the medical department of the army."

SIR WILLIAM BURNETT.—By a Treasury minute, dated April 3d, 1855, the late distinguished Director-General of the navy has been awarded a retiring allowance of £1000 per annum. Special reference is made to his thirty-three years of distinguished services on sea and land, and to his presence at the battles of St Vincent, the Nile, and Trafalgar.

MILITARY TRAINING OF YOUTH.—A small pamphlet has been published on this subject by *H. Lawson, Esq., F.R.S.*, in which, urging the expediency of military training, he says:—"The absolute necessity for early training, and the result of its neglect, is strikingly displayed in the authentic tables published in the years 1833, and onwards to 1837 inclusively, the recruiting parties who visited the cities of Edinburgh, Dublin, London, Manchester, and Liverpool, refused one half of the men who offered themselves, as being incapable of bearing arms! and this lamentable circumstance appeared to arise in many, or most, instances, from diseases engendered in their youth, particularly pulmonary complaints, and the ill condition of their limbs, from neglect, and the want of mental alacrity in obeying command." *Mr Lawson's* suggestions are as follow: "Schoolmasters should have no choice in this important branch of discipline. The details may, indeed, be left to schoolmasters, but there should be no impediment to their insuring to all their pupils a regular course of military training. If the scholars be the children of poor parents, and the schoolmasters cannot tax them with the extra charge for paying a drilling-master, means must be provided to relieve those poor scholars from the necessity of contributing towards the expense. In almost every town and village throughout the land there are old soldiers, many, if not all of them, enjoying pensions; these invalided soldiers are very generally employed by the masters of such establishments as can afford to remunerate them. I venture to suggest, that those pensioners should be suitably recompensed, and have regular appointments from government to attend schools, and, under the direction of the schoolmasters and tutors, insist upon every scholar being taught manual exercise."

THE NEWEST IRISH AGITATION.—Our excellent and talented contemporary, the *Dublin Medical Press*, exhibits considerable soreness at the present "Scotch" ascendancy. No less than three paragraphs are devoted to the subject in a recent number. One appears as a simple announcement, the other two under the apparently harmless headings of "Sanitary Science—Dr Gray," and "The Microscope." We append them for the curiosity of our readers, vouching, however, neither for the accuracy of the statements nor for the strength of the jokes :—

"The *Edinburgh Monthly* makes its appearance again, under its new editor. Amongst several matters of interest, it gives the result of the Turkish Contingent agitation in the northern Athens, under Professor Simpson, and the large salaries previously stated in the *Press*. We find *Punch* lately asking some ethnological questions of a puzzling nature—'Why are all bricklayers' labourers, at least in London, Irishmen? Why are all milkmaids Welsh girls? and Why are all army surgeons inevitably Scotchmen?' Yes, indeed, why?

"*Sanitary Science*.—Dr Gray has again been explaining his excellent mode of sanitary irrigation in cities. We fear, however, our friend has no means of applying it practically during the war. What if his name were M'Taggert, Forbes, or Invercauld—something very strongly Scotch—then we might promise the editor of the *Freeman* two guineas a day.

"*The Microscope*.—In an extract of a letter from a London correspondent, published last week under this head; referring to Mr Quekett's lectures, Malpighian *naevus* was printed in mistake for Malpighian *plexus*. Our correspondent writes—'Why does your typographer owe one such a grudge? He should be made at once a F.R.C.S.E.; we said Malpighian *plexus*, not *naevus*, as shown by Quekett.'

UNIVERSITY PUFFING IN AMERICA.—*The New York Medical College* [Dr Green's] held its commencement on the 2d March. Degrees were conferred upon twenty-eight graduates and four Doctors of Medicine. We have not met with any account of the proceedings. We trust that at the commencement of the winter course, Dr H. Green may be able to state with more *truth* than on the last occasion (Oct. 18th), that in proof of the high qualifications of their graduates, "*an unusually large number have been able to obtain positions in our public institutions, the army and the navy*," etc. [see "Daily Times," Oct. 19]. The returns from the medical boards of the army and navy up to that period show that *but one graduate* (Dr Bernard J. D. Irwin, of N. Y.), has ever been received from the New York Medical College into the ARMY LIST, and that *not one graduate* from that College has been received into the NAVY LIST!—*New York Medical Times*. [The italics and small capitals are not ours.—Ed. M. J.]

REBUKE OF AN ANTI-MALTHUSIAN UNIVERSITY ORATOR.—The oration, pronounced by Professor J. M. Smith, at the opening of the Crosby Street University, was full of sound wisdom and instruction, truly parental in its character. Among other good advice, he recommended early matrimony, without any reservation. We beg leave to dissent from the advice of our learned friend on this subject; we would, from our own experience and observation, say to a young graduate, "Get married as early as you have any reasonable prospect of ability to support a wife. If your circumstances are moderate, unless you can persuade an heiress to marry you, beware; better remain without consolation for a few years, than that yourself and wife should die of starvation." Our friend will excuse this criticism.—*New York Medical Times*.

THE NINETEENTH CENTURY.—Chloroform, according to Dr Arnott, "has been expelled from the wards of the Massachusetts Hospital in America, by orders of the governors of that institution; and the exhibition of anæsthetic vapours in operations, has been regulated by public authority in some parts of the Continent."

NEGRO HOSPITAL.—Drs J. J. Chisholm and F. Peyre Porcher, have opened a hospital for the reception of sick negroes, where all diseases except those of a contagious nature will be treated. Such an establishment will prove very convenient for those owners of negroes who are not provided with suitable accommodations for them when sick, and for those planters in the vicinity of the city who have negroes requiring surgical operations. We feel confident of the success of this undertaking from the known ability and experience of these gentlemen. With Dr Porcher our readers are well acquainted. Dr Chisholm is the lecturer on surgery in the Charleston Preparatory Medical School, and has devoted several years, at home and abroad, to the special study of surgical diseases.—*Charleston Medical Journal*.

FOREIGN APPOINTMENTS.—Professor Heyfelder of Erlangen has been nominated surgeon-in-chief of the Russian army in Finland. He has already set out for Sveaborg, and received, according to Russian custom, the rank of colonel in the army.—*Gazette Hebdomadaire*. April 27.

Dr Bernard Seyfert has been recently appointed Professor of Midwifery in the Faculty of Prague. Dr J. Sterig has, at the same time been elected Professor of the Art to the *Sages-femmes*.—*Wiener Wochenschrift*.

The College of France determined to appoint two Professors to the Chair of Natural History, founded by Cuvier and at present vacant by the recent death of M. Duvernoy. M. Flourens, we are glad to see, received the first appointment, and M. Valenciennes the second. M. Quatrefages on both trials had a minority.—*Gazette des Hôpitaux*. May 5.

At the Séance of the Academy of Sciences, April 23d, Dr Bonnet of Lyons was elected, by 39 votes out of 52 members voting, a corresponding member in the sections of medicine and surgery, in the room of Orfila.—*Gazette Hebdomadaire*. April 27.

MM. Guérard, Devergie, Tardieu, Bondin, Bouchut, and Brierre de Boismont were candidates for a vacancy in the Academy of Medicine in the section of hygiene and legal medicine. The final vote stood as follows :—Guérard, 39 ; Devergie, 23 ; Tardieu, 10 ; Bondin, 2, and Brierre de Boismont, 1. M. Guérard took his seat accordingly.—*L'Union Médicale*. May 17.

By imperial decree dated April 28th, M. Lauton, surgeon of the second class in the French Marine, has been named Chevalier of the Legion of Honour, for his courageous conduct before Sebastopol.—*Gazette des Hôpitaux*. May 5.

THE LATE PROFESSOR LALLEMAND.—The Academy of Sciences has been authorised to accept the legacy of 50,000 francs bequeathed in the will of the late M. Lallemand. It will be devoted to founding a prize for the best series of observations on the nervous system.

M. BERNARD, Member of the Institute, and at present supplying the place of Majendie in the College of France, has commenced his Course of Physiology for the summer, the subject being, "The Functions of the Glands."

A WORTHY PRELATE.—The Bishop of Montpellier having generously offered to the French Government his magnificent country residence as a convalescent hospital for invalids from the Crimea, the administration have availed themselves of his munificence, and arranged this beautiful mansion for the accommodation of 1600 patients.—*L'Union Médicale*, April 28, 1855.

A PROSECUTION UNDER THE FRENCH REGISTRATION ACT.—A charge has been brought against a Dr Bessems of Antwerp, for having refused to insert the name of the mother in a declaration of a birth, the mother having, as he declared, bound him over to secrecy. The law authorities, on consultation, had to dismiss the case, as the article of the code in question was not sufficiently precise, and the 378th article of the penal code expressly forbids the prisoner to reveal any fact which the law does not compel him to confess.—*Gaz. des Hôp.*, May 5.

PARIS EXHIBITION.—Honorary Jurors for Medical Section, Sir Joseph Oliffe, M.D., Professor Royle, and Mr Chadwick.

THE VALUE OF ACADEMIC REPORTS.—The Secretary of the Academy of Medicine had the other day a very delicate duty to discharge, and to his honour be it said, he did not hesitate to speak his mind freely, and without respect of persons. A Report by M. Bousquet, on a work by M. Moreau (de Tours), was being read, when it was ascertained, that of three commissaries who were charged with the examination of the work in question, two declared that they had signed the Report without having read it, and one made the humiliating confession, that he had neither perused the Report nor the work itself; the contents and title were alike unknown to him. The astonishment of the Academy was, if possible, increased, by the subsequent discovery, that the two members who expressed in writing their approval of M. Bousquet's Report had inscribed their names in the list of debaters to oppose it. M. Dubois d'Amiens characterised, in severe terms, this gross infraction of all academic proprieties, usages, and rules. Commissaries, to sign a Report one minute, and be the first to impugn it the next! This certainly was very irregular, and very singular. Of what value was the decision of their commissions to be for the future? What became of the authority of the Academy itself in the midst of the clashing and jarring among those whose only mission it was to agree upon and indicate their report to the Academy?

INCREASE OF INSANITY IN THE DEPARTMENT OF THE SEINE.—It appears from the researches of the Director de l'Assistance Publique, that the number of the insane in the department has in 10 years increased by 1073 individuals, or 107 yearly. If we go back to former years the increase becomes still more striking. Thus, in 1801, the number of insane on the 1st of January of that year was 946; it rose by the end of 1820 to 2392; to 2445 in 1838; to 2602 in 1844; and to 4189 on the 31st of December 1853. When we attempt to determine the causes of this extraordinary increase, so out of proportion with that of the general population, we soon perceive their diversity. First, the legislature itself has contributed to it by increasing the number of admissions, by adding idiots and imbeciles to the number of the insane, and by admitting children at all ages. On the other hand we must keep in mind the law of sequestration now applied to dangerous lunatics, and the facilities given for the private treatment of this malady; lastly, the lowering of the mortality, as a direct consequence of the increased care and attentive treatment of the insane, contributes to the increase in the number of the survivors.—*Annales Medico-Psychologiques*.

VACCINATION—FREE-TRADE ECONOMY.—Vaccination in England is now done so badly, from free-trade views of the cheapest estimate, that Mr Henley lately stated in parliament, that in Oxford, with 30,000 inhabitants (about one-third the size of Cork or Belfast), those who have been attacked within a twelvemonth could be counted by thousands. Dr Brady knew loathsome diseases imbibed from the same source.

ROYAL INFIRMARY.—Dr W. Begbie has been appointed a Physician in room of Dr W. Robertson, resigned.

PUBLICATIONS RECEIVED.

Mann's Guide to the Knowledge of Life.
Headland on the Action of Medicines, 2d ed.
Reynold on Diagnosis of Diseases of the Brain, Spinal Cord, etc.
Mayo on Medical Evidence in Lunacy Cases.
Report of Trial, Glover and Syme.
Waring's Statistics and Pathology of Abscess in the Liver.

Beaseley's Pocket Formulary, 6th edition.
Dixon's Guide to the Diseases of the Eye.
Micrographic Dictionary, Parts 9 and 10.
Jones' Ophthalmic Medicine, 2d edition.
Fresenius' Qualitative Analysis, 4th edition.
Carter on Nervous Affections.
Bouchut on the Diseases of Children.

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